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Building Security in Africa

Technical Appendixes

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APPENDIX A

Summary of U.S. Security Sector Assistance Programs

Table A.1
U.S. Security Sector Assistance Programs, Funding, and Initiatives in Africa

Program	Description
Equipment and related training	
Foreign Military Financing (FMF)	Provides partner nations with U.S. military equipment, services, and training. The State Department funds and oversees the program, while DoD is responsible for implementation. The FMF program was created in 1982, when it replaced the functionally equivalent Military Assistance Program, which had begun in 1950.[a]
Excess Defense Articles	Provides partner nations with equipment that the U.S. military no longer needs, at either reduced or no cost. DoD oversees and executes this program, which was first established in 1950.[b]
Global Train-and-Equip Authority ("Section 2282")	Provides partner-nation security forces involved in counterterrorism or stability operations with assistance in the form of "equipment, supplies, defense service, training, and small-scale military construction." DoD funds, oversees, and executes this program, with State Department concurrence. The program was first established in 2006.[c]
Security and Stabilization Assistance ("Section 1207")	Provided training, equipment, and institution-building support to partner nations in support of stabilization and internal security. Recipients included partner-nation military, police, judicial, and civil-society institutions in the DRC, Kenya, Liberia, Somalia, and Uganda. DoD funded the program, and the State Department administered it. It was established in 2006 and expired in 2010.[d]

Table A.1—Continued

Program	Description
Global Security Contingency Fund (GSCF)[e]	Provides training and equipment to partner-nation military and other security forces engaged in internal defense, counterterrorism, peacekeeping, and border security. The program is unique in that it is jointly overseen and administered by both DoD and the State Department. GSCF was established in 2012 and was expanded to Africa in 2014. Cameroon, Chad, Niger, Nigeria, Rwanda, Tanzania, and Uganda have all received GSCF assistance.[f]
Support of Special Operations to Combat Terrorism ("Section 1208")	Provides equipment, transportation, and training to foreign forces engaged in supporting U.S. SOF in counterterrorism operations. DoD funds, oversees, and executes this program.[g]
Exercises and training events	
Bilateral and regional exercises	Build partner capacity and improve coordination in counterterrorism, peacekeeping, maritime, and other operations. DoD funds and executes bilateral and regional military exercises in Africa, often on an annual basis.[h] These exercises are supported by a complex mix of DoD funds and authorities, which can sometimes make planning and coordination difficult.[i]
Joint Combined Exchange Training (JCET) events	Build tactical proficiency at the small-unit level. These U.S. SOF training events involving partner military forces, generally focused on building tactical proficiency at the small-unit level. Although the primary purpose of these events is to improve U.S. SOF's ability to engage and train foreign troops, the partner forces involved receive "incidental" training benefits.[j]
State Partnership Program	Provides recurring personnel exchanges and training events between U.S. states' national guards and the security forces of foreign partner states. Activities often focus on disaster and emergency response. The State Partnership Program first expanded to Africa in 2003; there are now 12 such partnerships between U.S. states and African nations.[k]

Table A.1—Continued

Program	Description
Professional military education and training	
International Military Education and Training (IMET)	Funds foreign military personnel's attendance at U.S. military education institutions and training courses. IMET was established in 1979, although it was preceded by similar training grants provided under the Military Assistance Program created in 1949. The E-IMET program was established in 1991 to focus on strengthening accountability systems and fostering respect for human rights, the rule of law, and civilian control of the military.[l] DoD funds and implements the program, while the State Department selects recipient countries.[m]
Combating Terrorism Fellowship Program (CTFP)	Funds foreign military personnel's attendance in U.S. education programs, training courses, and conferences focused on counterterrorism. DoD funds, oversees, and implements the program, which was established in 2002.[n]
Africa Center for Strategic Studies	Provides a forum for research, education, training, and conferences on African security issues for both U.S. and African partner-nation personnel. DoD funds and manages the center, which was established in 1999.[o]
DIB	
Defense Institutional Reform Initiative (DIRI)	"Provides subject-matter experts to work with partner nations to assess organizational weakness and share best practices for addressing those shortfalls in support of DIB." DoD funds and implements the program, which began in 2010.[p]
Ministry of Defense Advisors (MoDA)	Embeds DoD civilian advisers with partner-nation ministries of defense to support development of effective and accountable defense institutions.[q] DoD funds and implements the program. The MoDA program expanded to Africa in 2016 with adviser deployments to Botswana.[r]
Security Governance Initiative (SGI)	Supports "management, oversight, accountability, and sustainability" of partner security institutions at the national level. Partners currently include Ghana, Kenya, Mali, Niger, Nigeria, and Tunisia. The State Department and DoD jointly fund and manage this interagency program, which began in 2015.[s]

Table A.1—Continued

Program	Description
Regional programs	
Trans-Sahara Counterterrorism Partnership (TSCTP)	Provides equipment, training, and institutional support to partner military, police, border security, and justice-sector actors in northwest Africa. TSCTP also provides economic development and governance assistance designed to support counterterrorism efforts. It is jointly funded, overseen, and implemented by the State Department, USAID, and DoD. This program was established in 2005 and grew out of the 2002–2004 Pan Sahel Initiative. TSCTP partners are Algeria, Burkina Faso, Cameroon, Chad, Mali, Mauritania, Morocco, Niger, Nigeria, Senegal, and Tunisia.[t]
Partnership for Regional East Africa Counterterrorism (PREACT)	Provides equipment, training, and institutional support to partner military, police, border security, and justice-sector actors in east Africa. PREACT also provides economic development and governance assistance designed to support counterterrorism efforts. The State Department funds PREACT and, together with DoD and USAID, implements program activities. PREACT was established in 2009 and includes 12 partner states, with most program assistance going to Kenya, Ethiopia, Djibouti, Uganda, Tanzania, Somalia, and Burundi.[u]
Other State Department programs and funds	
PKOs	Supports a variety of equipping, training, education, and DIB activities that target both PKO troop contributors and host nations, as well as counterterrorism and crisis response efforts more broadly. SSA funding account has "been the primary vehicle" for U.S. support to UN and AU peacekeeping missions in Somalia, Mali, Sudan, and CAR. The PKO account also funds the related SGI, PREACT, and TSCTP programs, as well as the Africa Military Education Program implemented by the Africa Center for Strategic Studies.[v]
Nonproliferation, Anti-Terrorism, Demining, and Related Programs account	Provides equipment, training, and institutional support to partner military, police, border, and other security forces, primarily for counterterrorism purposes. This funding account supports PREACT and TSCTP, among other SSA efforts in Africa.[w]

Table A.1—Continued

Program	Description
International Narcotics Control and Law Enforcement	Provides equipment, training, and institutional support to partner police, border security, and justice-sector actors, including for strengthening security force accountability and building capacity for counternarcotics and counterterrorism purposes. This funding account supports PREACT and TSCTP, among other SSA efforts in Africa.[w]
Other DoD programs and funds	
Drug Interdiction and Counter-Drug Activities, Defense	Provides counternarcotics-related equipment and training to partner-nation military and police forces. DoD funds and implements this program. This program was established in 1991.[x]
Cooperative Threat Reduction Account, Defense	Provides equipment, training, and infrastructure support to partner security forces and health system personnel to build their capacity to address threats from weapons of mass destruction. This program expanded to Africa in 2010 to support partners in addressing potential biological threats. DoD funds and implements this program, with assistance from U.S. health agencies.[y]
Counterterrorism Partnerships Fund (CTPF)	Provides additional funding for SSA programs focused on counterterrorism and crisis response in Africa. Although it was intended as an interagency funding source, most of the funds have been appropriated to DoD so far and have been used primarily to pay for provision of equipment and related training under the Section 2282 program. CTPF was established in 2014.[z]
Other military assistance	Serves as an accounting category for DoD SSA funds used in the annual U.S. Overseas Loans and Grants reports on foreign assistance. This category includes funding for Excess Defense Articles, the Military Assistance Program that preceded FMF, and general DoD O&M funds used for SSA.

NOTE: Several DoD programs discussed, including Sections 2282 and 1207, were consolidated and reorganized by the NDAA for fiscal year (FY) 2017. Because this report evaluates the effects of U.S. SSA from 1945 to 2015, it refers to these programs using their historical rather than post-2016 labels. E-IMET = Expanded IMET.

[a] DSCA, "SAMM Chapters," undated (b), § C9.7.2.

[b] DSCA, undated (b), § C.11.3.1.

[c] DSCA, undated (b), § C15.1.4.5.

Table A.1—Continued

[d] Nina M. Serafino, *Department of Defense "Section 1207" Security and Stabilization Assistance: Background and Congressional Concerns, FY2006–FY2010*, Washington, D.C.: Congressional Research Service, RS22871, March 3, 2011.

[e] Although this fund was authorized by Section 1207 of the NDAA for FY 2012 (Pub. L. 112-81, December 31, 2011), it is entirely separate from the earlier Section 1207 Security and Stabilization Assistance program, which expired in 2010. In addition to GSCF, Section 1207 of the 2012 NDAA also authorized a separate temporary SSA program to support counterterrorism in Somalia.

[f] U.S. Department of State, "Global Security Contingency Fund (GSCF)," undated (a); Blanchard, 2015, p. 11.

[g] DSCA, *Security Cooperation Programs, Fiscal Year 2016*, Washington, D.C., revision 16, c. 2016, p. 64.

[h] AFRICOM, "Exercises," undated (a).

[i] Bolko J. Skorupski and Nina M. Serafino, *DoD Security Cooperation: An Overview of Authorities and Issues*, Washington, D.C.: Congressional Research Service, R44602, August 23, 2016, pp. 6, 11.

[j] DSCA, undated (b), § C.10.17.11.

[k] Lisa Vines, "AFRICOM and National Guard Leaders Meet to Discuss State Partnership Program," U.S. Africa Command, February 2, 2016; DSCA, 2016, pp. 161, 172–173.

[l] Richard F. Grimmett, *International Military Education and Training Program*, Washington, D.C.: Congressional Research Service, RS20506, October 28, 2004; DSCA, undated (b), § C.10.6.3.

[m] DSCA, "International Military Education and Training (IMET)," undated (a).

[n] Office of the Assistant Secretary of Defense for Special Operations and Low Intensity Conflict, *Regional Defense Combating Terrorism Fellowship Program: Report to Congress, Fiscal Year 2015*, Washington, D.C., c. 2015.

[o] Africa Center for Strategic Studies, "History," undated.

[p] DSCA, 2016, p. 166.

[q] DSCA, 2016, p. 165.

[r] Office of the Under Secretary of Defense (Comptroller), *Fiscal Year (FY) 2018 President's Budget: Justification for FY 2018 Operation and Maintenance, Defense-Wide*, Washington, D.C.: U.S. Department of Defense, May 2017, p. 443.

[s] U.S. Department of State, Bureau of African Affairs, *Security Governance Initiative: 2015 Review*, Washington, D.C., March 2, 2016a, p. 1.

[t] U.S. Government Accountability Office, *Combating Terrorism: U.S. Efforts in Northwest Africa Would Be Strengthened by Enhanced Program Management*, Washington, D.C., GAO-14-518, June 24, 2014b.

[u] U.S. Government Accountability Office, *Combating Terrorism: State Department Can Improve Management of East Africa Program*, Washington, D.C., GAO-14-502, June 17, 2014a.

Table A.1—Continued

[v] Lauren Ploch Blanchard, *Statement of Lauren Ploch Blanchard, Specialist in African Affairs, Congressional Research Service, Before the Senate Foreign Relations Committee Subcommittee on Africa and Global Health, Hearing: U.S. Security Assistance in Africa*, Washington, D.C.: Congressional Research Service, June 4, 2015, pp. 3, 7–9.

[w] Blanchard, 2015, p. 14.

[x] DoD and U.S. Department of State, *Foreign Military Training: Fiscal Years 2014 and 2015—Joint Report to Congress: Volume I*, Washington, D.C., c. 2015, p. II-4.

[y] Cooperative Biological Engagement Program, Cooperative Threat Reduction Program, U.S. Department of Defense, *FY 2015 Annual Accomplishments*, Washington, D.C., September 16, 2016, p. 13.

[z] Nina M. Serafino, *Security Assistance and Cooperation: Shared Responsibility of the Departments of State and Defense*, Washington, D.C.: Congressional Research Service, R44444, April 4, 2016, p. 14.

APPENDIX B

Model Specifications, Data, and Technical Discussion of Findings

This appendix accompanies Chapters Four and Five, which discuss our research design and main empirical results. We derived these results from a series of statistical regression models that use a variety of data and estimation strategies. We begin by describing our data sources and variable specification choices. How we scale SSA and our outcome measures are consequential choices that merit serious discussion. We then move on to our research design. To understand the myriad effects of SSA, our study explored several distinct outcome measures, which have important implications for modeling approach and estimation strategy. After this section, we summarize our results, beginning with the first-stage models on the selection of SSA recipients. Drawing on these first-stage models, we estimate propensity weights that we incorporated in the remaining models on the effects of SSA. The final three sections then proceed to outline these effects on civil-war onset, terrorist attacks, and regime repression, respectively. Each outcome measure receives its own discussion. These sections review the main results in greater depth than we provided in Chapter Five and assess potential threats to inference that would suggest caution in the interpretation of those results.

Data Sources and Variable Specification

Measuring Intrastate Conflict

Our quantitative models assess the effects of SSA to African states along three main measures of intrastate conflict: (1) civil-war onset,

(2) terrorist attacks, and (3) levels of state repression. Taken together, these three measures provide us with a broad perspective on how levels of SSA affect multiple forms of conflict within African states. Because SSA can have unintended consequences, it is important to look at not only indicators of the United States' primary objectives (reducing the frequency of civil wars and terrorist attacks) but also the ways in which U.S. assistance might prove harmful (increasing repression).

To measure civil-war onset, we used a dichotomous indicator that records the start of a period of civil war in a given year in each African state, drawn from version 17.1 of the UCDP/PRIO Armed Conflict Dataset.[1] We coded this measure as 1 in years in which a new period of civil war broke out in a given country and 0 for years in which the state was at peace.[2] In line with the standard data set's conflict coding, this indicator utilizes a threshold of 25 battle-related deaths for defining the start of a conflict period. In subsequent analyses, we also constructed an additional dichotomous indicator that utilizes a threshold of 1,000 battle-related deaths. To create these measures, we first collapsed all individual conflict years into periods of civil war for each state. When a state experienced multiple overlapping civil wars, we recorded the period of conflict as starting with the start of the first conflict and continuing until the last civil war ended. We also used a two-year gap rule to demarcate between separate periods of conflict. That is, two years of peace must transpire for a conflict spell to be measured as a new period of conflict rather than as a continuation of a previous conflict spell.

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[1] Nils Petter Gleditsch, Peter Wallensteen, Mikael Eriksson, Margareta Sollenberg, and Håvard Strand, "Armed Conflict 1946–2001: A New Dataset," *Journal of Peace Research*, Vol. 39, No. 5, 2002, pp. 615–637; Marie Allansson, Erik Melander, and Lotta Themnér, "Organized Violence, 1989–2016," *Journal of Peace Research*, Vol. 54, No. 4, 2017, pp. 574–587.

[2] We dropped years of ongoing conflicts from our analyses. Overall, out of 2,425 country-years, not including years of ongoing civil conflict, there were 1,932 country-years of peace and 76 country-years in which periods of civil war broke out. Similarly, out of 2,425 country-years, there were 41 country-years in which periods of high-intensity civil war, measured as causing 1,000 battle-related deaths, broke out.

To measure terrorist activity, we recorded the yearly number of terrorist attacks in each African state, drawn from the GTD.[3] This measure records and aggregates all terrorist attacks within each state.[4] Our main statistical models do not distinguish between domestic and transnational terrorism, in part because the boundaries between these two types of violence are often indistinct and in part because SSA might be expected to either deter or disrupt and degrade all nonstate violent actors.[5] However, we also present alternative models that separate terrorist activity into domestic and transnational terrorism, drawn from the GTD. As a second measure of terrorist activity, we recorded the yearly number of casualties caused by terrorist attacks.

We operationalize levels of state repression using the CIRI Human Rights Dataset.[6] The CIRI data measure the prevalence of human-rights abuses by incumbent governments along four dimensions—torture, political imprisonment, extrajudicial killings, and forced disappearances—based on annual reports by Amnesty International and the U.S. State Department. The CIRI index aggregates these four dimensions to create a unidimensional scale that broadly measures the incumbent government's respect for the right to life, security of person, and bodily integrity. Normally, this scale ranges from 0, meaning that the government has absolutely no respect for its populace's human rights, to 8, meaning that the government fully respects all human rights of its citizens. To better align this measure with our specific

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[3] National Consortium for the Study of Terrorism and Responses to Terrorism, "Global Terrorism Database," last updated June 2017; Gary LaFree, Laura Dugan, and Erin Miller, *Putting Terrorism in Context: Lessons from the Global Terrorism Database*, Abingdon, Oxon: Routledge, 2015.

[4] The yearly number of terrorist attacks in our data ranges from zero to 862 attacks in a single year, with a mean number of yearly terrorist attacks of 7.70 attacks. The median number of terrorist attacks in our data, however, was zero.

[5] Walter Enders, Todd Sandler, and Khusrav Gaibulloev, "Domestic Versus Transnational Terrorism: Data, Decomposition, and Dynamics," *Journal of Peace Research*, Vol. 48, No. 3, 2011, pp. 319–337.

[6] David L. Cingranelli, David L. Richards, and K. Chad Clay, "The CIRI Human Rights Dataset," CIRI Human Rights Data Project, version 2014.04.14, April 4, 2014.

interest in state repression, we simply inverted this measure, such that the index increases as states' use of repression increases.

Measuring Security Sector Assistance and the Security Environment

Our main independent variable is the aggregate annual level of SSA, in dollars, provided to each African state each year between 1946 and 2015. Our main source for this information was the USAID Greenbook, which tracks the vast majority of all assistance provided to partner states.[7] We supplemented these data with information on several smaller and more-recent assistance programs designed to bolster the security sectors of partner states. Specifically, we included levels of Section 1206/2282, Section 1207, and CTFP assistance, each of which is meant to specifically assist the counterterrorism and stability operations of partner states in combating and deterring intrastate conflict.

