WEB APPENDIX - GENOCIDAL CONSOLIDATION

The aim of this concise web appendix is to provide the interested reader with additional background information to the data and analyses as well as key robustness checks. For any additional questions not addressed in this appendix or the paper, please contact the author [author_email here] or access the data on mass indiscriminate violence and elite purges, the replication data, and the replication files from [author_website here] or the Dataverse Network. This document is structured in order of appearance in the paper starting with the data, followed by further elaboration on the respective quantitative tests.

A Scope of the Theory

As noted in the paper, the theoretical argument can be distilled down to three core mechanisms: 1) the direct relationship between the risks to tenure and the physical survival of elites at times of elite rivalry; 2) the relationship between mass violence and authoritarian coalition building; and 3) the relationship between mass violence and the undermining of rival elites' support coalitions. None of these mechanisms would operate in a democratic environment with working checks and balances. Therefore, the outer theoretical scope of the argument is authoritarian regimes—or at least non-democratic regimes.

For mass violence to potentially undermine elite support coalitions, the theory suggests a political structure in which elites rely on (armed) support coalitions with ties to local security institutions. This is a common arrangement within nondemocratic regimes that isn't specific to any particular type of authoritarian regime—e.g., personalist, single party, military. Authoritarian elites rely not only on horizontal coalitions with other elites for their survival but also rely on "vertical" support coalitions. These support coalitions generally have an armed component and can be found in key security institutions, like the military, secret service, paramilitary groups, conscripts, or police, for example. As argued in the paper, genocidal consolidation can undermine these support coalitions.

The mechanism of undermining rival elites' support coalitions through mass violence—and to a lesser extent the mechanism of coalition building through violence—also suggest a geographical location of the violence that corresponds to the geographical location of rival elites' support coalitions. As argued in the paper, the geographical location of genocidal consolidation generally includes the centre of political power. The geographical location differs from that of counterguerrilla mass violence, the victims of which are generally found in the areas where rebels are strongest—commonly the periphery of the country.

Ex-ante observability of the scope conditions. Unfortunately, only nondemocracy is ex ante observable whereas the geographical location of the violence in correspondence with the geographical location of elite support coalitions is not. The potential geographical location of the violence has two components: the geographical distribution of a potential victim outgroup and the geographical location of support coalitions of elite rivals. Neither is readily ex-ante observable.

First, with respect to the geographical distribution of potential target outgroups, we cannot always know in advance which groups will be targeted by a specific regime. While we have a good sense of potential victim outgroups in many cases—e.g., Tutsi in Rwanda—in other cases, potential victims include target groups that are not necessarily obvious before the onset of the conflict. Sudan and Myanmar, for example, have multiple potential victim outgroups with a different geographical distribution within its borders that were targeted at different times.

Moreover, particularly in previous Communist mass killings, victim outgroups have been quite malleable. In Cambodia, for example, an increasingly broad victim outgroup went from city dwellers to simply "Khmer with a Vietnamese mind". That is a very broad victim outgroup indeed; we would have had no way of predicting ex ante: i) that the Khmer Rouge would divide groups in this manner; ii) that the Khmer rouge would transport potential victims to geographical areas controlled by rival elites; and iii) that it would then target

forced migrants within these geographical areas together with local victims for mass killing and starvation. [1] Similarly, as the killing of up to 20,000 "drug criminals" by the Duterte government in the Philippines demonstrates,[2] a victim outgroup does not need to exist before the occurrence of mass violence and can be readily constructed. When the violence itself generates private security benefits—as argued in the paper—there is no limit to the construction of outgroup "enemies" that may be targeted.

Second, with respect to the geographical distribution of elite support coalitions, we cannot always readily observable where these support coalitions are located. Granted, this is more an empirical concern connected with large N studies. In qualitative studies of (potentially) genocidal regimes, it becomes more feasible to locate the geographical support base of specific elites. In Cambodia, for example, Thoy Khon, Ross Nimm, and So Phim all had their base of power in specific parts of the country.[3] Still, we cannot identify the exact geographical location of elite support bases without the identification of specific elite rivals. Consequently, the correspondence of the geographical location of a potential victim outgroup with the geographical location of elite support coalitions is not readily observable before the onset of the violence.

The genocidal consolidation theory, therefore, makes few assumptions with respect to the corresponding geographical location of victim outgroup and rival elites. It merely assumes that selection of which outgroup to target is not random. It assumes that most non-democratic societies contain one or more outgroups that could potentially be victimized—e.g., minorities, city dwellers, teachers, Khmer with a Vietnamese mind, or drug criminals. Genocidal actors would therefore select victims whose geographical distribution overlaps with areas controlled by rivals and at the centre as opposed to the periphery only.

[1] E.g., see Kiernan (1996).

[2] Estimates vary widely. Human rights watch puts the number on 12,000, but a Philippines government report puts the potential number over 20,000. E.g., Watch (2018), Regencia (2018)

[3] E.g., see Kiernan (1996).

Elite ideology and societal cleavages as part of the theoretical scope. Studies show that elite ideology is undeniably part of the process of mass violence.[4] However, elite ideology is not part of the theoretical scope of the genocidal consolidation argument. While genocidal consolidation may interact with societal cleavages and elite ideology, these factors are not actually part of the core theoretical mechanisms of the argument, which are: 1) the direct relationship between the risks to tenure and the physical survival of authoritarian elites at times of elite rivalry; 2) the relationship between mass violence and authoritarian coalition building; and 3) the relationship between mass violence and the undermining of rival elites' support coalitions.

As readily conceded in the paper, ideology and societal cleavages may play an important auxiliary role by helping to motivate and steer violent actors to target outgroups. Yet at the same time, ideology is an important but single motivator among many for violence. As argued in the paper, there exist many common motivators for individuals to participate in violence independent of ideology, such as greed, status, excitement, belonging to a group, and power. Ideology can provide a motivation for those for which ideology is highly salient and provide rationalization for others. It, therefore, can work as a catalyzer for the mobilization process. Still, the mechanisms of the theory work irrespective of ideology, and ideology and societal cleavages are therefore not part of scope of the theoretical argument.

With respect to societal cleavages, the theory makes no assumptions with respect to whether these cleavages pre-exist. It should be obvious that the violence, by definition, needs an outgroup to target. However, societal cleavages are malleable. While pre-existing cleavages like ethnicity or religion remain common in conflict in many areas of the world. There's actually a wide potential for outgroups. As noted above, the theory assumes that an outgroup could be constructed on the basis of existing social divisions such as class, urban background, occupation, or even criminal background.

More importantly, the theory makes no assumptions with respect to the salience of social

[4]E.g., see Maynard (2019), Kim (2018), Walter (2017), Straus (2015), Valentino (2004).

cleavages at the onset of the conflict. Social cleavages can be salient before the onset of the violence but they could also be made salient as part of the genocidal process. There is extensive debate in the field of civil conflict research whether political violence follows pre-existing societal cleavages or whether the salience of these cleavages follows from the violence process itself. The mechanisms of the theory should hold irrespective of whether the salient outgroups exist prior to the violence or becomes salient as part of the violence. Consequently, as noted in the conclusion on pages 41-42 the theory of genocidal consolidation is—in its current form—agnostic with respect to ideology and societal cleavages. As genocidal consolidation is theoretically possible irrespective of ideology and pre-existing societal cleavages, these are not part of the scope of the argument.

B Data

Purges. Some scholars have expressed doubts regarding the validity of the Banks (2012) data on purges within authoritarian regimes. The main issues of concern are with the transparency of the coding. Due to the commercial nature of the Banks data, it is unclear exactly what the purge event was, who was purged, when the purge was dated (e.g. on a removal, arrest, or execution), or what specific sources have been used. Therefore, I collected purge data (ACER)[5] from 1950 until 2004 on all 20 countries that have one or more mass indiscriminate violence events. For these MIV countries, all non-democratic country years were checked for Purges and for Elite Purges, with a particular focus on: 1) years that were coded as purges in Banks; 2) years that had coups or coup attempts;[6] 3) years that saw a leader change according to Archigos (Goemans, Gleditsch and Chiozza 2009); 4) years that saw a change in source of leadership support according to CHISOLS (Mattes, Leeds and Matsumura 2016); and 4) years that saw a change of regime or a change in the type of authoritarian regime according to Geddes (2003). The coding of Elite Purges followed the

[5]The purge data was collected as part of a larger data project on Authoritarian Consolidation and Elite Competition.

[6]The removal of rival elites during coups were not coded as purges. However, if elites that took part in a successful coup were later purged this was coded as a purge

same strategy and is further discussed in the paper.

Table A.1: Comparison of Banks and ACER Purge Data

	Purges in Banks	Purges in ACER
Purge observations in ACER and Banks correspond	94 (52.5%)	94 (44.1%)
Purge observations ACER and Banks differ by one year	9 (5.0%)	9 (4.2%)
Purge observation in Banks, non-regime in ACER	22 (12.3%)	
Purge observation in Banks, no evidence in ACER	54 (30.2%)	
Purge observation in ACER, no purges in Banks		100 (46.9%)
Purge observation in ACER, missing in Banks		10 (4.7%)
Total	179 (100.0%)	213 (100.0%)

Table A.1 contrasts the Banks data with my "ACER" data collection on non-elite Minor Purges. As can be seen from Table A.1 roughly three quarters of the purge observations in Banks either fully correspond or are no more than one year off with the purge data I collected. For 42.5% of the Purge observations in Banks there was either no evidence (30.2%) or the purges targeted people outside the regime (12.3%). From my data collection, several potential issues with the Banks data become apparent: first, Banks codes non-elite purges, which includes purges of junior regime members that by themselves cannot challenge the regime; second, Banks seems somewhat imprecise in its coding of the year of the purge and tends to code arrests and executions, where ACER codes initial dismissal from power; last Banks sometimes includes purges of key opposition figures.

In the paper, I address the first issue by distinguishing between Elite Purges and Minor Purges. The second issue and potential miscoding do introduce noise in the data, but are not otherwise problematic. However, the coding of opposition figures as purges is a concern

to the validity of this study; purged opposition members are not part of the regime and may therefore be conflated with mass indiscriminate violence against the outgroup. Therefore, any non-regime purges in the Banks data have been recoded as zeros for all years for the key countries that had one or more mass indiscriminate violence spells. In the analysis section of this appendix (Table A.4; cols. 1-3), the analysis that uses Banks purges as part of a model to estimate latent Elite Rivalry are repeated using the smaller ACER data for Minor Purges, which strengthens, but does not otherwise change any of the results in the article.

Correlation of Elite Purges and Minor Purges. Minor purges are correlated with elite purges. However, these are qualitatively distinct processes. Minor purges, elite purges and coups are all observable consequences of elite rivalry. However, where elite purges imply the climax and subsequent resolution of the rivalry, minor purges help us observe the rivalry process at an earlier stage. Therefore, minor purges are a useful component of the latent model.

Based on the theory, it is expected that genocidal consolidation is part of the rivalry process. More specifically, it is part of the resolution of the rivalry resulting in elite purges. Therefore, we should observe elite rivalry to correspond to genocidal consolidation (analysis 1) and observe the resolution of rivalry as indicated by elite purges during the genocidal consolidation process (analysis 2). Minor purges are a key indicator in analysis 1, whereas elite purges are a key indicator in analysis 2. While these two analyses are theoretically related, they are two distinct empirical pieces of evidence. Minor purges and the latent measure of elite rivalry do not feature in analysis 2. They therefore do not cause any endogeneity for analysis 2. The lag should alleviate any further concerns that elite purges (as opposed to elite rivalry) are driving mass indiscriminate violence.

Case selection in relation to the Elite Purge Data. To measure Elite Purges, I rely on a new collection of original data on purges of potential challengers within the regime. As noted in the paper, the variable Elite Purges is conceptualized as the purge in any given

year of elite rivals that may actually threaten the leader's tenure and physical security. Simply being a civilian cabinet minister is not sufficient to be considered an elite rival, as coup attempts require control of armed support coalitions. Therefore, purged elite rivals should have formal or informal control of support coalitions that have an armed component, such as the military, secret police, armed paramilitary groups, or praetorian guard. These rivals were operationalized as vice chairmen, senior military officers, chiefs of staff, defense ministers, heads of the secret police, or regional governors in control of armed forces. To ensure transparency and replicability, Elite Purges were coded only when the name of the purged elite could be established.

To identify purged elites and determine their official position and support coalition within the regime, it is important to understand the political context. Therefore data was collected through in-depth country study for the period between 1950 and 2004. There are three types of countries that are potentially relevant to the analysis: 1) countries that experienced non-counter-guerrilla mass violence; 2) countries that experienced counter-guerrilla mass violence; and 3 countries that could potentially experience non-counter-guerrilla mass violence but did not. Therefore, I selected all 20 countries that experienced mass violence at any time during the period of interest.

To arrive at a sample of countries that could potentially experience non-counter-guerrilla mass violence but did not, I listed the leaders who were most likely to initiate mass violence but did not, according to the propensity score matching in model 3. For this, I used a propensity score cut-off of .1. These leaders are listed with their country in Table A.2 below. Then, I collected elite purge data by doing country studies of all of these leaders' countries between 1952 and 2004: Russia (Soviet Union), Sierra Leone, Ethiopia, Mozambique, Chad, Thailand, Laos, Bangladesh, and Myanmar (Burma).