Levels of U.S. SSA vary widely across states and over time; to account for this variation, we transformed the raw SSA data in several ways. First, we normalized the magnitude of SSA to account for the varying characteristics and dynamics across African states. In our models of civil war and terrorism, we normalized the magnitude of SSA by each state's population per 10,000 people. In our models of state repression, we normalized the magnitude of SSA by the number of personnel in each state's military.

Second, we utilized a three-year moving average of SSA to better account for fluctuations in annually reported levels of SSA and more accurately account for long-term trends in the relationship between SSA and intrastate conflict.

Third, because we expected that SSA would require some time to show effects on levels of intrastate conflict, we utilized a four-year lag measure, meaning that our quantitative models assess SSA's impact on intrastate conflict four years after aid was allocated.[8] Initially, we

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[7] USAID, *U.S. Overseas Loans and Grants: Obligations and Loan Authorizations, July 1, 1945–September 30, 2016*, CONG-R-0105, c. 2016.

[8] In conjunction with the three-year moving average, this lag means that, in our models, the risk of civil-war onset in year x, for instance, is affected by the average of levels of SSA in years $x - 4$, $x - 5$, and $x - 6$.

chose a four-year lag to ensure that the provision of SSA fell outside the window of our three-year moving average. As a robustness check, we replicated this analysis using various other lags. Generally, we find that the effects of SSA cannot be seen in the short term (e.g., years 2 and 3); nor do they reliably persist a decade later. However, we do find stable results in the medium term (e.g., years 4 and 5). As we continue to further lag SSA (e.g., years 6 and 7), most results hold up, but the significance levels begin to attenuate.[9]

Finally, like with most foreign aid data, there is significant cross-national variation in the magnitude of SSA funding provided to African states, with many states receiving zero assistance and some high-risk states receiving especially large sums. This large variation is problematic for statistical analyses. To account for this dispersion while accounting for states receiving zero SSA funding, we utilized the inverse hyperbolic sine transformation to further normalize the magnitude of SSA across states.[10]

In addition to any direct effects of SSA, there is reason to believe that the security environment of the state and the way in which SSA is implemented will significantly affect SSA's success in mitigating intrastate conflict. Active PKOs by third-party actors should, in particular, influence SSA's effects for at least two reasons. First, active PKOs are mechanisms of deep and repeated interactions between assisting states and their regional partners. These repeated interactions should promote greater oversight of how partner governments use SSA and promote greater cooperation between states in implementing SSA pro-

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[9] Scholars should further explore these temporal dynamics in future work. To the extent that these findings prove robust across other studies of SSA, they raise important questions surrounding the time it takes for SSA to have an observable impact and how long these effects persist.

[10] John G. Burbidge, Lonnie Magee, and A. Leslie Robb, "Alternative Transformations to Handle Extreme Values of the Dependent Variable," *Journal of the American Statistical Association*, Vol. 83, No. 401, March 1988, pp. 123–127; Michael J. McNerney, Angela O'Mahony, Thomas S. Szayna, Derek Eaton, Caroline Baxter, Colin P. Clarke, Emma Cutrufello, Michael McGee, Heather Peterson, Leslie Adrienne Payne, and Calin Trenkov-Wermuth, *Assessing Security Cooperation as a Preventive Tool*, Santa Monica, Calif.: RAND Corporation, RR-350-A, 2014.

grams, both of which should increase SSA's effectiveness in achieving its goals.

Additionally, active PKOs provide a physical buffer against flare-ups in violence between warring parties in the state. Because we expect SSA to require sufficient time to yield positive change in the local security environment, the maintenance of continued peace by uniformed personnel is likely valuable in providing a window for SSA to work in partner states by creating more-stable conditions in the state. Further, both mechanisms should be strongly influenced by unbiased peacekeepers whose main objective is the provision of peace, rather than seeking victory for one side in a conflict. In line with these expectations, we also specified quantitative models that interact levels of SSA with a dichotomous indicator of whether there was an ongoing UN PKO at the time of SSA provision.

Controlling for Additional Factors Affecting Security Sector Assistance Funding and Intrastate Conflict

To better isolate SSA's effects on levels of intrastate conflict, we included in our quantitative models several additional variables associated with violent conflict. Across our quantitative models, we selected control variables that have been shown in the broader security studies literature to be robustly associated with our four measures of intrastate conflict. Because our control variables partially overlap across our different quantitative models, Table B.1 provides a quick reference for which control variables are included in which set of models.[11] Table B.2 provides summary statistics for the control variables included in our quantitative models.

[11] In addition to the variables described here, we also include in all models indicators for the Cold War period, the post-9/11 period, and an indicator for Francophone Africa. Following convention, our models of civil-war onset and coup attempts include the number of years since the previous civil war or coup attempt, respectively, along with cubic splines. Our models of terrorist attacks and state repression include the previous year's level of terrorist activity and state repression, respectively. See Nathaniel Beck, Jonathan N. Katz, and Richard Tucker, "Taking Time Seriously: Time-Series–Cross-Section Analysis with a Binary Dependent Variable," *American Journal of Political Science*, Vol. 42, No. 4, October 1998, pp. 1260–1288.

Table B.1
Control Variables to Isolate the Effects That Security Sector Assistance Has on Intrastate Conflict

Variable	Civil War	Terrorist Attacks	State Repression
Percentage of GDP from OECD ODA	x	x	x
GDP per capita	x	x	x
Polity level	x	x	
Military regime			x
Personalist regime			x
Single-party regime			x
Democracy			x
Percentage of GDP from natural-resource rents	x		
Military spending per service member	x	x	
Arms transfers from Russia or China	x		
Excluded-population size	x	x	
Youth bulge	x	x	
State bureaucratic capacity		x	
Number of parallel military institutions			
Neighborhood civil war	x		
Ongoing civil war		x	x
Previous civil war	x		
Previous coup attempt	x		

First, to better isolate the effects of SSA over those of other forms of economic assistance, we included in all of our models a measure of

Table B.2
Summary Statistics of Variables Included in Quantitative Models

Variable	*N*	Minimum	Maximum	Mean	Standard Deviation
Civil-war onset (1 = yes)	2,425	0	1	0.042	0.200
High-intensity civil-war onset (1 = yes)[a]	2,425	0	1	0.015	0.122
Yearly terrorist attacks	2,312	0	862	7.70	44.23
Level of state repression on the CIRI scale	1,410	−8	0	−4.30	2.77
Coup attempt (1 = yes)	2,718	0	1	0.066	0.248
Total SSA per population, in dollars	2,678	0	610,039.2	7,105.38	26,579.62
Ongoing UN PKO (1 = yes)	2,735	0	1	0.05	0.21
Percentage of GDP from ODA	2,611	0	100	9.45	10.46
GDP per capita, in dollars	2,709	206	25,306.44	1,760.03	2,227.30
Percentage of GDP from natural-resource rents	1,995	0.001	86.13	12.51	14.21
Polity level	2,585	0	20	7.60	5.91
Polity level, squared	2,585	0	400	92.69	116.02
Military regime (1 = yes)	2,209	0	1	0.08	0.27
Personalist regime (1 = yes)	2,209	0	1	0.29	0.45
Single-party regime (1 = yes)	2,209	0	1	0.40	0.49
Democracy (1 = yes)	2,682	0	1	0.15	0.36
Youth bulge (1 = yes)	2,456	0	1	0.76	0.42
Excluded population as a percentage of total population	2,425	0	98	0.27	0.32
State bureaucratic capacity on the Relative Political Capacity Index	2,417	0.10	3.75	1.02	0.52

Table B.2—Continued

Variable	*N*	Minimum	Maximum	Mean	Standard Deviation
Military spending per service member, in dollars	2,216	0	94,000	7,380.99	9,894.71
Arms transfers from Russia or China	2,735	0	3,814	26.10	172.21
Number of parallel military institutions	1,918	1	4.19	1.59	0.62
Neighborhood civil war (weighted-average spatial lag)	2,647	0	0.99	0.23	0.26
Ongoing civil war (1 = yes)	2,425	0	1	0.20	0.40
Previous civil war (1 = yes)	2,425	0	1	0.50	0.50
Previous coup attempt (1 = yes)	2,735	0	1	0.67	0.47
Cold War (1 = yes)	2,735	0	1	0.51	0.50
Francophone Africa (1 = yes)	2,735	0	1	0.49	0.50

[a] *High intensity* means more than 1,000 battle deaths.

the level of ODA from OECD states, scaled by states' GDP.[12] Similarly, because poorer states have been robustly associated with several indicators of political violence, we included across all of our models a measure of states' GDP per capita.[13] Economic deprivation can foster destitute populations' grievances against incumbent governments, making poorer states possibly more likely to face a higher risk of civil war and terrorist activity. Additionally, wealthier governments often possess more-capable institutions and have greater resources to deter internal

[12] OECD, "Net ODA," undated.

[13] Jutta Bolt and Jan Luiten van Zanden, "The Maddison Project: Collaborative Research on Historical National Accounts," *Economic History Review*, Vol. 67, No. 3, August 2014, pp. 627–651; World Bank, *World Development Indicators 2012*, Washington, D.C., 2012.

challengers to their rule, which serves the dual purpose of deterring civil war and terrorist attacks while also restraining incumbent governments' need to utilize violent repression. In addition to GDP per capita, we controlled for the percentage of GDP made up of income from the mining of natural resources, which has been shown to increase states' risk of civil war.[14]

The political structure of the state is another important determinant of intrastate conflict. Therefore, we included an indicator of the level of democracy or autocracy in the state.[15] Broadly representative and inclusive institutions provide peaceful outlets for political, economic, and social grievances, which help circumvent violent conflict. In contrast, particularly nondemocratic regimes, often less secure in their control and with fewer options against domestic opposition, can utilize greater institutionalized repression to preemptively crush opposition before conflict.[16] In our models of civil-war onset and terrorist attacks, we included the linear and square terms of the Center for Systemic Peace's Polity IV measure, a 21-point scale that ranges from autocratic regimes on the low end to consolidated democracies on the high end. In our empirical analyses, we included both the linear and quadratic terms of the polity scale to capture the potential nonlinear relationship between democracy and conflict. To further account for variations among different regimes in our state repression and coup models, we included dichotomous indicators of whether the incumbent government was a military, personalist, single-party, or democratic regime.

We also included two measures that control for characteristics of a state's population. First, we included a dichotomous indicator of whether at least 45 percent of the population was between the ages of

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[14] World Bank, 2012. See, among others, Navin A. Bapat and Sean Zeigler, "Terrorism, Dynamic Commitment Problems, and Military Conflict," *American Journal of Political Science*, Vol. 60, No. 2, April 2016, pp. 337–351, and Päivi Lujala, "The Spoils of Nature: Armed Civil Conflict and Rebel Access to Natural Resources," *Journal of Peace Research*, Vol. 47, No. 1, 2010, pp. 15–28.

[15] Center for Systemic Peace, "Polity IV Annual Time-Series, 1800–2016," undated.

[16] Håvard Hegre, Tanja Ellingsen, Scott Gates, and Nils Petter Gleditsch, "Toward a Democratic Civil Peace? Democracy, Political Change, and Civil War, 1816–1992," *American Political Science Review*, Vol. 95, No. 1, March 2001, pp. 33–48.

15 and 45.[17] Recent studies on intrastate conflict have documented the relationship between increases in the size of a youth cohort and the onset of armed conflict and terrorist activity.[18] Boys and young men often make up the main fighting force of militant groups, and states with large youth populations have a larger supply of fighters, which reduces the cost of conflict and makes escalation cheaper.

States can also be at an increased risk of intrastate conflict if large portions of their populations, particularly among ethnic minorities, are excluded from the state political apparatus.[19] Political exclusion along ethnic lines can enflame grievances against incumbent regimes, and being excluded from political power often leaves minority groups few avenues, other than armed resistance, to redress these grievances. To account for these dynamics, we also included a measure for the share of the state's ethnic population that is systematically excluded from political power.[20]

We further used three control variables to capture the state's bureaucratic and military capabilities. First, to measure the bureaucratic capacity of the state, we used data measuring the state's ability to extract taxes from the populace, drawn from the Relative Political Capacity data set.[21] We expected that partner states with stronger bureaucracies would be able to more effectively implement and benefit from U.S. SSA.

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[17] World Bank, 2012.

[18] Henrik Urdal, "A Clash of Generations? Youth Bulges and Political Violence," *International Studies Quarterly*, Vol. 50, No. 3, September 2006, pp. 607–629.

[19] Halvard Buhaug, Lars-Erik Cederman, and Jan Ketil Rød, "Disaggregating Ethno-Nationalist Civil Wars: A Dyadic Test of Exclusion Theory," *International Organization*, Vol. 62, No. 3, Summer 2008, pp. 531–551; Lars-Erik Cederman, Andreas Wimmer, and Brian Min, "Why Do Ethnic Groups Rebel? New Data and Analysis," *World Politics*, Vol. 62, No. 1, January 2010, pp. 87–119; Andreas Wimmer, Lars-Erik Cederman, and Brian Min, "Ethnic Politics and Armed Conflict: A Configurational Analysis of a New Global Data Set," *American Sociological Review*, Vol. 74, No. 2, 2009, pp. 316–337.

[20] Cederman, Wimmer, and Min, 2010; Wimmer, Cederman, and Min, 2009.

[21] Jacek Kugler and Ronald L. Tammen, eds., *The Performance of Nations*, Lanham, Md.: Rowman and Littlefield, 2012.

Stronger and more-capable militaries should be able to deter potential rebel groups from taking up arms without resorting to violent repression. At the same time, however, a strong military apparatus can turn such groups toward greater use of terrorism to avoid direct confrontations with the state.[22] To account for these relationships, we included a measure of the "quality" of the state's military, derived from military spending per military service member. This measure has also been shown to be a predictor of coup risk.[23]

In our quantitative models of coup attempts, we also included an additional measure of institutional military cohesion. Regimes facing heightened risk of coups often undertake measures to coup-proof their militaries to either deter would-be challengers or make coordination between different military units especially difficult. Often, such coup-proofing measures center on creating multiple, redundant military organizations to distribute power more broadly among different military units and counterbalance the strength of any would-be coup plotters. To capture this process, we included a measure that captures the number and strength of redundant military organizations within each state's military apparatus.[24]

Similarly, the actions of adversary states on the continent might gradually undermine U.S. security goals in Africa, either by facilitating intrastate conflicts or by working against U.S. ideals concerning human rights in Africa. Although we cannot directly measure SSA from other states, we use data on arms transfers from Russia and China to African governments, taken from the Stockholm International Peace Research Institute, as a proxy for adversaries' military assistance.

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[22] Cullen S. Hendrix and Joseph K. Young, "State Capacity and Terrorism: A Two-Dimensional Approach," *Security Studies*, Vol. 23, No. 2, 2014, pp. 329–363.

[23] Gabriel Leon, "Loyalty for Sale? Military Spending and Coups d'État," *Public Choice*, Vol. 159, Nos. 3–4, June 2014, pp. 363–383; Jonathan M. Powell, "Determinants of the Attempting and Outcome of Coups d'État," *Journal of Conflict Resolution*, Vol. 56, No. 6, 2012, pp. 1017–1040.

[24] Ulrich Pilster and Tobias Böhmelt, "Coup-Proofing and Military Effectiveness in Interstate Wars, 1967–1999," *Conflict Management and Peace Science*, Vol. 28, No. 4, 2011, pp. 331–350.

Finally, we included several measures of the broader conflict environment facing states. First, we included a measure of the level of civil war among each state's neighbors, which has been shown to increase both a state's risk of civil war and the risk that the state utilizes violent repression to keep its own population in check.[25] We also included a dichotomous indicator of whether a civil war was ongoing in the state or, in the case of our civil-war onset models, an indicator of whether a civil war had previously occurred in the state and a variable measuring time since the state last experienced a civil war. Alternatively, for our models of coup risk, we included a variable measuring whether the state had previously experienced a coup and a variable measuring time since the state last experienced a coup.

This list of contextual factors is extensive. To ensure that hidden relationships among our control variables did not drive our model results, we also ran variants of our models in which we dropped all control variables and focused on the bivariate relationship between U.S.-provided SSA and the outcomes of interest. In the discussion of checks on the robustness of our results, we report the results of these alternative models.

Research Design and Threats to Inference

Modeling Approach and Estimation Strategy

Having described the data for our analysis, we now discuss our research design and general modeling strategy. This study is a large-*N*, quantitative analysis that pools all African countries (excluding Egypt) in the post–World War II period. Missing data preclude us from going as far

[25] Specifically, we use a weighted-average spatial lag to measure the level of civil war around each state, which increases as more-proximate states experience civil war. See Alex Braithwaite, "Resisting Infection: How State Capacity Conditions Conflict Contagion," *Journal of Peace Research*, Vol. 47, No. 3, 2010, pp. 311–319; Halvard Buhaug and Kristian Skrede Gleditsch, "Contagion or Confusion? Why Conflicts Cluster in Space," *International Studies Quarterly*, Vol. 52, No. 2, June 2008, pp. 215–233; Nathan Danneman, and Emily Hencken Ritter, "Contagious Rebellion and Preemptive Repression," *Journal of Conflict Resolution*, Vol. 58, No. 2, 2014, pp. 254–279; and Kristian Skrede Gleditsch, "Transnational Dimensions of Civil War," *Journal of Peace Research*, Vol. 44, No. 3, 2007, pp. 293–309.

back as 1946 or up to 2017, but the available data nonetheless allow for a relatively large sample. This pooled sample of time-series–cross-section (TSCS) data are often referred to as panel data. TSCS or panel analysis can entail a variety of challenges depending on data structure and modeling approach, which we address next and throughout the discussion of our results.[26]

In all of our models, the unit of analysis is the country-year. A country-year (e.g., Morocco-2005) uniquely identifies a specific country for a particular year. For any given outcome measure, our general modeling approach estimates SSA's effects on that dependent variable, controlling for a variety of confounding factors. Because countries systematically differ from each other in important ways, which can often be hard to control for, our analyses cluster standard errors on countries.[27]

Because SSA might be implemented differently in different contexts, capturing all contextual effects through simple control variables is insufficient. Instead, we examined interactions between SSA and two contextual factors in particular: the Cold War and ongoing PKOs. As discussed in Chapter Three, the Cold War heavily influenced both incentives and capabilities for political violence. But beyond these direct effects, it also shaped the way that U.S.-provided SSA was implemented. Similarly, although PKOs themselves help to reduce incentives and opportunities for violence, they also influence the way in which SSA is conducted. To better capture these dynamics, we estimated models that included interaction terms between PKOs and SSA, and we split the panel into separate periods (e.g., Cold War and post–Cold War years) in addition to looking at all country-years together.