The resulting dataset contains elite purge data on: 10 regimes (counted by leader) that experienced non-counterguerrilla mass violence (genocidal consolidation) for a total of 131 country-year observations—of which 44 years are mass violence years; 30 regimes that expe-

rienced counter-guerrilla mass violence for a total of 303 country-year observations—of which 105 years are mass violence years; and 123 regimes that did not experience mass violence for a total of 676 country-year observations.

Of the regimes that did not experience mass violence, 32 regimes are in countries that at some point in the data experienced non-counterguerrilla mass violence for a total of 222 country-year observations; 66 regimes are in countries that at some point in the data experienced counterguerrilla mass violence for a total of 277 country-year observations; and 45 regimes are in countries that didn't experience any mass violence in the period under examination for a total of 297 country-year observations.[7]

Missing Data for Mass Indiscriminate Violence Observations. Because mass indiscriminate violence is very rare, this study aims to be comprehensive with respect to mass indiscriminate violence spells following the Second World War. In order to ensure that no mass indiscriminate violence spells are lost due to missing data-years of control variables, any missing data for mass indiscriminate violence years was researched (e.g., see King and Zeng 2001). For example, while the mass indiscriminate violence data is collected from 1945 with the first mass indiscriminate violence spell starting in 1949 (China), data on coups and coup attempts by Powell and Thyne (2011) starts in 1950. Consequently, I researched coups or coup attempts for China in 1948-49, which allows for China to enter the data in 1948.[8]

Precise Temporal Order. The small number of mass indiscriminate violence observations allowed for a precise determination of temporal order. Temporal order of Elite Rivalry and Genocidal Consolidation Onset was determined by contrasting coup dates, minor purge dates, and mass indiscriminate violence start dates. Coups and Minor Purges occurring within 12 months before the mass indiscriminate violence onset were coded as 1, but those that followed the onset of mass indiscriminate violence were coded as 0.

[7] 20 regimes that did not experience mass violence are in countries that experienced both non-counterguerrilla and counterguerrilla mass violence for a total of 120 country-year observations.

[8] The year before the onset of Mass Indiscriminate Violence is included to account for temporal order.

Table A.2: Leaders with a high risk of initiating Mass Violence

	Non-counter-guerrilla	
Leader	Country	Year
Khrushchev	Russia (Soviet Union)	1955
Mobutu	Congo, Democratic Republic of (Zaire)	1960
Selassie	Ethiopia	1960
Sukarno	Indonesia	1960
Kasavubu	Congo, Democratic Republic of (Zaire)	1962
Ne Win	Myanmar (Burma)	1962
Mobutu	Congo, Democratic Republic of (Zaire)	1965
Tombalbaye	Chad	1971
Thanon Kittakachorn	Thailand	1971
Marcos	Philippines	1972
Souvanna Phouma	Laos	1973
Machel	Mozambique	1976
Ziaur Rahman	Bangladesh	1976
Zia	Pakistan	1977
Mengistu Marriam	Ethiopia	1977
Ershad	Bangladesh	1982
Saddam Hussein	Iraq	1984
Buhari	Nigeria	1985
Strasser	Sierra Leone	1993

C Elite Rivalry and Genocidal Consolidation

Table A.3 demonstrates that the relationship between Elite Rivalry and Genocidal Consolidation holds for alternative model specifications. The first column addresses potential unobserved heterogeneity by including random effects, which suggests that any heterogeneity does not affect any of the conclusions.[9] The model in the second column corrects for temporal dependence by including non-Mass Indiscriminate Violence years and cubic splines as suggested by Beck, Katz and Tucker (1998), which does not meaningfully affect results.[10]

The third column of Table A.3 addresses potential bias origination from the small number of Genocidal Consolidation onsets in the data (e.g. see King and Zeng 2001). There are several ways to account for rare events by penalizing the likelihood; the Rare Events Logit by King and Zeng (2001) is most commonly adopted in political science. Here, I adopt Firth's Penalized Likelihood Logit (Firth 1993), because it provides almost identical results to the Rare Events Logit (King and Zeng 2001, 148) and provides estimates in cases of perfect discrimination, which allows for better comparison with the model that follows. As can be seen from column three, accounting for rare events does not meaningfully affect any of the results. The fourth column includes Militias as a variable in a Firth's Penalized Likelihood Logit analysis. Because all instances of genocidal consolidation have pro-government militias, Militias cannot not be estimated as part of a regular logit or probit regression on the onset of genocidal consolidation, because it predicts non-occurrence perfectly. The Firth Logit addresses this problem by penalizing the likelihood. After the inclusion of Militias, Irregular Conflict no longer attains conventional significance, likely because the mobilization effect of Irregular Conflict is in part captured by the Militias variable.

Some scholars have argued that mass indiscriminate violence occurs following in civil war (e.g. Licklider 1995, Uzonyi 2015). In the paper I argue that victory in civil war and

[9]Random effects are feasible, more appropriate, and more efficient than fixed effects: 1) the sample is unbalanced (not all countries are non-democratic for all years, for example); 2) the countries in the sample are not functionally equivalent (they are unlikely to share a common effect size); and 3) there is no reason to expect that the unobserved heterogeneity is correlated to regressors in the model.

[10]A cubic polynomial as suggested by Carter and Signorino (2010) generates similar results.

elite rivalry are indeed related: in cases such as Cambodia, victory in civil war resulted in risky competition among regime elites (e.g., Kiernan 1996). Without a common enemy, existing differences within the victorious coalition become salient and may turn deadly. It is therefore not an outgroup threat, but elite ingroup rivalry that drives leaders to initiate mass indiscriminate violence. Still, our confidence in elite rivalry as a cause of mass indiscriminate violence would be greater if it holds when controlling for civil conflict victory. Therefore, column 5 of Table A.3 includes *Civil Conflict Victory* as control variable. Based on the case studies of mass indiscriminate violence, two of the twelve potential cases of genocidal consolidation listed in Table 2 of the paper were initiated after victory in civil conflict: Cambodia in 1975 and China in 1949. These cases of genocidal consolidation onset were therefore coded as Civil Conflict Victory. Beyond these two cases, Civil Conflict Victory was coded whenever *Civil Conflict* had ended in the previous year. Civil Conflict data (25 deaths or greater) was taken from the UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002, Themnér and Wallensteen 2011). Column 5 shows that Elite Rivalry robustly corresponds to the onset of Genocidal Consolidation even when controlling for Civil Conflict Victory. Therefore, the correlation of Civil Conflict and Genocidal Consolidation Onset is likely caused by heightened elite rivalry that results from the breakdown of the victorious coalition.

Column six of Table A.3 repeats the analysis with Civil Conflict instead of Irregular Conflict with similar results. In the last column, I repeat the analysis for authoritarian regimes only, which improves the model fit, but does not otherwise change results. All analyses in Table A.3, with the exception of the Victory in Civil Conflict variable,[11] were repeated with Counter-Guerrilla Mass Violence Onset as dependent variable; as expected, none of these specifications uncovered a relationship between Elite Rivalry and Counter-Guerrilla Mass Violence Onset.

[11] Victory in Civil Conflict is a poor explanation when a guerrilla conflict is ongoing as is the case in Counter-Guerrilla Mass Violence.

Table A.3: Genocidal Consolidation on Elite Rivalry

Model	I Random Effects Probit	II Probit w. Cubic Time Trends	III Firth Logit	IV Firth Logit	V Probit	VI Probit	VII Probit‡
Elite Rivalry	1.11**	.94**	2.15**	2.11**	.78**	.72*	.95**
(Latent probability of coups & attempts)	(.37)	(.27)	(.60)	(.68)	(.30)	(.30)	(.28)
Log of GDP per Capita $_{t-1}$	-.43*	-.39*	-.87**	-1.84**	-.34†	-.31*	-.39*
	(.18)	(.17)	(.32)	(.51)	(.17)	(.15)	(.18)
Log of Population $_{t-1}$	.22	.15	.48*	-.34	.17	.18	.18†
	(.14)	(.11)	(.21)	(.29)	(.11)	(.12)	(.10)
Polity IV	-.10	-.08†	-.14	-.06	-.06	-.06	.22
	(.10)	(.05)	(.19)	(.17)	(.05)	(.04)	(.20)
Irregular Conflict	1.19**	.95**	1.94**	1.10†	1.04**		.98**
	(.35)	(.24)	(.60)	(.67)	(.24)		(.27)
Civil Conflict						.68**	
						(.20)	
Civil Conflict Victory $_{t-1}$					.41		
					(.32)		
Militias				2.78†			
				(1.49)			
Constant	-2.83	-1.82	-5.08	8.13	-2.52	-2.61†	-3.53*
	(1.89)	(1.57)	(3.45)	(5.88)	(1.62)	(1.56)	(1.50)
R^2	.285$^+$	.335	.365$^+$	.542$^+$	.283	.234	.317
Observations	2564	2564	2564	1075	2469	2564	1808

Probit analysis with robust country clustered standard errors in parentheses. †significant at 10%; *significant at 5%; **significant at 1%. Reported Pseudo R^2 is McKelvey & Zavoina's.

$^+$ Reported Pseudo R^2 is Nagelkerke (Cragg-Uhler) calculated by author.

‡ Sample restricted to authoritarian regimes only.

C.1 Latent Model of Elite Rivalry

As argued in the article, we can estimate Elite Rivalry by modeling the risk of a coup or coup attempt that a leader faces. In order to capture the latent rivalry that a leader faces, I estimate a two-stage probit model that first predicts the risk of a Coup Attempt and then adopts the corresponding estimate as a predictor of Genocidal Consolidation and Counter-Guerrilla Mass Violence onset. The functional form of the first stage or "reduced" model is: $\Pr(z_i = 1) = \Pr(\hat{z}_i > 0) = \boldsymbol{\Phi}(\boldsymbol{Z\theta} + \boldsymbol{\varepsilon})$ in which $\boldsymbol{z}$ represents Coup Attempt; $\hat{\boldsymbol{z}}$ represents the estimated Elite Rivalry; and $\boldsymbol{Z}$ is the vector of variables used to estimate $\hat{\boldsymbol{z}}$. The functional form of the second stage or "structural" model is: $\boldsymbol{y} = \boldsymbol{\Phi}(\boldsymbol{X\beta} + \hat{\boldsymbol{z}}\gamma + \boldsymbol{\epsilon})$ in which $\boldsymbol{y}$ represents Genocidal Consolidation or Counter-guerrilla Mass Violence; and $\boldsymbol{X}$ is the vector of control variables. ϵ and ε represent the error terms of the models. Standard errors are analytically derived after Chiozza and Goemans (2003) to account for using an estimate as DV in the second model.

I adopt the time that a leader has been in office (i.e. Leader Tenure, New Leader) and Minor Purges as exclusion restrictions to ensure the model is identified. Based on the theoretical framework presented earlier, none of these variables are expected to directly correspond to the onset of mass indiscriminate violence. However, leaders are expected to face reduced coup risk over time, whereas purges are expected to increase coup risk. (Svolik 2012, Roessler 2011). Therefore, any correlation between these variables and the onset of Genocidal Consolidation is expected to be a direct function of heightened Elite Rivalry. Note that the model captures a latent risk and that the endogeneity of Coup Attempt is not expected to be a concern. Even if it were, the reduced model explains a considerable part of the variation in Coup Attempt ($R^2 = .403$ - instruments: $R^2 = .276$) so bias due to a weakness of the instruments is not a concern either (e.g., Bound, Jaeger and Baker 1995).[12]

Table A.4 reports different specifications for the latent measure of Elite Rivalry. The analyses in the first three columns of Table A.4 repeat the latent analysis with the original

[12] Also, the instruments themselves are strongly predictive of the variation in Coup Attempt ($R^2 = .276$).

(ACER) data that I collected on Minor Purges for all the countries that have one or more onsets of mass indiscriminate violence from 1950 until 2004 or that have an estimated genocidal consolidation risk higher than .1 at any time during this period. Again, the results do not change, if anything they become more robust with the ACER data despite the much smaller set of observations. Columns 4-6 includes Civil Conflict in the analysis instead of Irregular Conflict with similar results. Last, Columns 7-9 repeat the latent analysis, but instead of predicting coups or coup attempts in the first stage, it predicts only successful Coups only. This slightly weakens effects, but does not meaningfully change the results.