Equation B.1 represents our baseline model. This equation is a general formulation and holds across various outcome measures. We regressed the dependent variable (e.g., civil-war onset in country *i* in

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[26] For a recent review of these challenges, see Nathaniel Beck and Jonathan N. Katz, "Modeling Dynamics in Time-Series–Cross-Section Political Economy Data," *Annual Review of Political Science*, Vol. 14, June 2011, pp. 331–352.

[27] This clustering method helps adjust standard errors for correlation of residuals within clusters (i.e., years grouped by country).

time *t*) on a four-year lag of SSA. To correct for varying accounting practices described earlier, the SSA variable is a three-year moving average. SSA's estimated effect on the outcome measure can be seen in $\beta_{i,t-4}$. We also included a lagged vector of controls, with $\tilde{\gamma}_{i,t-1}$ representing the corresponding coefficients. We first estimated Equation B.1 for all country-years, then replicated it for both the Cold War and post–Cold War periods, although some control variables differed slightly:

$$Y_{i,t} = \alpha + \beta_{i,t-4}(SSA) + \tilde{\gamma}_{i,t-1}(controls) + \in_{i,t} . \quad \text{(B.1)}$$

Equation B.2 adds the interaction term for SSA and ongoing PKOs. Like before, this term is lagged four years, and its estimate is captured by $\phi_{i,t-4}$. We estimated this model for the full panel and the post–Cold War period. Unfortunately, there are not enough country-years with PKOs before 1991 for us to estimate this conditional model for the Cold War period:

$$Y_{i,t} = \alpha + \beta_{i,t-4}(SSA) + \phi_{i,t-4}(SSA \times PKO) + \tilde{\gamma}_{i,t-1}(controls) + \in_{i,t} . \quad \text{(B.2)}$$

Admittedly, period-specific models and interaction terms do not provide an ideal test of our hypotheses on the effects of SSA. Given the data constraints that limited our analysis and the granularity with which we could evaluate effects, this approach is a second-best strategy. These interactions represent a coarse test of how effects should vary depending on how SSA is targeted and implemented. The use and execution of SSA have changed over time (e.g., increasing emphasis on SSR), and, although we would ideally capture these changes directly, the split-panel regressions and interactions at least offer a feasible approach for getting at some of these changes. However, the data still constrain this operationalization. The split panel reduces statistical power for the period-specific models, making it harder to identify effects. And the interaction term depends on only 122 country-years of PKOs, which makes the estimates sensitive to relatively few cases.

Our estimation strategy varied depending on the specific outcome measure. For binary variables, such as civil-war onset and coup attempts, we used a simple logistic regression. We also incorporated splines to better model some of the temporal processes that are typically found in TSCS analysis. In addition to these logit models, in our analysis of repression, we estimated OLS regressions. This outcome measure is continuous and thus best suited to traditional OLS. Finally, for our analysis on terrorist attacks, we used a negative binomial model. Terrorist attacks represent a discrete count, which derives from a data-generating process (DGP) best captured by a negative binomial model that simultaneously estimates the regression coefficients and a dispersion parameter.[28]

Threats to Inference

Before interpreting our results, we should discuss two critical threats to inference that are common across all of our models. Although each outcome model introduces its own challenges, *selection problems* and *missingness of data* can be found throughout our analysis. These problems are not terribly surprising: After all, such challenges are pervasive in observational studies of politics. Although scholars have devoted significant effort to developing methods for mitigating such problems, no correction can fully overcome these fundamental challenges.

Threat to Inference 1: Selection Bias

In the case of SSA, we have great reason to be concerned about selection bias. SSA is not randomly assigned; instead, policymakers strategically allocate aid based on a variety of political, social, and economic factors. And to the extent these factors are associated with civil war or a poor human-rights record, any positive relationship can be inflated or entirely driven by selection.[29] Scholars have long recognized this con-

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[28] Alternative estimation strategies include Poisson regression and a zero-inflated model.

[29] In a simple cross-sectional analysis, selection effects reduce to a form of omitted-variable bias. In our case, the omitted variables are those factors that (probabilistically) determine whether a country will be selected to receive SSA in a given year. In time-serial or panel analysis, such as our models, selection problems can even resemble endogeneity. If a given country is more or less likely to receive SSA in time *t* because of some observed characteristic

cern and offer a variety of potential solutions. All the potential solutions share a basic approach: Given that we know that SSA is not randomly assigned, we can try to model this selection process and incorporate it into the analysis. In effect, this approach begins by trying to answer the question of why some countries (in some years) receive SSA and others do not. If we can first describe the strategic logic of this policy choice, we can model it.

Several potential modeling strategies can be used to help mitigate selection problems.[30] Our preferred strategy is inverse propensity weighting. Like the Heckman selection model, propensity modeling is a two-stage approach. The first stage uses observed covariates to estimate the propensity to assignment (i.e., selection). Crucially though, this first stage does not require an instrument, like Heckman does. Nor are the first-stage estimates used as a control in subsequent models; they are instead transformed into propensity scores (i.e., the expected probability that a given country in a given year will receive SSA). These scores are then inverted and used to weight each unit (i.e., country-year) in the second-stage regressions. This process helps to reweight the data set to better reflect a nonbiased sample.[31]

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in time $t - 1$ (e.g., levels of terrorist activity), SSA's estimated effect on that characteristic in time $t + 1$ (e.g., future levels of terrorism) cannot be causally identified. Because outcome measures correlate over time (i.e., the number of terrorist attacks one year is associated with the number of attacks two years later), the direction of causality becomes unclear. Lagged right-side variables help correct this problem but can do only so much when outcomes follow from complicated, long, and slow-moving processes.

[30] The first, and most famous, of these approaches is the Heckman selection model. Like many of the approaches that would come later, this model uses a two-stage estimation strategy. The first stage estimates the likelihood that a given state will be selected to receive SSA. These estimated probabilities are then incorporated into the second stage to control for selection. Unfortunately, this approach is not suited for our project because our data do not satisfy the identifying assumptions. In particular, Heckman requires that the first stage include an instrument that satisfies the exclusion restriction. Absent this instrument, Heckman identification depends on distributional assumptions and is ill specified. See James J. Heckman, *Sample Selection Bias as a Specification Error (with an Application to the Estimation of Labor Supply Functions)*, Cambridge, Mass.: National Bureau of Economic Research, Working Paper 172, March 1977.

[31] An alternative but related approach uses the weights to build a matched sample. Matching procedures (e.g., one-to-k, caliper) vary significantly and can dramatically reduce the sample

The propensity-weighting strategy offers clear advantages over other methods, but it is not perfect. Second-stage regression results can be sensitive to outlier weights and model misspecification. These challenges were particularly difficult for this study given the various outcome measures that we analyzed in the second stage. If the provision of SSA is used, at least in part, to prevent these outcomes from occurring in the first place, and if these underlying outcomes vary somewhat in the conditions that produce them, we might not have been able to capture all of the important factors in the first stage. This approach, like Heckman or any other model that does not exploit random assignment to overcome selection, ultimately depends on the observable covariates. As a result, it cannot overcome data-quality issues or model unobservables.

Notwithstanding these challenges, we believe that propensity weighting can reduce selection bias and increase our overall confidence in the results. As a result, we incorporates these weights in all of our main models. In each section, we note the extent to which our findings change after dropping the propensity weights.

Threat to Inference 2: Missing Data

The problem of missing data is particularly difficult. This problem has two major consequences for our analysis: (1) It definitely reduces statistical power, and (2) it can introduce sample bias. First, consider the issue of statistical power. In statistical analysis, our ability to detect an effect, if one exists, depends partly on the sample size. The statistical power of our test represents the likelihood that we will make a type II error (i.e., fail to reject the null hypothesis of no effect). Missing data reduces the sample size in our regressions, inflating the standard errors and making it harder to tease out the potential effect of SSA. Given the data-quality and reporting issues in many developing countries, our focus on Africa obviously imposed constraints on this study's feasible sample size and overall power. Notwithstanding this challenge, most of

size. Given our various data constraints and the challenges of getting balance on a sufficiently large matched sample for period-specific (i.e., Cold War and post–Cold War) analyses, we prefer the inverse propensity-weighted regression approach.

our analyses (at least those that do not split the time period) included 1,200 to 1,600 country-years.

More concerning, however, is the problem of sample bias, which can also result from missing data. The DGP can take a variety of forms, encompassing different random and systematic processes that produce and record data. Often in social science, this DGP does not result in a complete data set, leaving researchers with a sample of the total population. Ideally, this "missingness" is completely at random. If this missingness is nonrandom, the incomplete data will be unrepresentative and suffer from sample bias. But not all bias is equally problematic. In general, the most-concerning forms of bias result from missing data that are systematically associated with higher or lower values of some variable (e.g., if GDP data are more likely to be missing for poorer countries than for other countries).

Given such nonrandom missingness, there are few options left to the researcher, whose only real recourse is to determine the direction of the bias, positive or negative. If this missing-data problem follows some clear pattern or conforms to our theoretical expectations about the DGP, we can at least speculate about the effects of this bias on our results, either making it more or less likely that the observed effects are genuine. If the bias works against finding a significant result, it makes for a more conservative test of the theory. If, however, the bias operates in the opposite direction, the researcher must acknowledge the limits of interpreting these results and the greater risk of making a type I error (i.e., falsely detecting some effect).

Alternatively, other forms of missingness are less problematic, and various approaches exist for addressing this challenge. If, for example, a variable's missingness is not correlated with the values of that variable (e.g., GDP is equally missing for rich and poor countries but disproportionately missing for countries engulfed in civil war), we can use other variables to impute the missing values. This procedure can take a variety of forms (e.g., mean or deterministic imputation), but we prefer to use *multiple imputation*.[32] This method is a form of iterative stochas-

[32] In our robustness checks, we implemented multiple imputation using the Stata 14 mi commands.

tic imputation, which uses existing data and a random component to produce a series of new data sets. Instead of simply imputing a single value for each missing observation, this procedure creates multiple data sets with randomly different values to help incorporate the uncertainty around the imputed values. This process can dramatically increase the sample size, helping improve power. We can then estimate our models on these imputed data sets and pool the coefficients and standard errors to adequately capture the expected effect size and uncertainty.

We should note, however, that this method is not without its limits. Multiple imputation can address only some forms of bias: If the data do not satisfy the method's assumptions (such as in the example of missing GDP for poorer countries), imputation will only increase the power without mitigating bias. Moreover, under such conditions, imputation can actually exacerbate the bias, and we would have little way of recognizing this problem. Additionally, imputation also has more-immediate implications for our analysis of SSA. As we discussed, selection bias represents a major challenge to studying SSA, which we address using propensity weights. Unfortunately, the imputed models do not allow us to estimate or use propensity weights. For this reason, and because of our concern that imputation can exaggerate bias, we treated the imputed models as a robustness check and make note of these results only where relevant.

Propensity Weights and the Allocation of Security Sector Assistance

Having described the data and research design, we now proceed to the analysis of results, beginning first with the selection models. Table B.3 reports the results from our first-stage models, which estimated a series of logistic regressions to evaluate the selection of SSA recipients. The dependent variable assigns 1 to a country if it receives *any* aid in a given year and 0 otherwise. This outcome measure is the weakest threshold for SSA selection. Some recipient countries receive as little as $698 in a given year but nonetheless receive a 1 on this variable.

Table B.3
First-Stage Models on the Selection of Security Sector Assistance Recipients

Variables	Model 1: Any SSA	Model 2: Cold War	Model 3: Post–Cold War
Polity level	–0.044*	–0.063*	0.028
	(0.026)	(0.033)	(0.042)
Political corruption	1.669***	3.777***	–1.330
	(0.637)	(0.969)	(1.119)
State repression	–0.517**	–0.212	–1.036***
	(0.210)	(0.274)	(0.316)
Youth bulge	0.595**	0.945**	–0.458
	(0.285)	(0.387)	(0.822)
Arms transfers from Russia or China	–0.084*	–0.280***	0.183*
	(0.046)	(0.082)	(0.104)
GDP per capita	–0.075	–0.323	–0.170
	(0.234)	(0.306)	(0.327)
Political instability	0.314	0.640*	–0.137
	(0.237)	(0.349)	(0.361)
Neighborhood civil war	–0.743	–0.771	–0.070
	(0.466)	(0.582)	(0.867)
Time since last successful coup	0.043***	0.078***	0.023
	(0.012)	(0.024)	(0.014)
Time since last civil war	0.004	0.042**	–0.027*
	(0.019)	(0.019)	(0.016)
Post-9/11	0.693	n/a	0.591
	(0.460)	n/a	(0.429)
Cold War	–0.983**	n/a	n/a
	(0.395)	n/a	n/a

Table B.3—Continued

Variables	Model 1: Any SSA	Model 2: Cold War	Model 3: Post–Cold War
Soviet ally	–1.407***	–2.498***	n/a
	(0.353)	(0.378)	n/a
Constant	0.082	–1.071	4.061
	(1.994)	(2.326)	(3.294)
Observations	2,171	1,148	1,023

NOTE: Cells report beta coefficients with robust standard errors in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

In Table B.3, model 1 examines this allocation choice over the entire post–World War II period, while models 2 and 3 break out this analysis across periods, focusing on the Cold War and post–Cold War periods, respectively. We included a broad set of variables that captures regime characteristics, political and economic development, recent domestic conflict, and other international security conditions. Most of the variables can also be found in the second-stage models. However, some variables that we included in the second stage are not found in the propensity models. Variables omitted from the first stage were excluded for one of two possible reasons: (1) We do not have a good theoretical reason to believe that the variable (e.g., ODA, excluded-population size) directly influences the SSA selection process, or (2) the variable (e.g., percentage of GDP from natural-resource rents, number of parallel military institutions) has too many missing observations, which not only reduces the first-stage model sample size but also constrains the estimation of propensity weights and thus limits the second-stage models' statistical power.

First, consider how regime characteristics and other political conditions of a partner nation shape this selection choice. Generally, we find that such concerns operated in significantly different ways across the Cold War and post–Cold War period. The pooled results from model 1 suggest that SSA recipients tend to be more corrupt and less repressive. Importantly, though, these results are clearly being driven

by distinct periods. Increasing corruption is strongly associated with more SSA in the Cold War period (see model 2), but this effect does not appear in the post–Cold War period. In contrast, we see that the marginally significant coefficient on repression in the pooled model is being driven by the post–Cold War period (see model 3). For the post–Cold War period, increasing repression is significantly associated with a decrease in SSA. No such evidence suggests holds for the Cold War period, when the selection of SSA recipients appears to be unrelated to a regime's level of human-rights abuses and other forms of repression. The polity variable reveals a similar trend to that of corruption: Less democratic regimes were more likely to receive SSA, but this result is driven by the Cold War period. No such result holds for the post–Cold War period. Nor are these effects particularly robust, reaching marginal significance only at the 0.10 level.

Beyond regime characteristics, we see that domestic security conditions drive the choice of SSA recipients. There is some weak evidence that, during the Cold War, increasing political instability was associated with a greater likelihood of receiving SSA. This result, however, is significant only at the 0.10 level and is not robust across periods. We see a stronger result on the coefficient for youth bulge, which is positive and significant in both the pooled model and the Cold War period. This result might suggest that SSA is directed to states that experience large shocks of young, fighting-age men. Notably, though, this coefficient becomes negative but insignificant in the post–Cold War period.

We also included two time variables to capture the relative insecurity of a potential SSA recipient. The first of these variables is the time since the last civil war. In the full model, this variable's effect is indistinguishable from zero. However, the coefficient is positive and highly significant in the Cold War period. This result indicates that, as a country gets further in time since its last civil war, it is more likely to receive SSA. Such a finding might suggest that, during the Cold War, the United States preferred more-stable countries for SSA provision. There is no evidence of a similar trend in the post–Cold War period, when the reverse might be true. The coefficient is negative and marginally significant in model 3, which might suggest an altogether different selection strategy. This finding indicates that, since the end of the Cold

War, the United States has tended to provide SSA to those countries that have more recently experienced civil war, possibly to buttress their governments or support postconflict stabilization.

The coefficient on the variable for time since the last coup is positive and significant in models 1 and 2. This result suggests that countries were more likely to receive SSA as time since the last coup increased. Such a trend might reveal the U.S. preference during the Cold War for more-stable, consolidated regimes that had not recently experienced a coup. The coefficient in the post–Cold War model is positive but not quite significant.

Finally, we included several variables meant to capture the broader strategic or international environment. The coefficient on the Cold War dummy in model 1 suggests that, in general, countries were less likely to receive SSA in the Cold War period. However, we see no significant result on the post-9/11 dummy variable. As expected, the dummy variable for being a Soviet ally is negative and highly significant in models 1 and 2.

Only the coefficient on adversary arms transfers produces surprising results. This variable is negative and significant in the full model and Cold War period. But it is positive and significant in the post–Cold War period. In models 1 and 2, adversary arms transfers are associated with reduced likelihood of receiving SSA. This effect suggests that, during the Cold War, the United States was less likely to provide SSA to countries receiving arms from Russia or China. Surprisingly, the coefficient on this variable changes signs in the post–Cold War period, becoming positive and significant (albeit only at the 0.10 level). This result might suggest a new competitive pressure driving SSA provision. With the distinction between allied and enemy states less stark since the Cold War, the United States might use SSA to compete for influence in countries that Russia and China have traditionally controlled or are actively pursuing.