Table A.4: Two-Stage Elite Rivalry Models

	With ACER Purge Data			With Civil Conflict			Predict Coups in First Stage		
	I Coup Attempt	II Cons. Genocide	III Counter-Guerrilla	IV Coup Attempt	V Cons. Genocide	VI Counter-Guerrilla	VII Coup	VIII Cons. Genocide	IX Counter-Guerrilla
Elite Rivalry (Latent probability of coups & attempts)		.59** (.16)	.09 (.13)		.39** (.14)	.24 (.15)			
Elite Rivalry (Latent probability of coups)								.20* (.08)	.10 (.07)
Log of GDP per capita $_{t-1}$	-.09 (.09)	-.21 (.31)	.09 (.12)	-.06 (.06)	-.28† (.16)	.02 (.10)	-.17* (.08)	-.30* (.15)	-.03 (.10)
Log of Population $_{t-1}$	-.20** (.06)	.21† (.13)	-.05 (.09)	-.19** (.05)	.23† (.12)	.05 (.09)	-.18** (.07)	.18* (.09)	.03 (.07)
Polity	.01 (.03)	-.10 (.08)	-.01 (.05)	-.01 (.02)	-.08 (.05)	-.04 (.04)	-.05* (.02)	-.07 (.04)	-.02 (.05)
Irregular Conflict	.30* (.15)	.58* (.29)	1.06** (.25)				.26† (.15)	.83** (.25)	1.30** (.23)
Civil Conflict				.57** (.10)	.52* (.23)	1.12** (.30)			
Leader Tenure	-.04* (.02)			-.04** (.01)			-.04** (.01)		
Minor Purges (Non-Elite)	1.23** (.17)			.61** (.11)			.69** (.14)		
New Leader (incl. transition year)	.99** (.15)			.96** (.08)			2.30** (.21)		
Constant	.47 (.70)	-2.18 (1.98)	-2.58* (1.05)	.45 (.58)	-2.34 (1.55)	-3.02** (.79)	-.14 (.85)	-1.98 (1.38)	-2.82** (.79)
R^2	.495	.344	.281	.422	.265	.256	.599	.273	.241
Observations	924	924	924	2564	2564	2564	2564	2564	2564

Robust country clustered standard errors in parentheses.
†significant at 10%; *significant at 5%; **significant at 1%, two-tailed. Reported Pseudo R^2 is McKelvey & Zavoina's.

D Genocidal Consolidation and Elite Purges

Table A.5 repeats the probit analyses on Elite Purges for Genocidal Consolidation and Counter-guerrilla Mass Violence spells as reported in Table 5 in the paper. Columns I-V of Table A.5 repeat the analyses with Civil Conflict instead of Irregular Conflict. Again, none of the substantive results change. Moreover, the analyses in Columns VI-X of Table A.5 show that results are robust to a correction for unobserved heterogeneity using random effects.

Columns I-III of Table A.6 repeats the probit analyses on Elite Purges for Genocidal Consolidation and Counter-guerrilla Mass Violence spells as reported in Columns I-III of Table 5 of the paper with the at-risk sample. There's an increase in variance that can be explained by the considerably smaller sample size but genocidal consolidation retains conventional ($p<.05$) or greater significance for all models. Moreover, the analyses in Columns IV-VII of Table A.6 repeats all analyses of the at-risk sample with all genocidal observations inlcuded, which strengthens but does not otherwise change results.

D.1 Precise Temoral Order

The analyses of Genocidal Consolidation and Elite Purges in columns I-III of Table 5 of the paper take a one-year lag and therefore fail to capture rapid genocidal consolidation processes like Rwanda. The limited number of mass violence observations does allow for an alternative specification with a precise determination of temporal order, however. Specifically, because we know the exact timing of the purges and the mass violence onsets, we can precisely determine whether to connect elite purges to mass violence onset observations or pre-onset observations. For example, if mass violence took place in winter 1991 and elite purges would occur any time between winter 1991 and winter 1992, the coding would show these purges to correspond to the year of mass violence. If elite purges were to occur before the start of the violence, however, the coding would show elite purges to correspond to the year before mass

violence. This coding therefore both captures purges that occurred only two weeks after the start of the violence as well as those purges that occurred within a year of the violence.

Table A.5: Probit on Elite Purges for Consolidatory and Counter-Guerrilla Mass Violence

	With Civil Conflict					Random Effects				
	I Elite Purges	II Elite Purges	III Elite Purges	IV Elite Purges‡	V Elite Purges‡	VI Elite Purges	VII Elite Purges	VIII Elite Purges	IX Elite Purges‡	X Elite Purges‡
Mass Indiscriminate Violence $_{t-1}$	.32†					.63**				
	(.18)					(.23)				
Genocidal Consolidation $_{t-1}$ (non-counter-guerrilla)		1.02**	.73**				.98**	.80**		
		(.24)	(.22)				(.27)	(.22)		
Genocidal Consolidation (non-counter-guerrilla)				1.38**	1.29**				1.47**	1.27**
				(.22)	(.31)				(.29)	(.41)
Counter-guerrilla Mass Violence $_{t-1}$		-.28	-.27†				.10	-.12		
		(.20)	(.15)				(.32)	(.22)		
Counter-guerrilla Mass Violence				-.30	.00				.02	.01
				(.35)	(.50)				(.34)	(.46)
Civil Conflict	-.11	.13	.14	.12	-.11					
	(.17)	(.15)	(.12)	(.17)	(.28)					
Irregular Conflict						-.66**	-.52**	-.24*	-.34†	-.23
						(.18)	(.19)	(.12)	(.18)	(.40)
Population	.10†	.12*	.10*	.13*	-.03	.08	.08	.11†	.11	-.03
	(.06)	(.05)	(.04)	(.05)	(.10)	(.09)	(.07)	(.06)	(.08)	(.12)
GDP per Capita	.00	.13	-.03	.25*	.24	.01	.10	.00	.26*	.25
	(.12)	(.11)	(.10)	(.12)	(.21)	(.11)	(.11)	(.07)	(.12)	(.22)
Polity	-.06	-.07	-.04	-.05	.05	-.10*	-.09*	-.04	-.07†	.05
	(.04)	(.04)	(.04)	(.04)	(.13)	(.04)	(.04)	(.03)	(.04)	(.13)
Leader Tenure	-.02	-.02	-.02*	-.02	-.01	-.02	-.02†	-.02**	-.02	-.01
	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.02)
New Leader (incl. transition year)	.28*	.29*	.44**	.16	.12	.40*	.40*	.53**	.23	.11
	(.12)	(.13)	(.09)	(.14)	(.35)	(.18)	(.17)	(.12)	(.17)	(.33)
Militias	.23	.08		-.04	-.39	.42	.34		.14	-.41
	(.25)	(.23)		(.23)	(.51)	(.26)	(.24)		(.25)	(.43)
Constant	-1.87	-2.96*	-1.71*	-3.94**	-2.05	-1.66	-2.21*	-1.95**	-3.68**	-1.99
	(1.32)	(1.20)	(.77)	(1.22)	(2.27)	(1.21)	(1.12)	(.69)	(1.20)	(2.26)
R^2	.096	.137	.102	.163	.193	.112+	.128+	.087+	.159+	.165+
Obs.	535	535	1025	536	119	535	535	1025	536	119

Cols I-V: Probit analysis with robust country clustered standard errors in parentheses. Cols VI-X: Random Effects Probit analysis clustered by country with standard errors in parentheses. †significant at 10%; *significant at 5%; **significant at 1%. Reported Pseudo R^2 for is McKelvey & Zavoina's unless otherwise noted.
+ Reported Pseudo R^2 is Nagelkerke (Cragg-Uhler) calculated by author.
‡ Precise temporal order.