However suggestive the results of Table B.3, these models offer more than just a snapshot of patterns in SSA provision. They also serve as the first-stage models for estimating the propensity weights used throughout our subsequent analysis. Crucially, these models have revealed real differences in the selection processes operating in the Cold

War and post–Cold War periods. As a result, our second-stage models used propensity weights from model 1 whenever estimating a full panel and weights from model 2 or 3 whenever we focused the analysis on the Cold War or post–Cold War, respectively. We now turn to these second-stage models, beginning with civil-war onset.

Civil-War Onset

Table B.4 details the results of our quantitative models that assess SSA's impact on the risk of civil-war onset among African states. Although model 1 examines this relationship over the entire post–World War II period, model 2 and model 3 separately assess this relationship in the Cold War and post–Cold War periods, respectively. In addition, model 4 examines the conditional effects that active UN PKOs have had on SSA effects in the post–World War II period. Because the vast majority of UN PKOs have occurred since the end of the Cold War, model 5 examines this same conditional relationship specifically in the post–Cold War period.[33]

Contrary to our expectations, the positive and significant coefficient for SSA funding in model 1 in Table B.4 suggests that increasing the level of SSA funding to an African state actually increases the risk that the partner state will experience a civil war in the near future. Interestingly, this adverse effect occurs even when controlling for possible selection effects in which the United States provides greater sums of assistance to high-risk partners. To better interpret this relationship, Figure B.1 provides a visual representation of this relationship by plotting the predicted risk that a state will experience a civil war in a given year as a function of increasing levels of SSA. The solid black line of Figure B.1 shows the predicted probability that an African state will experience a civil war in a given year as levels of SSA per 10,000 people

[33] When using multiple imputation to circumvent missing-data problems, the results of our quantitative models generally conform to the results presented in Table B.4. The level of significance is attenuated, but all variables that achieve statistical significance in Table B.4 are also statistically significant in our imputed models.

Table B.4
The Effects That Security Sector Assistance Has on the Risk of Civil War Among African States

Variable	Model 1: Post–World War II	Model 2: Cold War	Model 3: Post–Cold War	Model 4: Ongoing PKO	Model 5: Ongoing PKO Post–Cold War
Total SSA per population, three-year moving average and four-year lag	0.151**	0.202*	0.046	0.184***	0.092
	(0.062)	(0.119)	(0.088)	(0.065)	(0.092)
Ongoing UN PKO, one-year lag	1.427**		1.030	0.891	0.984
	(0.633)		(0.671)	(1.325)	(1.292)
Ongoing UN PKO, four-year lag				1.626	1.075
				(1.987)	(1.641)
Total SSA per population, three-year moving average × ongoing PKO four years prior				−0.413**	−0.534**
				(0.172)	(0.212)
GDP per capita	−0.903**	−1.759*	−1.076	−0.889**	−1.056
	(0.430)	(0.920)	(0.959)	(0.450)	(0.980)
Polity level	0.285	−0.031	0.115	0.277	0.086
	(0.195)	(0.363)	(0.200)	(0.198)	(0.195)
Policy level, squared	−0.012	0.008	−0.005	−0.012	−0.003
	(0.010)	(0.021)	(0.010)	(0.010)	(0.010)

Table B.4—Continued

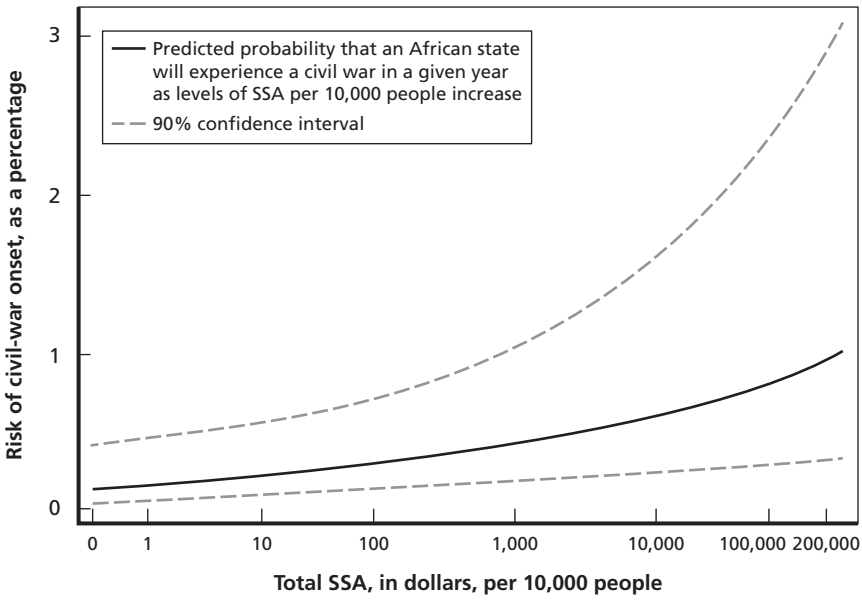
Variable	Model 1: Post–World War II	Model 2: Cold War	Model 3: Post–Cold War	Model 4: Ongoing PKO	Model 5: Ongoing PKO Post–Cold War
Percentage of GDP from natural-resource rents	0.246	0.097	0.362*	0.271*	0.416**
	(0.164)	(0.452)	(0.206)	(0.163)	(0.197)
Percentage of GDP from OECD ODA	–0.574**	–0.660	–0.106	–0.580**	–0.043
	(0.241)	(0.507)	(0.423)	(0.254)	(0.421)
Excluded-population size	0.362	2.173**	0.397	0.314	0.340
	(0.591)	(1.029)	(0.882)	(0.594)	(0.840)
Youth bulge	0.384	0.244	2.603*	0.412	2.518*
	(0.639)	(0.791)	(1.412)	(0.665)	(1.396)
Military spending per service member	0.023	0.549	0.267	0.007	0.245
	(0.117)	(0.361)	(0.306)	(0.122)	(0.307)
Arms transfers from Russia or China	0.162	–0.252	0.226	0.175	0.252
	(0.108)	(0.279)	(0.170)	(0.108)	(0.173)
Neighborhood civil war	2.404***	–4.222*	3.055***	2.422***	2.901***
	(0.602)	(2.354)	(0.965)	(0.633)	(0.979)

Table B.4—Continued

Variable	Model 1: Post–World War II	Model 2: Cold War	Model 3: Post–Cold War	Model 4: Ongoing PKO	Model 5: Ongoing PKO Post–Cold War
Previous civil war	–1.933***	–0.899	–1.829	–1.976***	–1.690
	(0.666)	(1.272)	(1.219)	(0.657)	(1.211)
Francophone Africa	0.829**	0.913	0.673	0.888**	0.599
	(0.413)	(0.852)	(0.689)	(0.422)	(0.677)
Cold War	0.140			0.185	
	(0.532)			(0.549)	
Post-9/11	–0.999*		–0.397	–0.988	–0.396
	(0.571)		(0.781)	(0.627)	(0.801)
Constant	1.335	2.851	–4.045	1.217	–4.274
	(4.225)	(7.882)	(7.977)	(4.645)	(8.253)
Observations	1,269	562	707	1,269	707

NOTE: Cells report beta coefficients with robust standard errors clustered by country in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Models also included time since the last civil war and cubic polynomials (not shown). We weighted observations by propensity scores estimated in the first-stage model.

Figure B.1
Risk of Civil-War Onset as Security Sector Assistance Increases



SOURCES: N. Gleditsch et al., 2002; Allansson, Melander, and Themnér, 2017.
NOTE: This figure corresponds to model 1 in Table B.4. All other continuous variables are held at their mean values, and all other dichotomous variables are held at their modal values.
RAND *RR2447-B.1*

increase. The dashed gray lines represent the upper and lower bounds of the 90-percent confidence intervals around this prediction.

Two important substantive findings are immediately apparent in Figure B.1. First, in real terms, the risk of civil war, even among states receiving significant amounts of SSA, is very small. To be sure, civil wars are rare events; even among states receiving the highest amounts of SSA in our analyses, the risk of civil war is only about 1.0 percent.

In relative terms, however, a change from a state receiving no SSA to receiving roughly $200,000 of SSA per 10,000 people, a change representing the full range of our analyses, results in a roughly 900-percent increase, from 0.01 percent to 1.0 percent, in the risk that a state will experience a civil war in a given year. Although the change in state's

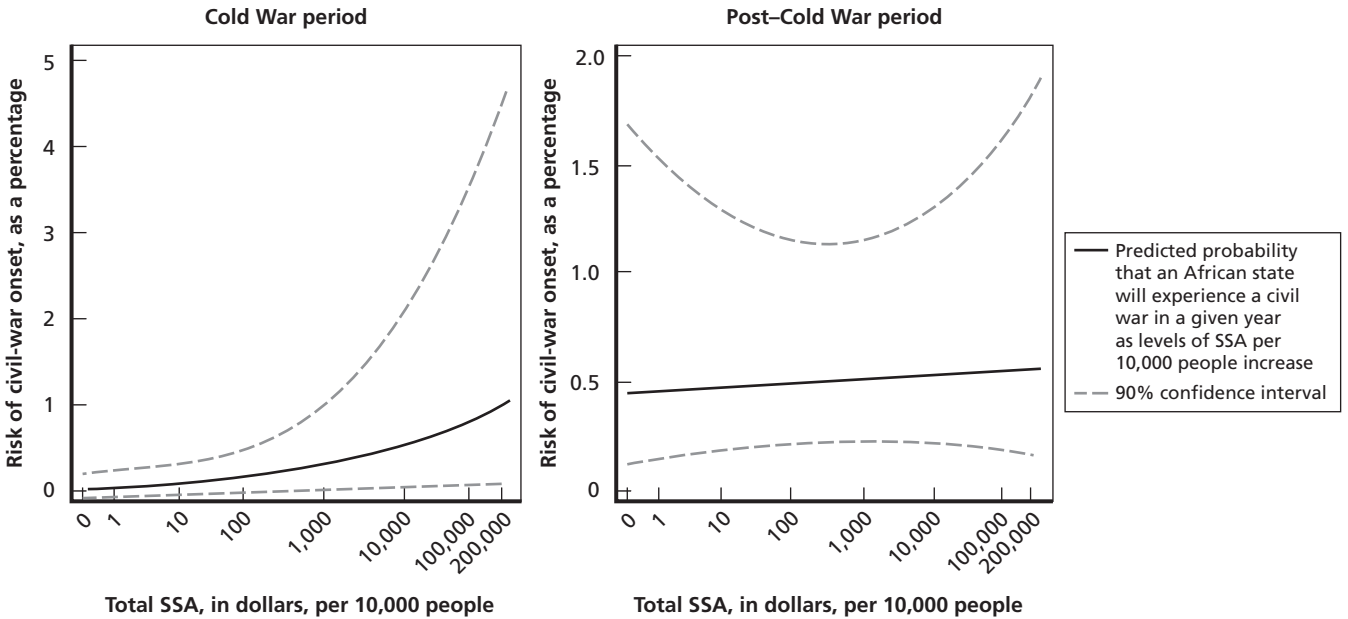
annual risk of civil war is small in absolute terms, this relative change in the yearly risk of civil war should not be overlooked: Over a decade, the risk of civil war would increase from 0.10 percent to 10 percent across the range of our analyses and even more over longer time periods.

As shown in model 2 and model 3 in Table B.4, however, this adverse effect is largely contained to the Cold War period; although the relationship between SSA and civil-war onset is positive and significant in the Cold War period, there is no statistically significant relationship in the post–Cold War period. To help us better understand these diverging effects, Figure B.2 plots the predicted risk of civil war over the range of SSA in our analyses in the Cold War and post–Cold War periods, respectively. The solid black line in Figure B.2 shows the predicted probability that an African state will experience a civil war in a given year as levels of SSA per 10,000 people increase during the Cold War and post–Cold War periods, respectively. The dashed gray lines represent the upper and lower bounds of the 90-percent confidence intervals around these predictions.

Again, civil wars are rare events, and dramatically increasing levels of SSA funding to African states during the Cold War only marginally increases the risk of civil wars among partner states: The predicted risk of civil-war onset in a given year increases only from 0.1 percent to 1.0 percent across the full range of our data. Although this is small in absolute terms, this increasing risk that a state will experience a civil war in a given year is very large in relative terms, a level of risk that only increases further as these effects propagate through multiple years and throughout the AFRICOM AOR.

From a policy perspective, these period-specific relationships are interesting and perhaps suggestive of the impact of U.S. regional strategy in measuring the success of SSA to partner states. During the Cold War era, U.S. regional policy in Africa was driven largely by the need and desire to balance against Soviet influence, rather than directly accounting for the internal power dynamics of partner states. As a result, the adverse relationship between U.S. assistance and civil wars during the Cold War period was probably significantly driven by U.S.–Soviet efforts at counterbalancing through support to regional powers and opposition movements. In contrast, the post–Cold War

Figure B.2
Risk of Civil-War Onset as Security Sector Assistance Increases in the Cold War and Post–Cold War Periods



SOURCES: N. Gleditsch et al., 2002; Allansson, Melander, and Themnér, 2017.
NOTE: The figures correspond to model 2 and model 3 in Table B.4, respectively. All other continuous variables are held at their mean values, and all other dichotomous variables are held at their modal values.
RAND *RR2447-B.2*

period, especially the post-9/11 era, has been more focused on specifically mitigating the risk of intrastate conflicts among regional partners. It is highly likely, then, that the ways in which SSA is implemented among partner states, toward deterring or preventing intrastate conflict, accounts for the attenuation of the positive relationship seen during the Cold War.

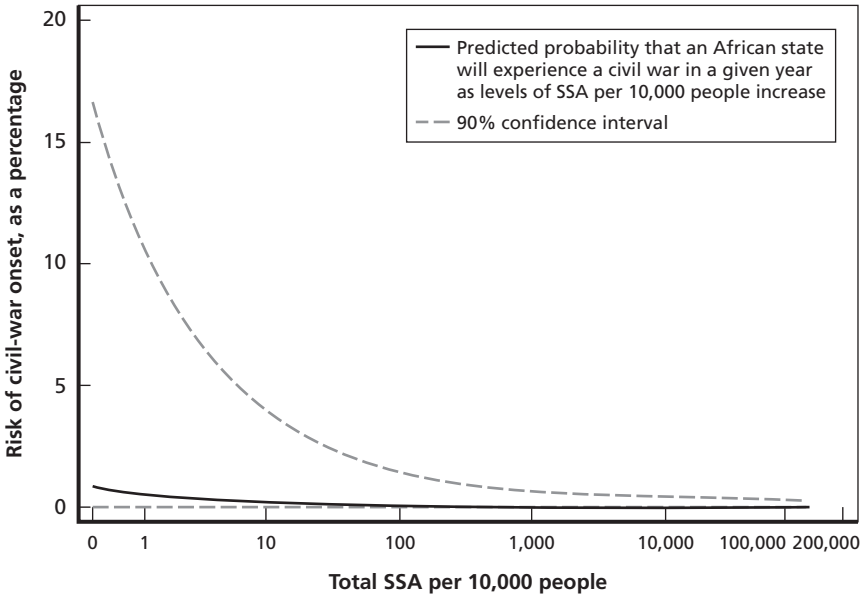
Aside from these direct effects that SSA has on states' risk of experiencing civil war, we examined several additional models to assess whether the relationship between SSA and civil war varies by the characteristics of partner states. Specifically, we used a series of interactions to separately examine whether regime type, bureaucratic capacity, and the quality of partner states' military forces influence SSA's effects on civil war. The effects of interactions between SSA and these conditioning characteristics on states' risk of civil war were not statistically significant in our models.

When accounting for the broader security environment in the state, the results of our quantitative models aligned more closely with our theoretical expectations, and we find strongly divergent effects of SSA on the risk of civil war centered on the presence of an active PKO at the time of assistance. Model 5 in Table B.4 examines SSA's effects on civil war in states with and without active PKOs during the post–Cold War period. Although SSA funding has no statistically significant effect on the risk of civil war among most African states in the post–Cold War era, increasing SSA funding, coupled with an active PKO, is associated with a significantly decreased risk of civil war.[34]

To help us better interpret the substantive effects of this relationship, Figure B.3 visualizes the probability of civil war facing states in a given year as levels of SSA increase. Immediately apparent is that, although they are statistically significant, the substantive effects of this relationship are quite small. Over the full range of our analyses, a change from no SSA to roughly $200,000 of SSA per 10,000 people,

[34] Model 4 in Table B.4 also reveals a negative and statistically significant relationship between the interaction of SSA and ongoing PKOs on the one hand and, on the other, the risk of civil-war onset in the entire post–World War II period. However, because most PKOs have occurred since the end of the Cold War, we constrained our substantive conclusions to that period.

Figure B.3
Risk of Civil-War Onset Among States with Active Peacekeeping Operations as Security Sector Assistance Increases in the Post–Cold War Period



SOURCES: N. Gleditsch et al., 2002; Allansson, Melander, and Themnér, 2017.
NOTE: The figure corresponds to model 5 in Table B.4. The plot shows the predicted probability of civil war as levels of SSA per 10,000 people increase in states with active UN PKOs. All other continuous variables are held at their mean values, and all other dichotomous variables are held at their modal values.

RAND *RR2447-B.3*

representing the full range of our analyses, decreases the risk of civil war in states with active PKOs from only about 0.97 percent to about 0.004 percent. In relative terms, however, this difference indicates that SSA significantly lowers the risk of civil war when utilized in conjunction with PKOs: The range of SSA in our analyses represents roughly a 97-percent decrease in states' risk of civil war during active PKOs.

In line with the broader literature on civil conflict is that several other variables are significantly associated with changes in the risk of civil war among African states. Broadly, increasing per capita wealth and increased levels of developmental assistance to African states are associated with a decreased risk of civil war. In contrast, a greater abun-

dance of exploitable natural resources, excluded and disenfranchised populations, and regional conflicts significantly increases the risk of civil war across our quantitative models.

As detailed in Table B.5, we also developed quantitative models to assess SSA's effects on the risk of high-intensity civil war among African partner states. Along those lines, models 1 through 5 parallel the structure of models 1–5 in Table B.4 but utilize a threshold of 1,000 battle-related deaths to demarcate periods of high-intensity conflict.

Interestingly, U.S. SSA does not significantly affect the risk of high-intensity civil war among partner states in any of our quantitative models, regardless of time period or broader security environment. Taken together, this perhaps suggests that, although SSA funding might marginally affect the risk of low-intensity conflict among African partner states, it has little effect on larger or more-violent conflicts in Africa. Also of note is that other large economic drivers of civil war in our previous models, GDP per capita and ODA, are also not associated with significant changes in states' risk of high-intensity civil war, broadly suggesting that economic tools to deter low-intensity conflicts might not affect higher-intensity civil wars. In contrast, abundant natural resources, political exclusion, and ongoing civil wars among neighboring states, all factors that potentially influence rebel groups' willingness to continue fighting or escalate conflicts, are statistically significant.

Terrorist Attacks

Table B.6 details the results of our quantitative models assessing SSA's effects on yearly terrorist attacks among African partner states. Model 1 assesses the relationship between SSA and terrorist attacks over the entire post–World War II period. Model 2 and model 3 examine this relationship over the Cold War and post–Cold War eras, respectively. To help us see any potential intervening effects of active PKOs on SSA, model 4 examines the conditional effects that active UN PKOs have on SSA's effects since 1945. Similarly, model 5 examines this condi-

Table B.5
Effects of Security Sector Assistance on Risk of High-Intensity Civil War Among African States

Variable	Model 1: High-Intensity Civil War	Model 2: Cold War	Model 3: Post–Cold War	Model 4: Ongoing PKO	Model 5: Ongoing PKO Post–Cold War
Total SSA per population, three-year moving average and four-year lag	–0.005	0.109	0.078	–0.014	0.086
	(0.065)	(0.128)	(0.120)	(0.068)	(0.132)
Ongoing UN PKO, one-year lag	–1.257		0.400	–0.847	0.521
	(1.085)		(0.992)	(0.893)	(0.731)
Ongoing UN PKO, four-year lag				–0.927	0.094
				(1.056)	(1.154)
Total SSA per population, three-year moving average × ongoing PKO four years prior				0.017	–0.149
				(0.312)	(0.272)
GDP per capita	–0.146	–1.368	0.896	–0.151	0.877
	(0.600)	(1.167)	(0.635)	(0.611)	(0.661)
Polity level	0.219	0.080	0.274	0.209	0.254
	(0.347)	(0.254)	(0.324)	(0.330)	(0.322)
Polity level, squared	–0.008	–0.005	–0.014	–0.008	–0.013
	(0.017)	(0.014)	(0.017)	(0.016)	(0.016)

Table B.5—Continued

Variable	Model 1: High-Intensity Civil War	Model 2: Cold War	Model 3: Post–Cold War	Model 4: Ongoing PKO	Model 5: Ongoing PKO Post–Cold War
Percentage of GDP from natural-resource rents	0.725*	0.517	0.890***	0.758*	0.886***
	(0.376)	(0.766)	(0.323)	(0.394)	(0.320)
Percentage of GDP from OECD ODA	0.163	0.197	0.229	0.207	0.231
	(0.254)	(0.597)	(0.434)	(0.275)	(0.422)
Excluded-population size	1.918***	2.572*	3.424***	1.904***	3.406***
	(0.627)	(1.550)	(1.048)	(0.656)	(1.046)
Youth bulge	–0.462	–0.703	1.397	–0.493	1.381
	(1.368)	(1.293)	(0.960)	(1.403)	(0.954)
Military spending per service member	0.036	1.789**	–0.134	0.056	–0.132
	(0.158)	(0.803)	(0.367)	(0.154)	(0.338)
Arms transfers from Russia or China	0.194	0.161	0.268*	0.203	0.268*
	(0.171)	(0.190)	(0.151)	(0.175)	(0.148)
Neighborhood civil war	3.390**	–0.291	2.977**	3.412**	2.908**
	(1.532)	(1.310)	(1.273)	(1.555)	(1.278)

Table B.5—Continued

Variable	Model 1: High-Intensity Civil War	Model 2: Cold War	Model 3: Post–Cold War	Model 4: Ongoing PKO	Model 5: Ongoing PKO Post–Cold War
Previous civil war	2.313**		0.331	2.376***	0.368
	(0.924)		(1.097)	(0.916)	(1.094)
Francophone Africa	–0.165	0.961	–1.324	–0.241	–1.329
	(0.754)	(0.686)	(0.845)	(0.731)	(0.809)
Cold War	0.591			0.532	
	(1.175)			(1.152)	
Post-9/11	–1.606***		–1.669**	–1.676***	–1.657**
	(0.538)		(0.693)	(0.512)	(0.690)
Constant	–8.762*	–14.414	–17.789**	–8.933*	–17.572**
	(5.036)	(11.255)	(7.738)	(4.950)	(7.757)
Observations	1,332	577	755	1,332	755

NOTE: Cells report beta coefficients with robust standard errors clustered by country in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Models also include time since the last civil war and cubic polynomials (not shown). We weighted observations by propensity scores estimated in the first-stage model.

Table B.6
The Effects That Security Sector Assistance Has on Numbers of Terrorist Attacks in African States

Variable	Model 1: Terrorist Attacks	Model 2: Cold War	Model 3: Post–Cold War	Model 4: Ongoing PKO	Model 5: Ongoing PKO Post–Cold War
Total SSA per population, three-year moving average and four-year lag	–0.028	–0.044	0.002	–0.014	0.032
	(0.034)	(0.034)	(0.052)	(0.033)	(0.053)
Ongoing UN PKO, one-year lag	0.830***	–1.154	0.575	0.539	0.387
	(0.315)	(1.261)	(0.475)	(0.384)	(0.384)
Ongoing UN PKO, four-year lag				1.618*	1.870**
				(0.878)	(0.919)
Total SSA per population, three-year moving average × ongoing PKO four years prior				–0.235**	–0.309**
				(0.114)	(0.144)
GDP per capita	–0.486*	0.316	–1.537***	–0.369	–1.396***
	(0.286)	(0.381)	(0.350)	(0.276)	(0.348)
Polity level	0.017	–0.027	0.106	0.020	0.093
	(0.077)	(0.134)	(0.109)	(0.079)	(0.106)
Polity level, squared	–0.000	0.003	–0.005	–0.000	–0.004
	(0.004)	(0.007)	(0.006)	(0.004)	(0.005)

Table B.6—Continued

Variable	Model 1: Terrorist Attacks	Model 2: Cold War	Model 3: Post–Cold War	Model 4: Ongoing PKO	Model 5: Ongoing PKO Post–Cold War
State bureaucratic capacity	0.520*	0.481	0.316	0.447	0.267
	(0.295)	(0.324)	(0.408)	(0.282)	(0.378)
Percentage of GDP from OECD ODA	–0.262*	0.059	–0.576***	–0.267**	–0.561***
	(0.139)	(0.165)	(0.192)	(0.135)	(0.184)
Excluded-population size	0.641*	0.550	1.209**	0.673*	1.420**
	(0.380)	(0.562)	(0.548)	(0.384)	(0.554)
Youth bulge	0.652	1.566***	–0.318	0.733	–0.258
	(0.525)	(0.495)	(0.754)	(0.541)	(0.756)
Military spending per service member	0.136	0.136	0.327**	0.135	0.312*
	(0.113)	(0.138)	(0.163)	(0.115)	(0.163)
Ongoing civil war	1.470***	1.469***	1.842***	1.535***	1.941***
	(0.271)	(0.280)	(0.358)	(0.277)	(0.366)
Francophone Africa	–0.422**	–1.213***	–0.260	–0.416**	–0.233
	(0.202)	(0.339)	(0.309)	(0.200)	(0.305)

Table B.6—Continued

Variable	Model 1: Terrorist Attacks	Model 2: Cold War	Model 3: Post–Cold War	Model 4: Ongoing PKO	Model 5: Ongoing PKO Post–Cold War
Cold War	–1.601***			–1.551***	
	(0.296)			(0.285)	
Post-9/11	–1.353***		–1.324***	–1.386***	–1.326***
	(0.229)		(0.323)	(0.232)	(0.327)
Number of terrorist attacks (previous year)	0.050**	0.218*	0.045**	0.050*	0.048**
	(0.024)	(0.115)	(0.020)	(0.026)	(0.022)
Constant	2.482	–6.801**	9.821***	1.482	8.608***
	(2.616)	(3.291)	(2.904)	(2.432)	(2.633)
Observations	1,635	719	916	1,635	916

NOTE: Cells report beta coefficients with robust standard errors clustered by country in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. We weighted observations by propensity scores estimated in the first-stage model.

tional relationship in the post–Cold War period.[35] Table B.7 follows the same structure but details SSA's effects on terrorist *casualties* rather than terrorist attacks.

Across our quantitative models, levels of U.S. SSA do not appear to directly affect numbers of terrorist attacks experienced by African partner states. This lack of a statistically significant relationship is evident in both the Cold War and post–Cold War periods. In addition to these direct relationships, we found no robust statistical evidence that SSA affects levels of terrorist activity in the context of partner states' bureaucratic or military capacities.

However, we found an exception to these general trends among states with active PKOs at the time of SSA. Again, we expected that, because of greater coordination, increased leverage, and greater stability provided by PKOs, SSA would have a greater effect in mitigating terrorist activity when a PKO was also present in the partner state. In line with our theoretical expectations, the coefficient for SSA among states with active PKOs is negative and statistically significant in both model 4 and model 5 in both Table B.6 and Table B.7. This suggests that, among partner states with active PKOs, increasing levels of SSA significantly decreases yearly levels of terrorist activity within a state.[36]

To help us better interpret these relationships, Figure B.4 plots the predicted number of terrorist attacks in African states as a function of SSA in the post–Cold War period. And Figure B.5 plots the predicted number of casualties caused by terrorist attacks as a function of SSA. The solid black line in each figure plots the predicted number of terrorist attacks experienced by African states in a given year and the predicted number of casualties resulting from terrorist attacks in a given

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[35] When using multiple imputation to overcome missing-data problems, we find that SSA does not significantly affect either the annual number or the lethality of terrorist attacks among African states in either the Cold War or post–Cold War period. In contrast with our main models, however, our quantitative models using multiple imputation also show no statistically significant relationship between levels of SSA and terrorist attacks among states with active PKOs.

[36] We found these statistically significant relationships in both the post–Cold War era and the entire post–World War II era. However, because most PKOs have occurred in the post–Cold War era, we constrain our discussion to this period.

Table B.7
Effects of Security Sector Assistance on Numbers of Casualties Caused by Terrorist Attacks in African States

Variable	Model 1: Terrorist Attack Casualties	Model 2: Cold War	Model 3: Post–Cold War	Model 4: Ongoing PKO	Model 5: Ongoing PKO Post–Cold War
Total SSA per population, three-year moving average and four-year lag	0.045	–0.010	0.120	0.052	0.146
	(0.058)	(0.079)	(0.096)	(0.060)	(0.099)
Ongoing UN PKO, one-year lag	1.367***	–2.798**	1.513**	1.295**	1.712**
	(0.445)	(1.315)	(0.738)	(0.548)	(0.724)
Ongoing UN PKO, four-year lag				0.724	0.529
				(0.951)	(1.237)
Total SSA per population, three-year moving average × ongoing PKO four years prior				–0.247*	–0.369*
				(0.132)	(0.202)
GDP per capita	–0.448	0.727	–2.151***	–0.434	–2.209***
	(0.504)	(0.862)	(0.581)	(0.526)	(0.590)
Polity level	0.020	–0.341	0.065	0.016	0.062
	(0.119)	(0.259)	(0.184)	(0.121)	(0.187)
Polity level, squared	–0.002	0.013	–0.004	–0.002	–0.004
	(0.006)	(0.014)	(0.009)	(0.006)	(0.010)

Table B.7—Continued

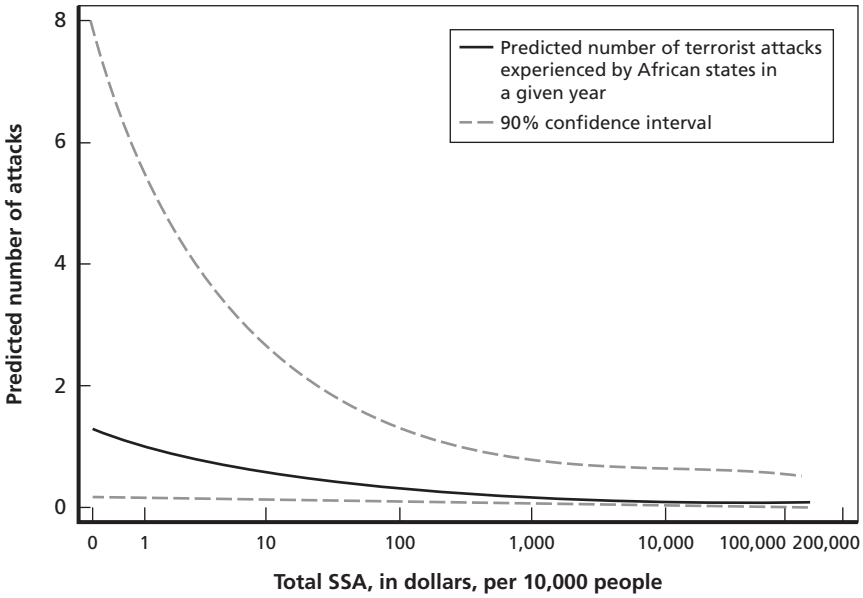
Variable	Model 1: Terrorist Attack Casualties	Model 2: Cold War	Model 3: Post–Cold War	Model 4: Ongoing PKO	Model 5: Ongoing PKO Post–Cold War
State bureaucratic capacity	0.835*	2.046***	–0.596	0.816*	–0.628
	(0.434)	(0.703)	(0.878)	(0.443)	(0.902)
Percentage of GDP from OECD ODA	–0.277	0.016	–0.641**	–0.269	–0.658**
	(0.263)	(0.337)	(0.266)	(0.264)	(0.268)
Excluded-population size	1.416**	3.112**	2.059**	1.431**	2.187**
	(0.579)	(1.292)	(0.856)	(0.579)	(0.867)
Youth bulge	1.481**	2.603***	–0.846	1.484**	–0.910
	(0.729)	(0.835)	(0.964)	(0.737)	(0.974)
Military spending per service member	0.247*	0.213*	0.450**	0.247*	0.441**
	(0.131)	(0.112)	(0.213)	(0.132)	(0.216)
Ongoing civil war	2.286***	2.831***	2.495***	2.300***	2.568***
	(0.315)	(0.943)	(0.522)	(0.322)	(0.559)
Francophone Africa	–0.210	–1.695**	0.221	–0.239	0.149
	(0.387)	(0.762)	(0.398)	(0.390)	(0.408)

Table B.7—Continued

Variable	Model 1: Terrorist Attack Casualties	Model 2: Cold War	Model 3: Post–Cold War	Model 4: Ongoing PKO	Model 5: Ongoing PKO Post–Cold War
Cold War	–1.743***			–1.738***	
	(0.542)			(0.546)	
Post-9/11	–1.070***		–1.490***	–1.066***	–1.457***
	(0.319)		(0.448)	(0.329)	(0.465)
Number of casualties of terrorist attacks (previous year)	0.008**	0.013**	0.010**	0.008**	0.011**
	(0.003)	(0.006)	(0.004)	(0.004)	(0.005)
Constant	0.670	–11.989*	15.435***	0.542	15.945***
	(4.809)	(7.274)	(4.471)	(4.965)	(4.615)
Observations	1,635	719	916	1,635	916

NOTE: Cells report beta coefficients with robust standard errors clustered by country in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. We weighted observations by propensity scores estimated in the first-stage model.

Figure B.4
Predicted Number of Terrorist Attacks as Security Sector Assistance Increases Among States with Active Peacekeeping Operations in the Post–Cold War Period

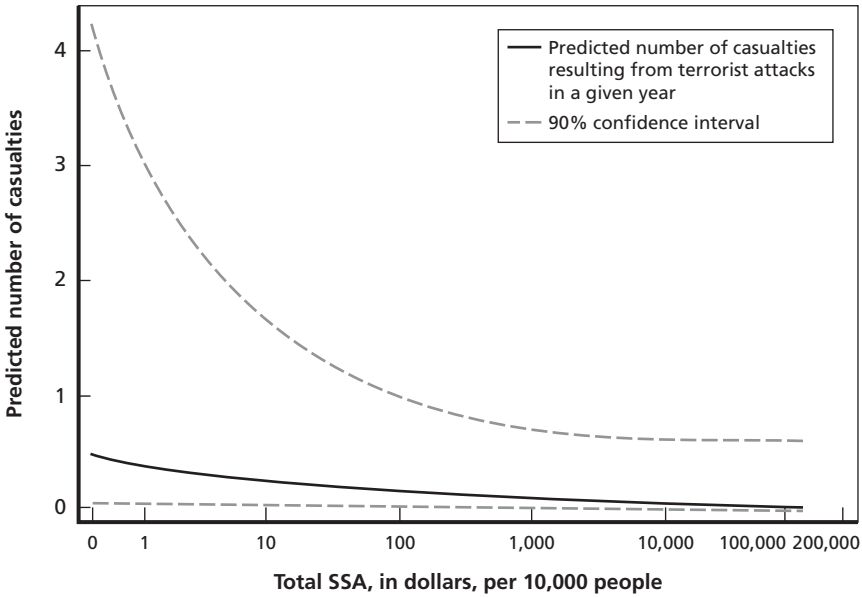


SOURCES: National Consortium for the Study of Terrorism and Responses to Terrorism, 2017; UN Peacekeeping, "List of Peacekeeping Operations, 1948–2018," undated.
NOTE: The figure corresponds to model 5 in Table B.6. All other continuous variables are held at their mean values, and all other dichotomous variables are held at their modal values.
RAND *RR2447-B.4*

year, respectively. The dashed gray lines in both figures represent the upper and lower bounds of the 90-percent confidence intervals around these predictions.