Table A.6: Probit on Elite Purges for Consolidatory and Counter-Guerrilla Mass Violence

	At-risk models			All Genocidal Consolidation obs.			
	I Elite Purges	II Elite Purges	III Elite Purges	IV Elite Purges	V Elite Purges	VI Elite Purges	VII Elite Purges‡
Mass Indiscriminate Violence $_{t-1}$	.08			.15			
	(.31)			(.20)			
Genocidal Consolidation $_{t-1}$ (non-counter-guerrilla)		.68*	.59*		.50**	.58**	
		(.27)	(.28)		(.19)	(.14)	
Genocidal Consolidation (non-counter-guerrilla)							1.08**
							(.26)
Counter-guerrilla Mass Violence $_{t-1}$		-.55	-.33		-.48	-.32	
		(.42)	(.23)		(.42)	(.22)	
Counter-guerrilla Mass Violence							.10
							(.50)
Irregular Conflict	-1.04**	-.98**	-.67**	-1.19**	-.99**	-.72**	-.40
	(.33)	(.31)	(.15)	(.25)	(.22)	(.14)	(.25)
Population	-.06	-.10	-.05	-.07	-.06	-.04	-.01
	(.11)	(.11)	(.06)	(.09)	(.09)	(.05)	(.07)
GDP per Capita	.12	.23	.11	.19	.22	.13	.22
	(.20)	(.21)	(.11)	(.15)	(.17)	(.10)	(.14)
Polity	.00	-.04	.12†	.03	.01	.13*	.11
	(.15)	(.16)	(.07)	(.13)	(.12)	(.06)	(.10)
Leader Tenure	-.02	-.02	-.01	-.02	-.02	-.02	-.01
	(.02)	(.02)	(.01)	(.02)	(.02)	(.01)	(.01)
New Leader (incl. transition year)	.37	.52	.46**	.24	.37	.40**	.01
	(.29)	(.32)	(.15)	(.28)	(.28)	(.15)	(.34)
Militias	-.01	-.12		.01	-.16		-.43
	(.40)	(.41)		(.39)	(.42)		(.43)
Constant	-.11	-.14	-1.09	-.43	-.67	-1.23	-2.07
	(2.31)	(2.33)	(.87)	(1.94)	(1.99)	(.86)	(1.79)
R^2	.159	.210	.186	.238	.267	.218	.263
Obs.	118	118	272	139	139	293	140

Probit analysis with robust country clustered standard errors in parentheses. Cols I-III: At-risk observations with lagged independent variable. Cols IV-VII: At-risk observations with all genocidal consolidation observations included. †significant at 10%; *significant at 5%; **significant at 1%. Reported Pseudo R^2 for is McKelvey & Zavoina's.

‡Precise temporal order.

E Mass Violence and Leader Fates

Both the treatment and control units for the matching analysis were drawn from the pool of non-democratic regimes. For each leader only a single observation was selected. Specifically, I selected a single observation per leader based on the predicted probabilities generated by the first model - as shown in column 1 of Table 3 on page 31 of the paper. For each leader with multiple observation years, only the observation with the highest predicted probability was retained.

While all outcome variables are at the leader level, data on which to estimate predicted probabilities and on which to match leaders is at the country-year level. Therefore it would not make sense to have more than a single leader observation per country-year. I therefore take the first leader for each country year for which there is more than one leader. An exception is for those years in which there is a successful coup. An unsuccessful coup is a good proxy for elite rivalry. A successful coup, on the other hand, is a good proxy for elite rivalry in the new government, but it is also the exit event for the incumbent. Therefore, I take the second leader when there are only two leaders and the first coup event in the year is successful. When there is a successful coup and there are more than two leaders for that year, I qualitatively established the leader that came to power as a result of the coup. In these years, the first leader that came to power by a successful coup was used as the observation for that year.

For each leader, only the observation with the highest predicted probability was retained. Ties in predicted probability of leader observations were examined and were found to have exactly the same leader outcomes. Therefore, the choice of tied observation was inconsequential for the analysis and I simply used the later observation. Also, there are three genocidal consolidation onset observations for Mao Tse-Tung of which only one (1966) falls within the support of control observations in the matching analysis. Therefore, I retain only the 1966 observation, which fits with using a single observation per leader.

Variables used in the estimation of the propensity score. After selecting a single observation the sample was matched using the PSMATCH2 matching algorithm (Leuven and Sianesi 2003). Note that we can err on the side of inclusion as the inclusion of variables unassociated with the treatment has little influence on the propensity score model (e.g., Stuart 2010). The sample was therefore matched on all variables adopted in analysis 1 to predict the onset of Genocidal Consolidation: GDP per Capita, Population, Polity, Tenure, New Leader, Minor Purges, Irregular Conflict, and Rumored Coups (i.e. coups, coup attempts, as well as rumored or alleged coups). An alternative specification that includes Horizontal Inequality is presented in Appendix G.4 on page 31 of this Appendix.

To further improve the balance, I adopted a combination of nearest neighbor matching and radius matching with a caliper with replacement of control observations. With radius matching and nearest neighbor matching there is always a trade off between the closeness of the matches and efficiency. The appropriate caliper in part depends on the data (e.g., Lunt 2014, Stuart and Rubin 2008). I have adopted a variety of specifications for matching on the propensity score and selected the specification that provided the greatest balance, while keeping the standardized differences in means less than 0.25 (B), and the variance ratios between 0.5 and 2 (R), as suggested by Stuart and Rubin (2008).

Table A.7 below shows the mean balance, difference in means (B), and variance ratios (R) of various specifications. As can be seen from Table A.7, radius matching, which includes all control observations within a certain caliper distance of a treated observation, results in suboptimal balance. This is mainly because the propensity score is estimated using a probit and therefore compressed at the tails. A change on a single variable is going to have a relatively small effect on the estimated propensity score at the tails and a relatively large effect on the estimated propensity score at the center of the curve. Therefore, the propensity score distances between observations at high propensity scores (e.g., Mao Tse-Tung .225) are much greater than those at low propensity scores (e.g., Sindikubwabo .018). As can be seen from Table A.7, the best balance and efficiency is achieved through a combination of

nearest neighbors and a caliper of .1, which results in a smaller caliper distance at the tail and a greater distance at higher propensities.