In either case, moving from no SSA to roughly $200,000 of SSA per 10,000 people, the range of our analyses, yields about a 97-percent reduction in terrorist activity in states with active PKOs. Over the range of our analyses, the predicted number of terrorist attacks drops from roughly 1.4 per year to about 0.04 per year. Similarly, the predicted number of casualties from terrorist attacks drops from about 0.5

Figure B.5
Predicted Number of Casualties of Terrorist Attacks as Security Sector Assistance Increases Among States with Active Peacekeeping Operations in the Post–Cold War Period



SOURCES: National Consortium for the Study of Terrorism and Responses to Terrorism, 2017; UN Peacekeeping, undated.
NOTE: The figure corresponds to model 5 in Table B.7. All other continuous variables are held at their mean values, and all other dichotomous variables are held at their modal values.
RAND *RR2447-B.5*

to about 0.03 per year, indicating that SSA is associated with a decline in both the frequency and lethality of terrorist attacks in partner states.

In line with expectations from the broader literature on terrorism is that increasing per capita wealth and foreign developmental assistance is associated with decreased levels of terrorist activity. In contrast, in our statistical models, having large excluded or disenfranchised populations, along with ongoing civil wars, significantly increases levels of terrorist activity.

Although our main statistical models suggest that the combination of SSA and PKOs attenuate levels of terrorist activity among

partner states, U.S. focus centers primarily on preventing transnational terrorism. As a result, it is important to gauge whether the results of our main models are driven by SSA's impact on domestic or transnational terrorist activity. Along those lines, Table B.8 and Table B.9 further examine SSA's effects on levels of terrorist activity by separately examining SSA's effects on domestic and transnational terrorism, respectively.[37]

Interestingly, these separated models suggest that U.S. assistance to African partners, in conjunction with ongoing PKOs, is associated with more-significant declines in transnational terrorist activity than domestic. Although there are certainly more domestic terrorist attacks than transnational attacks in Africa, these results are heartening given the United States' operational focus on preventing transnational terrorism. As with our main statistical models, however, these results rely on comparatively few cases of PKOs, meaning that, although the results are informative of a potentially broader and important impact for U.S. foreign policy, more-detailed analyses should be conducted to verify our findings.

Given the ongoing scholarly debate about the appropriateness of including lagged dependent variables in regression models, we also estimated a statistical model of terrorist activity that did not include the number of terrorist attacks in the previous year, referenced in Table B.10.[38]

SSA's effects in this alternative model vary slightly from those in our main statistical models. Overall, the findings of our main statistical models, that the conjunction of SSA and PKOs significantly lowers levels of terrorist activity, are somewhat attenuated and are not statistically significant for the post–Cold War period. Overall, however, these models suggest that SSA still provides a notable benefit in support of ongoing PKOs to suppressing terrorism. In addition, when we do not

[37] To separate domestic and transnational terrorist attacks, we use the GTD's coding for whether a terrorist had to cross a state border to commit an act of terrorism.

[38] Luke Keele and Nathan J. Kelly, "Dynamic Models for Dynamic Theories: The Ins and Outs of Lagged Dependent Variables," *Political Analysis*, Vol. 14, No. 2, Spring 2006, pp. 186–205.

Table B.8
The Effects That Security Sector Assistance Has on Numbers of Domestic Terrorist Attacks in African States

Variable	Model 1: Domestic Terrorist Attacks	Model 2: Cold War	Model 3: Post–Cold War	Model 4: Ongoing PKO	Model 5: Ongoing PKO Post–Cold War
Total SSA per population, three-year moving average and four-year lag	−0.068	−0.104**	−0.084	−0.040	−0.062
	(0.048)	(0.047)	(0.077)	(0.043)	(0.075)
Ongoing UN PKO, one-year lag	0.406	0.162	−0.135	0.074	−0.251
	(0.437)	(0.780)	(0.549)	(0.463)	(0.594)
Ongoing UN PKO, four-year lag				3.537**	3.055*
				(1.634)	(1.696)
Total SSA per population, three-year moving average × ongoing PKO four years prior				−0.382	−0.340
				(0.255)	(0.276)
GDP per capita	−0.609	0.129	−2.136***	−0.066	−1.647***
	(0.452)	(0.614)	(0.476)	(0.413)	(0.457)
Polity level	0.210**	0.393**	0.115	0.231**	0.108
	(0.099)	(0.187)	(0.166)	(0.096)	(0.160)
Polity level, squared	−0.009*	−0.018*	−0.006	−0.010**	−0.006
	(0.005)	(0.011)	(0.008)	(0.005)	(0.008)

Table B.8—Continued

Variable	Model 1: Domestic Terrorist Attacks	Model 2: Cold War	Model 3: Post–Cold War	Model 4: Ongoing PKO	Model 5: Ongoing PKO Post–Cold War
State bureaucratic capacity	0.718	0.347	0.022	0.514	–0.096
	(0.452)	(0.445)	(0.584)	(0.395)	(0.531)
Percentage of GDP from OECD ODA	–0.432**	0.227	–0.848***	–0.382**	–0.738***
	(0.181)	(0.232)	(0.261)	(0.170)	(0.234)
Excluded-population size	0.132	0.110	0.741	0.279	0.970
	(0.528)	(0.781)	(0.720)	(0.515)	(0.662)
Youth bulge	0.282	1.343	–0.558	0.653	–0.301
	(0.644)	(1.060)	(0.941)	(0.700)	(0.832)
Military spending per service member	0.059	0.012	0.375*	0.087	0.442**
	(0.143)	(0.101)	(0.212)	(0.161)	(0.200)
Ongoing civil war	2.473***	2.508***	2.719***	2.565***	2.796***
	(0.439)	(0.399)	(0.521)	(0.430)	(0.521)
Francophone Africa	–0.961***	–2.436***	–0.483	–0.874**	–0.293
	(0.358)	(0.475)	(0.436)	(0.355)	(0.427)

Table B.8—Continued

Variable	Model 1: Domestic Terrorist Attacks	Model 2: Cold War	Model 3: Post–Cold War	Model 4: Ongoing PKO	Model 5: Ongoing PKO Post–Cold War
Cold War	–1.174***			–0.993***	
	(0.352)			(0.328)	
Post-9/11	–1.227***		–1.276***	–1.346***	–1.296***
	(0.296)		(0.396)	(0.296)	(0.405)
Domestic terrorist attack in the previous year	0.101	0.186**	0.087*	0.098	0.086
	(0.071)	(0.086)	(0.050)	(0.074)	(0.053)
Constant	2.720	–6.347	14.165***	–2.307	9.074**
	(4.168)	(5.156)	(5.402)	(3.623)	(4.565)
Observations	1,635	719	916	1,635	916

NOTE: Cells report beta coefficients with robust standard errors clustered by country in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. We weighted observations by propensity scores estimated in the first-stage model.

Table B.9
The Effects That Security Sector Assistance Has on Numbers of Transnational Terrorist Attacks in African States

Variable	Model 1: Transnational Terrorist Attacks	Model 2: Post–Cold War	Model 3: Ongoing PKO	Model 4: Ongoing PKO Post–Cold War
Total SSA per population, three-year moving average and four-year lag	0.054	0.098	0.070**	0.121
	(0.034)	(0.085)	(0.035)	(0.091)
Ongoing UN PKO, one-year lag	1.676***	1.113	1.440**	1.179
	(0.644)	(0.887)	(0.612)	(0.914)
Ongoing UN PKO, four-year lag			1.201	0.830
			(1.035)	(1.337)
Total SSA per population, three-year moving average × ongoing PKO four years prior			−0.637***	−0.749***
			(0.152)	(0.188)
GDP per capita	−0.665*	−0.668	−0.612	−0.597
	(0.374)	(0.479)	(0.374)	(0.470)
Polity level	0.150	0.472*	0.151	0.471*
	(0.118)	(0.250)	(0.119)	(0.257)
Polity level, squared	−0.010	−0.026**	−0.010	−0.025**
	(0.006)	(0.012)	(0.006)	(0.012)

Table B.9—Continued

Variable	Model 1: Transnational Terrorist Attacks	Model 2: Post–Cold War	Model 3: Ongoing PKO	Model 4: Ongoing PKO Post–Cold War
State bureaucratic capacity	0.365	0.089	0.312	0.062
	(0.378)	(0.670)	(0.378)	(0.678)
Percentage of GDP from OECD ODA	–0.203	–0.091	–0.221	–0.080
	(0.223)	(0.285)	(0.221)	(0.288)
Excluded-population size	0.707	1.046	0.611	1.069
	(0.669)	(0.704)	(0.667)	(0.687)
Youth bulge	0.862	0.632	0.923	0.627
	(0.638)	(0.721)	(0.661)	(0.711)
Military spending per service member	0.704***	1.034*	0.704***	0.996*
	(0.227)	(0.550)	(0.236)	(0.563)
Ongoing civil war	0.751**	1.410***	0.834**	1.501***
	(0.368)	(0.489)	(0.358)	(0.517)
Francophone Africa	–0.444	–0.464	–0.441	–0.472
	(0.381)	(0.432)	(0.374)	(0.433)

Table B.9—Continued

Variable	Model 1: Transnational Terrorist Attacks	Model 2: Post–Cold War	Model 3: Ongoing PKO	Model 4: Ongoing PKO Post–Cold War
Cold War	–0.210		–0.180	
	(0.435)		(0.440)	
Post-9/11	–0.985**	–1.265**	–1.016**	–1.259**
	(0.472)	(0.602)	(0.471)	(0.614)
Transnational terrorist attack in the previous year	0.686***	0.752***	0.757***	0.825***
	(0.176)	(0.219)	(0.170)	(0.212)
Constant	–5.080	–9.551**	–5.529	–9.916**
	(3.716)	(4.701)	(3.718)	(4.878)
Observations	1,635	916	1,635	916

NOTE: Cells report beta coefficients with robust standard errors clustered by country in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. We weighted observations by propensity scores estimated in the first-stage model.

Table B.10
Effects of Security Sector Assistance on Numbers of Terrorist Attacks in African States, No Lagged Dependent Variable

Variable	Model 1: Terrorist Attacks	Model 2: Cold War	Model 3: Post–Cold War	Model 4: Ongoing PKO	Model 5: Ongoing PKO Post–Cold War
Total SSA per population, three-year moving average and four-year lag	−0.043	−0.066*	−0.038	−0.028	−0.017
	(0.041)	(0.036)	(0.059)	(0.041)	(0.062)
Ongoing UN PKO, one-year lag	0.740*	0.895**	0.359	0.270	0.103
	(0.394)	(0.431)	(0.451)	(0.433)	(0.423)
Ongoing UN PKO, four-year lag				1.904**	1.737*
				(0.878)	(1.034)
Total SSA per population, three-year moving average × ongoing PKO four years prior				−0.197*	−0.229
				(0.115)	(0.156)
GDP per capita	−0.428	0.456	−1.540***	−0.267	−1.389***
	(0.325)	(0.406)	(0.396)	(0.326)	(0.413)
Polity level	0.108	−0.071	0.217*	0.108	0.196
	(0.089)	(0.181)	(0.129)	(0.091)	(0.127)
Polity level, squared	−0.004	0.006	−0.010	−0.004	−0.009

Table B.10—Continued

Variable	Model 1: Terrorist Attacks	Model 2: Cold War	Model 3: Post–Cold War	Model 4: Ongoing PKO	Model 5: Ongoing PKO Post–Cold War
	(0.004)	(0.010)	(0.007)	(0.004)	(0.007)
State bureaucratic capacity	0.641	0.617*	0.282	0.548	0.223
	(0.417)	(0.363)	(0.461)	(0.408)	(0.432)
Percentage of GDP from OECD ODA	–0.448***	0.157	–0.821***	–0.446***	–0.802***
	(0.151)	(0.180)	(0.205)	(0.145)	(0.205)
Excluded-population size	0.541	0.433	0.949*	0.549	1.090**
	(0.445)	(0.603)	(0.525)	(0.445)	(0.527)
Youth bulge	0.878	1.770***	–0.109	0.984*	–0.043
	(0.548)	(0.563)	(0.740)	(0.577)	(0.753)
Military spending per service member	0.166	0.186	0.326*	0.169	0.322*
	(0.126)	(0.123)	(0.171)	(0.131)	(0.175)
Ongoing civil war	2.359***	1.848***	2.780***	2.409***	2.860***
	(0.337)	(0.321)	(0.407)	(0.329)	(0.413)
Francophone Africa	–0.339	–1.537***	–0.269	–0.319	–0.238
	(0.222)	(0.339)	(0.336)	(0.217)	(0.347)

Table B.10—Continued

Variable	Model 1: Terrorist Attacks	Model 2: Cold War	Model 3: Post–Cold War	Model 4: Ongoing PKO	Model 5: Ongoing PKO Post–Cold War
Cold War	–1.599***			–1.550***	
	(0.331)			(0.317)	
Post-9/11	–1.280***		–1.386***	–1.357***	–1.414***
	(0.242)		(0.370)	(0.242)	(0.369)
Constant	1.618	–8.436**	10.329***	0.247	9.034***
	(2.962)	(3.553)	(3.230)	(2.836)	(3.199)
Observations	1,667	751	916	1,667	916

NOTE: Cells report beta coefficients with robust standard errors clustered by country in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. We weighted observations by propensity scores estimated in the first-stage model.

account for previous years' levels of terrorist violence, we see that U.S. assistance during the Cold War appears to have significantly lowered levels of terrorist activity, even as the presence of PKOs increased the risk of heightened terrorist activity.

Repression

Table B.11 details the results of our quantitative models examining the relationship between U.S. SSA and levels of violent repression utilized by African states against their populations. Model 1 analyzes this relationship in the period since 1945, while model 2 and model 3 examine this relationship in the Cold War and post–Cold War periods, respectively. To help us see any potential intervening effects that active PKOs have on SSA, model 4 examines the conditional effects that active UN PKOs have had on SSA's effects since 1945. Similarly, model 5 examines this conditional relationship in the post–Cold War period.[39]

In our models, we find little evidence that U.S. SSA significantly affects levels of violent repression utilized by African states, either in the Cold War or post–Cold War period. The coefficient on SSA is not statistically significant in any of the models, 1 through 5. In addition, we find no robust statistical evidence that the relationship between SSA and levels of state repression is significantly influenced by the conditioning effects of partner states' regime types or their bureaucratic or military capacities.

However, among states with active PKOs at the time of assistance, U.S. SSA is associated with a significant reduction in partner states' use of violent repression. This is true in both the entire period since 1945 and in the post–Cold War period, when most African PKOs have occurred. Our theoretical expectations assert that PKOs partly support SSA's goals through deep and repeated interactions between U.S. institutions and African partner governments. These repeated interactions, in turn, could provide greater oversight of how African partners imple-

[39] The results of our quantitative models using multiple imputation conform to the results of our main statistical models as reported in Table B.8.

Table B.11
The Effects That Security Sector Assistance Has on Levels of State Repression Among African States

Variable	Model 1: State Repression	Model 2: Cold War	Model 3: Post–Cold War	Model 4: Ongoing PKO	Model 5: Ongoing PKO Post–Cold War
Total SSA per military service member, three-year moving average and four-year lag	0.003	0.015	–0.017	0.007	–0.012
	(0.016)	(0.017)	(0.018)	(0.017)	(0.019)
Ongoing UN PKO, one-year lag	0.191	2.223***	0.340*	–0.056	0.024
	(0.206)	(0.426)	(0.184)	(0.344)	(0.302)
Ongoing UN PKO, four-year lag				0.812	1.073**
				(0.490)	(0.458)
Total SSA per military service member, three-year moving average × ongoing PKO four years prior				–0.120**	–0.129**
				(0.057)	(0.049)
GDP per capita	–0.475***	–0.528**	–0.332***	–0.474***	–0.325***
	(0.111)	(0.198)	(0.093)	(0.111)	(0.094)
Democracy	–0.230	–0.477***	–0.231	–0.233	–0.235
	(0.192)	(0.176)	(0.177)	(0.195)	(0.180)
Military regime	0.377**	–1.009*	1.063***	0.384**	1.089***
	(0.175)	(0.577)	(0.220)	(0.179)	(0.232)

Table B.11—Continued

Variable	Model 1: State Repression	Model 2: Cold War	Model 3: Post–Cold War	Model 4: Ongoing PKO	Model 5: Ongoing PKO Post–Cold War
Personalist regime	0.288*	–0.345	0.318*	0.289*	0.328*
	(0.159)	(0.497)	(0.173)	(0.161)	(0.177)
Single-party regime	0.210	–0.731	0.143	0.210	0.142
	(0.177)	(0.448)	(0.177)	(0.179)	(0.180)
Percentage of GDP from OECD ODA	–0.240***	–0.222*	–0.255***	–0.240***	–0.253***
	(0.070)	(0.120)	(0.081)	(0.070)	(0.083)
Ongoing civil war	0.539***	0.858**	0.536***	0.566***	0.559***
	(0.170)	(0.369)	(0.156)	(0.167)	(0.157)
Francophone Africa	–0.226	–0.383**	–0.161	–0.219	–0.154
	(0.149)	(0.181)	(0.146)	(0.151)	(0.147)
Cold War	–0.237			–0.229	
	(0.154)			(0.154)	
Post-9/11	0.131		0.182*	0.139	0.183*
	(0.130)		(0.108)	(0.130)	(0.109)

Table B.11—Continued

Variable	Model 1: State Repression	Model 2: Cold War	Model 3: Post–Cold War	Model 4: Ongoing PKO	Model 5: Ongoing PKO Post–Cold War
State repression in the previous year	0.573***	0.502***	0.558***	0.569***	0.552***
	(0.043)	(0.079)	(0.039)	(0.045)	(0.039)
Constant	2.235**	2.742	0.941	2.167**	0.814
	(0.981)	(1.737)	(0.918)	(0.987)	(0.926)
Observations	1,260	326	934	1,260	934
R-squared	0.543	0.553	0.562	0.544	0.565

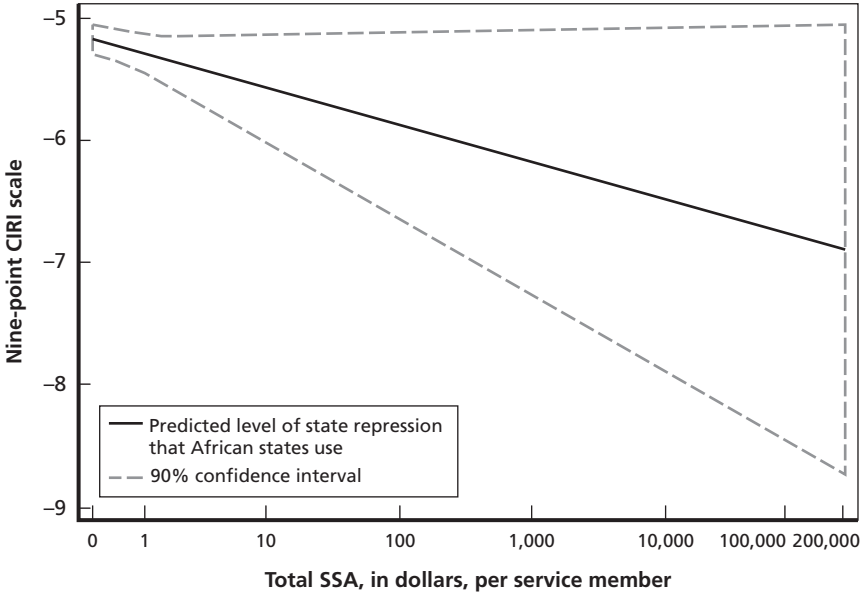
NOTE: Cells report beta coefficients with robust standard errors clustered by country in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. We weighted observations by propensity scores estimated in the first-stage model.

ment SSA funds, subsequently mitigating the risk that those funds will be used against partner-state populations.