Table A.7: Balance of various matching specifications

Specification	Mean bias	B: under 25	R: between .5 and 2
Caliper .01	8.3	no (29.0)	no (0.47)
Caliper .02	8.2	no (38.7)	no (0.31)
Caliper .05	22.2	no (67.9)	no (0.24)
Caliper .1	31.2	no (98.2)	no (0.17)
Caliper .05 & 50 nearest neighbors	7.4	no (26.6)	yes (1.20)
Caliper .1 & 50 nearest neighbors	7.6	yes (23.5)	yes (1.39)
Caliper .2 & 50 nearest neighbors	9.4	no (30.4)	yes (1.53)

B: standardized differences in means. Should be less than 25.
R: variance ratios. Should be between .5 and 2

Table A.8 presents the propensity scores of "treated" leaders, that of their furthest matches, and the effective maximum propensity distance (caliper) of these leaders to their matches. As can be seen from Table A.8 below, 50 nearest neighbors results in a nice caliper distribution that is very small at the tails, but gradually expands as we move higher up the propensity curve. The addition of a caliper of .05 or .1 ensures the treatment observations with the highest propensity scores can find sufficient matches that are still close. Moreover, treated and control observations share common support. We therefore have added confidence that the slightly higher effective caliper for Micombero (.064) and Mao Tse-Tung (.097) is appropriate, because they are not taking any matches outside the range of treatment observations. Treated leaders their matches are listed in Tables A.15-A.24 and unmatched leaders are listed in Table A.25.

In sum, 50 nearest neighbors and a .1 caliper resulted in optimal balance on all indicators (see Table A.7), including on t-tests of the individual matching variables. This specification was therefore adopted as the main specification for all analyses in the paper. The balance of the specification with a .5 caliper is close and I therefore also provide the outcomes for this specification in column 2 of Table A.9. Columns 3 and 4 of A.9 show the outcomes of

Table A.8: Propensity score distances for 50-neighbor matching with .1 caliper

Leader	P-score	P-score Lowest match	P-score Highest match	Effective caliper
Sindikubwabo	0.018	0.013	0.022	0.004
Amin	0.022	0.017	0.028	0.005
Milosevic	0.031	0.022	0.040	0.009
Gowon	0.046	0.028	0.064	0.019
Suharto	0.050	0.030	0.068	0.020
Al-Bashir	0.055	0.034	0.076	0.021
Kayibanda	0.073	0.041	0.106	0.033
Pol Pot	0.089	0.050	0.128	0.039
Micombero	0.120	0.056	0.179	0.064
Mao Tse-Tung	0.225	0.128	0.189	0.097

Adverse Leader Fates within a three year window. If we take a shorter window, the effects of Genocidal Consolidation on Death are generally the same, but the variance of Prison increases. The effects of Genocidal Consolidation on Internal Irregular Exit, and Exile on the other hand are strengthened. Overall, results indicate that leaders that initiate Genocidal Consolidation have a reduced risk of Adverse Leader Fates and losing office to ingroup rivals.

F Placebo: Counter-Guerrilla Mass Violence and Leader Fates

Figure A.1 revisits the placebo treatment and includes the placebo model details. Like the matching analysis in the paper, the placebo treatment matches leaders on the propensity to adopt mass indiscriminate violence. However, unlike the analysis in the paper, the placebo test matches of the propensity to initiate Counter-Guerrilla Mass Violence.[13] Consequently, the Treated and Control observations differ from the main test in the paper. Therefore, because both the treatment and placebo treatment have their own control groups, the error bars do not provide us with any indication of whether there exists a significant difference

[13]While the balance for the placebo test is almost as good as for the main analysis, optimal balance for the placebo test would be achieved with a .05 caliper rather than a .1 caliper. Specifically, the difference in means is 26.8 for with a .1 caliper and 24.8 with a .5 caliper. However, to serve as placebo, the specification should be exactly the same as for the main model.

Table A.9: Average treatment effect of Genocidal Consolidation on Adverse Leader fates within 5 and 3 years

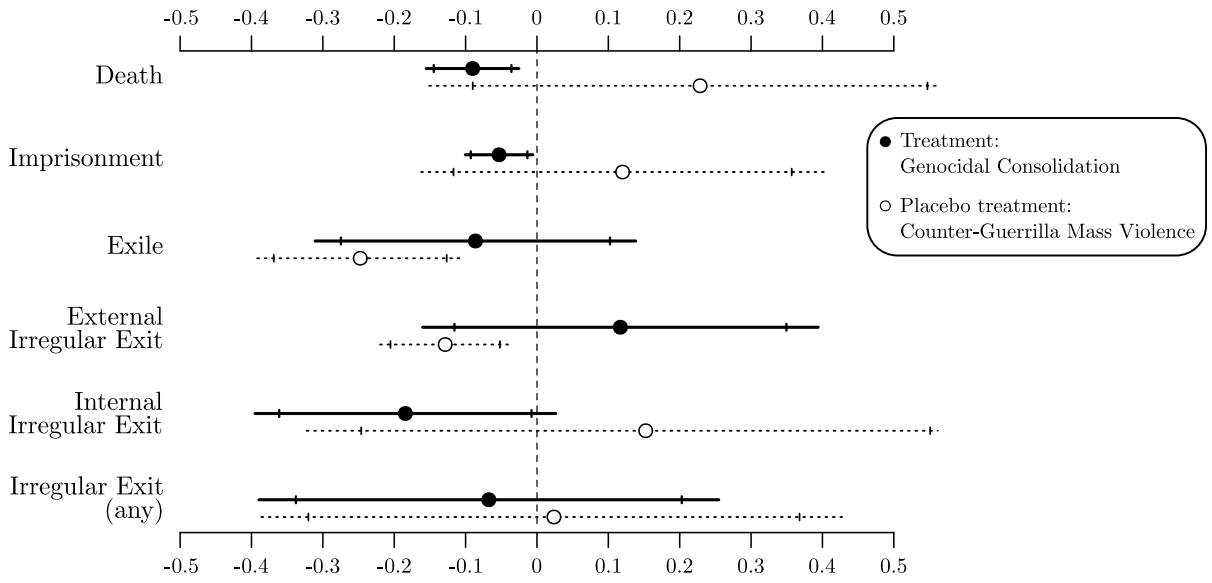
	Fate within 5 years		Fate within 3 years	
	50:n with .1 caliper	50:n with .05 caliper	50:n with .1 caliper	50:n with .05 caliper
Death	-.09**	-.07*	-.06*	-.06*
	(.03)	(.03)	(.03)	(.02)
Prison	-.05*	-.05*	-.03†	-.03†
	(.02)	(.02)	(.02)	(.02)
Exile	-.09	-.16	-.14**	-.22*
	(.11)	(.15)	(.04)	(.09)
External Irregular Exit	.12	.08	.02	-.01
	(.14)	(.16)	(.11)	(.13)
Internal Irregular Exit	-.18†	-.20†	-.18**	-.22**
	(.11)	(.12)	(.04)	(.07)
Irregular Exit (any)	-.07	-.12	-.16	-.23†
	(.17)	(.19)	(.11)	(.14)

Average treatment effect of Genocidal Consolidation with bootstrapped standard errors in parentheses.
†significant at 10%; *significant at 5%; **significant at 1%.

between the Treatment and the Placebo. Treatment effects are the difference between the treated and the most likely to be treated; and placebo effects are the difference between the placebo treatment and those most likely to receive the placebo treatment.