To visualize these effects, Figure B.6 plots the predicted level of state repression (as measured on the nine-point CIRI scale) as SSA levels increase among African states with active PKOs during the post–Cold War period. The solid black line in Figure B.6 shows the predicted level of state repression that African states use as levels of SSA increase. As shown in the figure, levels of repression decline as the number of deployed peacekeepers increases. The dashed gray lines in Figure B.6 represent the upper and lower bounds of the 90-percent confidence interval around this prediction.

Also in line with our expectations is that several control variables significantly affect levels of state repression among African states. Not

Figure B.6
Predicted Level of State Repression as Security Sector Assistance Increases Among States with Active Peacekeeping Operations in the Post–Cold War Period



SOURCES: Cingranelli, Richards, and Clay, 2014; UN Peacekeeping, undated.

surprisingly, more-affluent and more-democratic states, along with states receiving greater developmental assistance, display a greater respect for human rights. In contrast, autocratic regimes, particularly military and personalist regimes, are more likely to control their populations through violence. Similarly, states experiencing violent civil wars are more likely to repress their populations as regimes attempt to utilize violence to maintain order within their borders.

APPENDIX C

A Deep Dive on Security Sector Assistance and Coups

In this appendix, we conduct an analysis of the relationship between U.S. SSA and coup propensity in partner states. We have various reasons to suspect that SSA might affect the likelihood of military coups in partner states. Through SSA, the United States can play a prominent role in developing a partner state's military norms and respect for civilian authority. Alternatively, this relationship can engender an elite class of well-trained and equipped officers who seek regime change out of frustration with their civilian leadership. But SSA's overall effects on these domestic conflict dynamics are not immediately obvious.

To better understand these dynamics, we conducted a statistical analysis of the relationship between SSA and coups. Parts of this analysis draw on similar data and empirical strategies to those in our analysis of the relationships between SSA and civil wars, terrorism, and state repression (see Appendix B for a fuller discussion on sources and models). Some of our tests, however, use measures of U.S.-provided SSA that differ from those used in our other analyses. Given the differences in approach and the potential for confusion that these differences might create, we elected to separate out our analysis of coups and place it in this stand-alone appendix.

Unfortunately, our statistical models offer little conclusive evidence on this topic. Across various models and measures of SSA, we find no robust association with coup propensity. Informed by our analysis, we do not find sufficient evidence to conclude that SSA increases or decreases the likelihood of coups in Africa. This null finding is especially unsatisfying in light of recent attention on the relation-

ship between SSA and military coups.[1] It nonetheless might help to advance quantitative analyses in this field.

The remainder of this appendix proceeds as follows. In the next section, we briefly discuss recent studies on SSA and coups, which have garnered some attention from policymakers. Having discussed these studies and their implications, we then proceed to our empirical models. We consider several different measures of SSA, which not only have important consequences for modeling strategy but can also dramatically shape the results.

Theories on Security Sector Assistance and Coups

New quantitative studies focusing on various education and training programs have reached conflicting results, reigniting scholarly interest and leading policymakers to question the value of some long-heralded programs, IMET in particular. In Chapter Three, we described some of the reasons that SSA, in general, might be associated with coup propensity, but this new debate focuses specifically on the effects of training and education. As a result, it is worth briefly exploring the possible mechanisms, positive and negative, that underlie this specific relationship.

In terms of positive effects, U.S. training programs might improve a partner nation's civil–military relations through several mechanisms. The first mechanism is socialization. The U.S. military has internalized the norm of civilian supremacy and other norms of democratic civil–military relations. By interacting with the U.S. armed services, partner nations might begin to internalize these values. The second mechanism is a more direct program of professionalization. During their participation in IMET courses, participants are taught U.S. best practices for civil–military relations. A third mechanism is sustainment. By providing its forces with training through a U.S. program,

[1] See especially Jesse Dillon Savage and Jonathan D. Caverley, "When Human Capital Threatens the Capitol: Foreign Aid in the Form of Military Training and Coups," *Journal of Peace Research*, Vol. 54, No. 4, 2017, pp. 542–557

the partner nation's government demonstrates an interest in investing in the military. Moreover, if U.S. training helps a partner nation improve its human resources and sustainment practices (e.g., paying soldiers fully and on time, providing decent facilities in which soldiers can live and work, providing the equipment necessary to fight), morale among partner-nation forces might improve and incentives for mutiny or coups could diminish. Finally, these training courses can serve as a U.S. signaling device. By investing in the partner nation's military and encouraging the adoption of Western norms of civil–military relations, the United States can more credibly signal its interest in preventing military coups. Thus, a potential coup plotter knows that he or she could risk the country's relationship with the United States and the aid that comes with it.

Debates about the effects that U.S. military assistance has on foreign officers—and statistical analyses of those effects—are several decades old.[2] But these debates have been reignited in recent years by a pair of articles making use of improved data. Tomislav Ruby and Douglas Gibler fall on one side of this debate, arguing for the potential benefits of U.S. education and training.[3] Jesse Savage and Jonathan Caverley, on the other hand, find that such programs (IMET and CTFP in particular) increase the likelihood that foreign military officers will attempt to conduct a coup.[4] Savage and Caverley's work originally appeared in an academic journal but has subsequently appeared in opinion pieces in popular media.[5]

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[2] See, for instance, T. Y. Wang, "Arms Transfers and Coups d'État: A Study on Sub-Saharan Africa," *Journal of Peace Research*, Vol. 35, No. 6, 1998, pp. 659–675.

[3] Tomislav Z. Ruby and Douglas Gibler, "US Professional Military Education and Democratization Abroad," *European Journal of International Relations*, Vol. 16, No. 3, 2010, pp. 339–364.

[4] Savage and Caverley, 2017, pp. 545–546.

[5] Their work was cited, for instance, in opinion pieces in *U.S. News and World Report* and *Salon*. See William D. Hartung, "Military Aid Should Do No Harm: The Failures of U.S. Military Assistance Programs Far Outpace the Successes," *U.S. News and World Report*, March 8, 2016, and Nick Turse, "Can the Pentagon Win When Putsch Comes to Shove? A Rare Pentagon 'Success' Story," *Salon*, August 11, 2017.

The divergence in the Ruby and Gibler article's and the Savage and Caverley article's findings appear to derive largely from the different data sources used in the two analyses. Ruby and Gibler studied partner nations' participants in U.S. PME programs and find that participation in these programs is associated with a decreased incidence of military coups. Their work is subject to criticism, however, in that the PME programs usually accept only people in the top ranks of the armed forces, while coup leaders often come from lower positions. Furthermore, their analysis depends on an original data set of coup attempts, which departs from the standards found in existing work.

Savage and Caverley, on the other hand, draw on a global data set that consists of 189 countries from 1970 to 2009. They control for many of the standard factors that can affect the risk of coups, including regime characteristics, economic development, and ongoing civil war.[6] The relationship between student attendance and coups appears robust to the addition of various controls and across different time periods.

Although Savage and Caverley's results are particularly robust, their findings also seem to rely on data that might be problematic. We discuss these challenges in greater depth in the next section. But we focus here on one major challenge: conceptual validity and measurement. Despite measuring IMET participation in a variety of ways, Savage and Caverley did not consider the qualitative variation in IMET course curricula. For example, some classes teach special operatives training or strategic decisionmaking, which can certainly provide useful skills for staging a coup. Other courses, however, do not seem to have ready application for would-be coup plotters, such as optometrist training or disease prevention. Savage and Caverley's series of measures for IMET participation cannot distinguish between these different kinds of courses. There is also significant variation in the length of IMET courses, with more than one-fifth of these courses lasting less than five days (a period likely too short to provide new skills or motivation to conduct a coup), while others can be several months long.

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[6] Joseph Wright, Erica Frantz, and Barbara Geddes, "Oil and Autocratic Regime Survival," *British Journal of Political Science*, Vol. 45, No. 2, April 2015, pp. 287–306; N. Gleditsch et al., 2002.

Courses that last longer presumably have a much greater impact than short seminars. This variation is important to consider when evaluating the impact that IMET courses have on coup propensity.

Although we cannot correct for the problem of course relevance, we can adjust our measure of IMET participation to better capture varying lengths. At least for Africa, Savage and Caverley's results depend on IMET participation measured in terms of the number of students who participated and the dollars the United States expended. We instead multiply the number of students participating in IMET by the average length of IMET courses for those students to create a new measure called *student-days*, which seems to better capture the causal logic of Savage and Caverley's argument.[7] If we use the number of student-days per country as the main explanatory variable, instead of the number of students or dollars spent per year, the relationship between IMET participation and coup occurrence disappears in the Africa sample.

Models and Results

Our variable for coup attempts is simply a dichotomous indicator of whether a military-backed coup attempt occurred in an African state in a given year, drawn from the Global Instances of Coups data set.[8] This measure tracks both successful and failed coups that occurred in each African state but does not record any alleged or unrealized coups that might have been planned but not executed.[9] Importantly, the provision of SSA can alter the balance of power within the state military apparatus, thus affecting potential coup plotters' calculus about whether or not to undertake a coup. As such, we use yearly coup attempts as our

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[7] Because of data limitations on IMET, we limited our analysis of this connection to Africa from 2002 to 2013.

[8] Jonathan M. Powell and Clayton L. Thyne, "Global Instances of Coups from 1950 to 2010: A New Dataset," *Journal of Peace Research*, Vol. 48, No. 2, 2011, pp. 249–259.

[9] Military-backed coup attempts occurred in 179 out of the 2,735 country-years of our data set. Interestingly, of those 179 coup attempts, 90 were successful and 89 resulted in failure for the coup plotters.

main dependent variable. If SSA makes military officers better coup plotters, however, the effects of SSA should also be evident in the rate of successful coups among African militaries. To capture these possible effects, we use a second dependent variable that indicates whether a successful military-backed coup occurred in a given year, also drawn from the Global Instances of Coups data set.

Table C.1 reports the results from our models on SSA and coup propensity in African states. These models depart from those in the pre-

Table C.1
Effects of Aggregate Security Sector Assistance Expenditures on Coup Propensity in African States

Variable	Model 1: Coup Attempts	Model 2: Cold War	Model 3: Post–Cold War
Total SSA per military service member, three-year moving average and four-year lag	−0.007	0.046	0.213*
	(0.041)	(0.071)	(0.115)
Ongoing UN PKO, one-year lag	−0.648		−0.700
	(0.641)		(0.797)
GDP per capita	−0.945***	−0.708*	−1.833***
	(0.345)	(0.426)	(0.565)
Military regime	0.728*	0.366	1.830**
	(0.398)	(0.452)	(0.885)
Democracy	0.354	−0.824	0.618
	(0.296)	(0.981)	(0.418)
Percentage of GDP from OECD ODA	−0.090	−0.099	−0.261
	(0.186)	(0.193)	(0.265)
Military spending per service member	−0.276	−0.267	−0.668***
	(0.192)	(0.329)	(0.197)
Number of parallel military institutions	0.134	−0.433	−0.370
	(0.205)	(0.370)	(0.332)
Ongoing civil war	0.143	0.791***	−0.203

Table C.1—Continued

Variable	Model 1: Coup Attempts	Model 2: Cold War	Model 3: Post–Cold War
	(0.369)	(0.277)	(0.532)
Previous coup attempt	0.570	–0.358	2.995**
	(0.548)	(0.788)	(1.316)
Francophone Africa	0.221	0.146	0.565
	(0.322)	(0.448)	(0.520)
Cold War	0.272		
	(0.372)		
Post-9/11	0.355		–0.055
	(0.503)		(0.540)
Constant	7.423***	7.117	14.385***
	(2.661)	(4.996)	(4.091)
Observations	1,416	565	850

NOTE: Cells report beta coefficients with robust standard errors clustered by country in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. We weighted observations by propensity scores estimated in the first-stage model.

vious sections in a couple of ways. First, the table reports results from only three model specifications. Like before, model 1 represents the baseline estimation for the entire post–World War II period. Model 2 and model 3 then evaluate the relationship between SSA and coup propensity in the Cold War and post–Cold War periods, respectively. Notably absent, however, are any interaction terms. Recall that there are only 122 country-years of PKOs. For the other outcome measures, this small number is not terribly problematic: There are enough terrorist attacks and varying levels of repression across countries with (and without) ongoing peacekeeping mission to allow for robust estimation of interaction terms. Even civil wars are frequent enough that the results are not particularly sensitive to any one case. But coup attempts are sufficiently rare that the small number of PKOs makes any results

from an interaction model extremely fragile.[10] As such, we focus our analysis exclusively on models without interaction terms.

Second, the SSA variable is scaled differently in the coup models. For civil wars and terrorism, we scaled the SSA measure by population. This choice represents our theoretical expectations on how SSA affects these violent outcomes. SSA's effects on civil wars, terrorist attacks, or even the degree of repression should vary with population size; $1 million of SSA expenditures would be a substantial amount in tiny Comoros, for instance, but should not be expected to make much difference in a country that, like Nigeria, has a large population. For coups, however, population might not be the most appropriate scaling variable. After all, coups result from an altogether distinct set of possible mechanisms, which depend on the strength and organization of small groups of military personnel rather than any dynamic involving population size. Although the population can, under certain conditions, play some role in the success of a coup, it is hard to justify why this variable should scale SSA's potential impact on coup *attempts*. Instead, we scale SSA by the partner nation's military personnel. Many of the theoretical mechanisms linking SSA to coup propensity—either positively or negatively—are more directly tied to the overall size of the military. If, for example, SSA decreases coups by inculcating a set of norms surrounding civil–military relations, we should expect this effect to vary with the relative size of the military. Norm diffusion and similar mechanisms should be most effective when SSA funds are spread throughout a military, making the number of personnel an important scaling variable.

As Table C.1 reveals, there is little evidence that overall SSA (measured in dollar terms) associates with coup propensity in Africa. In model 1, which considers all country-years, the coefficient on SSA is negative but highly insignificant. In the Cold War period, this coefficient becomes positive but remains statistically indistinguishable from zero. Only in the post–Cold War period, model 3, do we find a marginally significant result (at the 0.10 level). In the specification,

[10] Given the fact that some data are missing, under some specifications, there are only two coup attempts during a peacekeeping mission and only one successful coup during a mission.

the coefficient is positive, suggesting that SSA is associated with an increase in coup propensity. Notwithstanding this marginal result, the table offers weak evidence of any systematic relationship between total SSA and the likelihood of coup attempts.

Nor does the table report particularly robust results across the control variables. Among the battery of controls we include, only GDP per capita appears robust across all three specifications. The negative coefficient is consistent with the conventional wisdom that coup attempts are less likely in wealthier states. The period-specific models offer some potentially interesting results, but these findings should be interpreted with caution given their fragility. In the Cold War period, coups attempts are more likely in states experiencing violent civil wars, which potentially represent an opportunity for would-be coup plotters. This result does not hold for the post–Cold War period. But model 3 does suggest that, in this latter period, coup attempts were more likely in military regimes and countries that had previously experienced a coup. The negative coefficient on military spending also suggests that coups were less likely when militaries enjoyed ample resources.

But this analysis considers only aggregated SSA's effects on coups. The relationship between SSA and coups has recently garnered increased attention with the work of Savage and Caverley, who argued that U.S. military assistance in the form of IMET and CTFP courses causes military coups.[11] This finding has attracted significant interest from policymakers, especially those who have championed these programs in the past. Given the somewhat alarming implications of this result, we replicated the coup models above but separated out IMET funding from other forms of SSA. Table C.2 shows the results from these estimations. These models are identical to those in Table C.1 except that we have replaced total SSA with separate variables for IMET and non-IMET assistance. As before, both of these variables are inverse hyperbolic sine–transformed, three-year moving averages, scaled by military personnel and lagged four years. The results for these models are even less significant than those using aggregated SSA. The coefficient on the IMET variable looks qualitatively similar to that on

[11] Savage and Caverley, 2017, pp. 542–557.

Table C.2
Effects That International Military Education and Training Expenditures Have on Coup Propensity in African States

Variable	Model 1: Coup Attempts	Model 2: Cold War	Model 3: Post–Cold War
Total IMET expenditure per service member, three-year moving average and four-year lag	–0.056	0.087	0.132
	(0.083)	(0.164)	(0.134)
Total non-IMET expenditure per service member, three-year moving average and four-year lag	0.030	–0.014	0.108
	(0.076)	(0.109)	(0.152)
Ongoing UN PKO, one-year lag	–0.694		–0.638
	(0.642)		(0.756)
GDP per capita	–0.948***	–0.698	–1.782***
	(0.349)	(0.435)	(0.541)
Military regime	0.747*	0.354	1.856*
	(0.413)	(0.441)	(0.988)
Democracy	0.377	–0.812	0.625
	(0.313)	(0.983)	(0.404)
Percentage of GDP from OECD ODA	–0.081	–0.098	–0.215
	(0.183)	(0.192)	(0.237)
Military spending per service member	–0.259	–0.264	–0.654***
	(0.200)	(0.330)	(0.195)
Number of parallel military institutions	0.151	–0.465	–0.351
	(0.204)	(0.400)	(0.337)
Ongoing civil war	0.122	0.832***	–0.239
	(0.394)	(0.322)	(0.633)
Previous coup attempt	0.599	–0.396	2.982**
	(0.532)	(0.808)	(1.305)
Francophone Africa	0.218	0.169	0.575
	(0.334)	(0.401)	(0.545)

Table C.2—Continued

Variable	Model 1: Coup Attempts	Model 2: Cold War	Model 3: Post–Cold War
Cold War	0.218		
	(0.353)		
Post-9/11	0.326		–0.057
	(0.480)		(0.548)
Constant	7.308***	7.033	13.960***
	(2.595)	(5.018)	(3.875)
Observations	1,416	565	850

NOTE: Cells report beta coefficients with robust standard errors clustered by country in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. We weighted observations by propensity scores estimated in the first-stage model.

the SSA variable in Table C.1—it is negative in the pooled model but positive in each of the period-specific models. However, none of these coefficients is remotely significant, suggesting no relationship between IMET and coup propensity in Africa. The sign on the coefficient for non-IMET funds also jumps around and is never significant. All told, the results provide little indication that IMET or non-IMET funds have any strong relationship with coups.

These results markedly deviate from those of Savage and Caverley. Although we do not reach the same conclusions as Savage and Caverley, we should note that there are several major differences between our analysis and theirs. First, our models are specific to Africa, while they included 189 countries in their analysis. Similarly, the time frame differs somewhat. Savage and Caverley drew on a shorter time period, beginning later (1970) and ending earlier (2009) than our study. Second, our measure of IMET, like that of aggregated SSA, is a moving average and scaled by numbers of military personnel, while their measure was neither averaged nor scaled. Generally, though, we do not find that our results qualitatively vary with different scaling choices, and this difference does not likely drive the conflicting results. Perhaps more importantly, Savage and Caverley did not use a four-year lag like

we did with SSA. If IMET or other programs are to have any effect at all, whether positive or negative, we do not expect these effects to be immediate, especially if they depend on such slow-moving processes as norm diffusion, promotion to more-influential roles within the military, or recruiting and organizing coconspirators.

Notwithstanding these differences, the Savage and Caverley article has attracted sufficient attention in some policy circles that we felt that it deserved a deeper dive. To fully unpack these results, we began by replicating the article's findings. The authors were generous enough to share their data, making this initial replication fairly straightforward. Savage and Caverley's data set consists of data on 189 countries from 1970 to 2009. Their main independent variable is a country's involvement in IMET and CTFP, which is measured in several different ways, including (1) using a dummy variable to indicate whether a country had any students in an IMET or CTFP program, (2) using the log of the number of IMET students from that country in the previous year, and (3) the log of the number of IMET students from that country in the past five years combined. Like in the first two methods, the authors also constructed logged variables for IMET and CTFP funding from the past year and previous five years. It is these spending variables that most closely resemble our measures.

Savage and Caverley used a standard set of control variables for testing military coups, all of which were lagged. They used the log of GDP per capita and economic growth.[12] Developing countries are generally more likely to undergo military coups, as are countries with shrinking economies. Savage and Caverley also used oil revenue because there are arguments that oil affects regime stability, civil–military relations, and U.S. strategic interest.[13] They similarly captured U.S. strategic interest by controlling for UN voting affinity with the United

[12] World Bank, 2012.

[13] Stephen Haber and Victor Menaldo, "Do Natural Resources Fuel Authoritarianism? A Reappraisal of the Resource Curse," *American Political Science Review*, Vol. 105, No. 1, February 2011, pp. 1–26.

States.[14] They included non-IMET assistance (as a percentage of GDP) and other forms of aid that regimes can use to stay in power.[15] To isolate the effects of spending on foreign military training from other defense-related investments by the state, the authors also included defense spending per soldier, which is likely to be strongly related to both levels of foreign military training and the number of military personnel in the state.[16] To capture institutional coup proofing, they included a measure for the degree of fractionalization within the ground forces.[17] For different regime characteristics, they included a dummy variable of democracy because the model for transmission of norms should work best in a democratic setting and roughly capture stability through regime age.[18] They controlled for empowered ethnic groups because elites from these groups could launch military coups.[19] To account for time dependence, they also control for the years since the last coup and include square and cubed terms.[20] Finally, they included post–Cold War and post-9/11 dummy variables to control for any possible period

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[14] Erik Gartzke, *The Affinity of Nations Index, 1946–2002*, version 4.0, New York: Columbia University, March 10, 2006.

[15] Michael J. Tierney, Daniel L. Nielson, Darren G. Hawkins, J. Timmons Roberts, Michael G. Findley, Ryan M. Powers, Bradley Parks, Sven E. Wilson, and Robert L. Hicks, "More Dollars Than Sense: Refining Our Knowledge of Development Finance Using AidData," *World Development*, Vol. 39, No. 11, November 2011, pp. 1891–1906.

[16] J. David Singer, "Reconstructing the Correlates of War Dataset on Material Capabilities of States, 1816–1985," *International Interactions*, Vol. 14, No. 2, 1988, pp. 115–132; Powell, 2012.

[17] Pilster and Böhmelt, 2011.

[18] Carles Boix, Michael Miller, and Sebastian Rosato, "A Complete Data Set of Political Regimes, 1800–2007," *Comparative Political Studies*, Vol. 46, No. 12, 2013, pp. 1523–1554.

[19] Wimmer, Cederman, and Min, 2009.

[20] Philip Roessler, "The Enemy Within: Personal Rule, Coups, and Civil War in Africa," *World Politics*, Vol. 63, No. 2, April 2011, pp. 300–346; Powell, 2012; David B. Carter and Curtis S. Signorino, "Back to the Future: Modeling Time Dependence in Binary Data," *Political Analysis*, Vol. 18, No. 3, Summer 2010, pp. 271–292.

effects. And to address missing-data problems, they imputed observations using the *amelia* program implemented in R.[21]

Table C.3 reports Savage and Caverley's results for Africa. Models 1, 2, and 3 use the student variables to capture participation in IMET and CTFP; in Table C.4, models 4, 5, and 6 use the spending variables. Overall, we find that the results are significantly attenuated for the African subsample.

Table C.3
Savage and Caverley's Coup Propensity Results for an African Subsample: International Military Education and Training Participation, in Numbers of Students

Variable	Model 1: Coup Attempts, Any Students	Model 2: Coup Attempts, Total Students, Annual	Model 3: Coup Attempts, Total Students, Five-Year Sums
Any students	0.35*		
	(0.201)		
Total students, annual		0.065*	
		(0.038)	
Total students, five-year sums			0.0712*
			(0.0373)
GDP per capita	–0.415***	–0.416***	–0.349***
	(0.148)	(0.038)	(0.167)
U.S. affinity	–0.004	0.019	0.29
	(0.273)	(0.272)	(0.329)
Percentage of GDP from military assistance	–17.80	–18.5	–25.0
	(18.40)	(18.6)	(23.2)
Military spending	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)

[21] James Honaker and Gary King, "What to Do About Missing Values in Time-Series Cross-Section Data," *American Journal of Political Science*, Vol. 54, No. 2, April 2010, pp. 561–581.

Table C.3—Continued

Variable	Model 1: Coup Attempts, Any Students	Model 2: Coup Attempts, Total Students, Annual	Model 3: Coup Attempts, Total Students, Five-Year Sums
Military personnel	−0.000	−0.000	−0.000
	(0.000)	(0.002)	(0.002)
Oil revenue	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Coup proofing	−0.071	−0.073	−0.046
	(0.157)	(0.158)	(0.169)
Civil war	0.280	0.266	0.241
	(0.252)	(0.252)	(0.26)
Growth	−0.024*	−0.025*	−0.017
	(0.013)	(0.013)	(0.014)
Democracy	−0.188	−0.195	−0.244
	(0.312)	(0.313)	(0.318)
Regime age	−0.004	−0.005	−0.003
	(0.013)	(0.005)	(0.005)
Terror attack	−0.190	−0.189	−0.23
	(0.235)	(0.235)	(0.24)
Total aid	0.506	0.566	0.542
	(0.667)	(0.664)	(0.752)
Empowered ethnic group	−0.051	−0.052	−0.033
	(0.051)	(0.051)	(0.056)
Post–Cold War	−0.004	−0.016	−0.0493
	(0.233)	(0.232)	(0.241)
Post-9/11	−0.447	−0.464	−0.408
	(0.313)	(0.313)	(0.317)

Table C.3—Continued

Variable	Model 1: Coup Attempts, Any Students	Model 2: Coup Attempts, Total Students, Annual	Model 3: Coup Attempts, Total Students, Five-Year Sums
Years since coup	–0.147***	–0.151***	–0.182***
	(0.069)	(0.069)	(0.074)
Years since coup, squared	0.007	0.008	0.001
	(0.006)	(0.006)	(0.0069)
Years since coup, cubed	–0.00017	–0.00018	–0.000233
	(0.00015)	(0.00015)	(0.000169)
Constant	0.871	1.05	0.739
	(0.902)	(0.902)	(1.02)
Observations	1,994	1,994	1,735

NOTE: Cells report beta coefficients with robust standards errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.4
Savage and Caverley's Coup Propensity Results for an African Subsample: International Military Education and Training Expenditures

Variable	Model 4: Coup Attempts, Any Spending	Model 5: Coup Attempts, Total Annual Spending	Model 6: Coup Attempts, Total Spending
Any spending	0.267		
	(0.203)		
Total spending, annual		0.034	
		(0.026)	
Total spending, five-year sums			0.049*
			(0.027)
GDP per capita	–0.41***	–0.411***	–0.347**
	(0.147)	(0.147)	(0.167)

Table C.4—Continued

Variable	Model 4: Coup Attempts, Any Spending	Model 5: Coup Attempts, Total Annual Spending	Model 6: Coup Attempts, Total Spending
U.S. affinity	–0.028	0.041	0.281
	(0.274)	(0.272)	(0.329)
Percentage of GDP from military assistance	–17.5	–18	–25.2
	(18.3)	(18.5)	(23.2)
Military spending	–0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Military personnel	–0.000	–0.000	–0.002
	(0.002)	(0.002)	(0.002)
Oil revenue	–0.000	0.000	–0.000
	(0.000)	(0.000)	(0.000)
Coup proofing	–0.061	–0.064	–0.042
	(0.157)	(0.157)	(0.169)
Civil war	0.278	0.267	0.239
	(0.252)	(0.251)	(0.26)
Growth	–0.024*	–0.025*	–0.018
	(0.013)	(0.014)	(0.014)
Democracy	–0.172	–0.179	–0.232
	(0.312)	(0.313)	(0.317)
Regime age	–0.004	–0.004	–0.004
	(0.005)	(0.005)	(0.005)
Terror attack	–0.197	–0.198	–0.227
	(0.235)	(0.235)	(0.24)
Total aid	0.529	0.553	0.518
	(0.664)	(0.662)	(0.75)

Table C.4—Continued

Variable	Model 4: Coup Attempts, Any Spending	Model 5: Coup Attempts, Total Annual Spending	Model 6: Coup Attempts, Total Spending
Empowered ethnic group	–0.049	–0.05	–0.003
	(0.003)	(0.051)	(0.056)
Post–Cold War	0.0111	–0.021	–0.041
	(0.233)	(0.232)	(0.241)
Post-9/11	–0.456	–0.458	–0.387
	(0.313)	(0.313)	(0.317)
Years since coup	–0.15**	–0.15***	–0.181***
	(0.069)	(0.069)	(0.074)
Years since coup, squared	0.008	0.008	0.01188
	(0.007)	(0.00654)	(0.007)
Years since coup, cubed	–0.000179	–0.00018	–0.000232
	(0.000158)	(0.000159)	(0.00017)
Constant	0.85	0.946	0.657***
	(0.901)	(0.899)	(1.02)
Observations	1,994	1,994	1,735

NOTE: Cells report beta coefficients with robust standards errors in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

More concerning, however, is the comparison of the student and spending measures. Although both sets are robust for the global sample analyzed in Savage and Caverley's article, we find strong results in Africa only when using the student measures of IMET and CTFP participation. The coefficient on the dummy variable for any students is marginally significant, as is the one-year measure for total students who participated. Both coefficients are positive, suggesting that IMET or CTFP participation is associated with an increased risk of coup. This result remains positive and becomes much more significant in

model 3, which uses the five-year sum of IMET and CTFP students. These models certainly corroborate the findings from the global panel analysis and run counter to our null findings. However, we see results consistent with our models when turning to the spending measures of IMET and CTFP participation. The results are now largely insignificant. Neither the dummy variable for spending nor the one-year measure of IMET and CTFP funds is significant. Only in model 6, which uses the five-year sum of spending, do we find anything significant. The coefficient is positive and marginally significant at the 0.10 level.

Finally, model 1 in Table C.5 replaces these student and spending measures with our constructed measure for student-days. Given the high variation in course length, this measure represents a more appro-

Table C.5
Savage and Caverley's Coup Propensity Results for an African Subsample, International Military Education and Training Participation, in Student-Days

Variable	Model 1: Coup Attempts (2002–2013) and Student-Days	Model 2: Coup Attempts (2002–2013) and Total Students
Student-days	–0.000	
	(0.000)	
Total students, annual		0.155
		(0.137)
GDP per capita	–1.03	–1.13***
	(0.762)	(0.791)
U.S. affinity	–0.542	–0.448
	(0.521)	(0.53)
Percentage of GDP from military assistance	–0.28	–0.404
	(2.02)	(2.01)
Military spending	0.000	0.000
	(0.000)	(0.000)
Military personnel	–0.904	–0.102
	(1.1)	(1.12)

Table C.5—Continued

Variable	Model 1: Coup Attempts (2002–2013) and Student-Days	Model 2: Coup Attempts (2002–2013) and Total Students
Coup proofing	0.213	–0.21
	(0.483)	(0.5)
Civil war	–0.364	0.209
	(0.775)	(0.811)
Growth	–0.566	–0.669
	(0.508)	(0.552)
Democracy	–0.754	–0.905
	(0.666)	(0.672)
Terror attack	0.034	–0.01
	(0.55)	(0.548)
Total aid	–0.000	0.000
	(0.000)	(0.000)
Empowered ethnic group	–0.004	–0.043
	(0.14)	(0.14)
Years since coup	–0.0921	–0.107***
	(0.135)	(0.132)
Years since coup, squared	0.000	0.000
	(0.007)	(0.006)
Years since coup, cubed	–0.00002	–0.00018
	(0.00013)	(0.00728)
Constant	–2.54	–2.85
	(0.857)	(0.965)
Observations	625	625

NOTE: Cells report beta coefficients with robust standards errors in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

priate test of the effects of training than a simple count of the number of students who participated or dollars spent. Unfortunately, this measure is limited by data availability and includes only the years 2002 to 2013. This limitation dramatically reduces the sample size from around 1,900 country-years to fewer than 700. Such a reduction obviously has implications for statistical power and makes it more difficult to detect an effect, which is an important caveat. But the *N* is close to many of our period-specific models and should be sufficient to identify large, robust relationships.

But as model 1 reveals, we do not find any such relationship. The coefficient on student-days is negative and insignificant. For comparison, model 2 in Table C.5 replicates the earlier analysis from Table C.3, which found the student measure to be positive and significant. Model 2 uses the same variables as Savage and Caverley did but restricts the analysis to the same time period as our student-days measure. Although we find that the number of students still is associated with more coup attempts, the coefficient is no longer significant, which is potentially a result of the reduction in power. However, the different signs on these coefficients suggest that our student-day measure could be capturing some underlying variation that a raw count of students overlooks. With the mixed findings from the previous Savage and Caverley models, and our generally null results from earlier, we conclude that there is little evidence of a strong relationship between SSA and coup propensity in Africa.

Unfortunately, not even this student-day measure is a perfect test of Savage and Caverley's argument. Various data sources are critical to properly evaluating the relationship between IMET and coups. First, and most importantly, we need data on IMET program length from before 2001. Second, we need to differentiate between participation in IMET and E-IMET. E-IMET courses are offered to nations that have poor civil–military relations or that have militaries at risk of committing human-rights abuses. These countries might also be the most at risk for military coups. As a result, a portion of IMET funding suffers from clear selection effects—that is, it is directed to countries at high risk of coups in hopes of reducing coup risk, so it might be that coup risk is causing the allocation of IMET spending (at least in part), rather

than IMET spending causing coup risk. Finally, a list of participants in IMET courses would be a useful validity check on whether IMET participation leads to coups. If IMET participants represent a higher proportion of coup members than nonparticipants, this might suggest that IMET contributes to a higher likelihood of coups. However, if IMET participants are absent from the list, that absence would suggest that underlying problems drive nations to participate in IMET and make them more likely to experience coups.

Although there are reasons to question the robustness of recent findings linking U.S. military education and training to coup attempts, these findings suggest, at a minimum, the importance of better tracking these programs and their outcomes. For more on the importance of improved monitoring and evaluation, see Chapter Six.

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