As expected in the theory, leaders that initiate Counter-Guerrilla Mass Violence are no more likely to survive than most similar leaders that do not, but leaders that initiate Counter-Guerrilla Mass Violence are less likely to be Exiled or face External Irregular Exits. Recall that External Irregular Exits are foreign or rebel induced and that leaders that lose a Civil War are more likely to be Exiled. Where genocidal consolidation is expected to be at a higher risk of foreign intervention, Counter-Guerrilla Mass Violence should not affect foreign intervention; Counter-Guerrilla Mass Violence is, however, expected to be an effective strategy in guerrilla conflicts and should therefore reduce the probability of being removed by rebels. We should therefore observe a reduced risk of External Irregular Exits and Exile. As expected, Figure A.1 shows that Counter-Guerrilla Mass Violence affects External Irregular Exits and Exile only.

Figure A.1: Average Treatment Effect of Placebo (Counter-Guerrilla Mass Violence) on Adverse Leader Fates within 5 Years



Leaders matched to the 50 nearest neighbors on the estimated propensity score of initiating Non-Counter-Guerrilla Mass Violence (based on GDP per Capita, Population, Polity, Tenure, New Leader, Minor Purges, Irregular Conflict, and Rumored Coups) with a mean caliper range of 0.03 and a maximum caliper range of 0.097. N = 367; Treated = 10; and Control = 357. Bars represent the 95% confidence interval; and tics represent the 90% confidence interval.

G Horizontral Inequality

Some scholars have pointed towards societal cleavages as an important confounder that could potentially bias results in the paper. Below, I have therefore incorporated Horizontal Inequality as an explanatory variable in all analyses. As detailed below, the inclusion of Horizontal inequality does not meaningfully change results.

G.1 Horizontal Inequality Data

Data on horizontal inequality was taken from the Ethnic Power Relations Dataset (Vogt et al. 2015). I focused on discrimination and created a dummy variable labled Minority Discrimination that takes a 1 for country years in which there's any group consisting of at least 5% of the country's population whose "*Group members are subjected to active, intentional, and targeted discrimination by the state, with the intent of excluding them from*

political power. Such active discrimination can be either formal or informal, but always refers to the domain of public politics (excluding discrimination in the socio-economic sphere)" (Vogt 2014, 5)

G.2 HI and the relationship between elite rivalry and mass violence

The argument for confounding primarily affects the first study that examines the relationship between elite rivalry and mass violence. Table A.10 below, shows the results for study 1 when we control for horizontal inequality; the inclusion of horizontal inequality does not meaningfully change results as presented in the paper.

Moreover, the estimated effects of elite rivalry become even slightly stronger.[14] Based on the model with horizontal inequality we estimate that in any given year a median non-democratic regime has essentially a 0 percent chance [CI 95%: 0.0%; 0.1%] of genocidal consolidation onset; during Elite Rivalry this percentage increases to 0.4 percent [CI 95%: 0.0%; 1.3%]. However, the estimated effect is considerably stronger for a median country with horizontal inequality and guerilla activity, which would have an estimated 1.8 percent risk [CI 95%: 0.1%; 6.8%] without Elite Rivalry and 9.5 percent risk [CI 95%: 2.5%; 22.1 %] with Elite Rivalry.

Also note that controlling for Minority Discrimination doesn't substantially improve model fit of the first study as indicated by a likelihood ratio test (chi-squared=2.61; p=0.105—without clustered standard errors) or Wald test (chi-squared=1.85; p=0.174). I therefore did not include it in the propensity score estimation of study 3 in the paper. Note that I did, however, redo study 3 with Minority Discrimination as part of the propensity score estimation under Appendix F.4 below.

[14]Note that the strengthening of the effect could partially be caused by compression—i.e., the artifact of dichotomous models that fit the predicted probability on an S-curve. Because of compression, marginal effects of specific variables are dependent on the other variables in the model, particularly at the center (Berry, DeMeritt and Esarey 2010, Rainey 2016).

Table A.10: Elite Rivalry and Mass Indiscriminate Violence Onset with Minority Discrimination

	I Cons. Genocide	II Counter-Guerrilla	III Coup Attempt	IV Cons. Genocide	V Counter-Guerrilla
Elite Rivalry (Coup rumors, allegations, attempts & successes)	.88** (.28)	.35 (.23)			
Elite Rivalry (Latent probability of coups & attempts)				.42* (.16)	.14 (.16)
GDP per Capita $_{t-1}$	-.35* (.16)	-.09 (.11)	-.07 (.06)	-.31* (.15)	-.08 (.11)
Population $_{t-1}$	.21* (.10)	.04 (.08)	-.18** (.05)	.25** (.09)	.06 (.09)
Polity	-.07 (.05)	-.02 (.05)	-.01 (.02)	-.09† (.05)	-.02 (.05)
Minority Discrimination	.42 (.31)	.55* (.22)	-.08 (.12)	.40 (.30)	.54* (.22)
Irregular Conflict	.94** (.24)	1.38** (.24)	.26* (.10)	.79** (.27)	1.34** (.24)
Leader Tenure			-.04** (.01)		
Regime Purges (Non-elite)			.57** (.11)		
New Leader (incl. transition year)			.95** (.08)		
Constant	-2.87† (1.43)	-3.08** (.92)	.42 (.56)	-2.53† (1.33)	-3.06** (.92)
R^2	.314	.273	.403	.312	.287
Observations	2564	2564	2564	2564	2564

Probit analysis with robust country clustered standard errors in parentheses. Onsets only, ongoing mass indiscriminate violence dropped from the analysis. Corrected for temporal order of Elite Rivalry and Mass Indiscriminate Violence Onsets. †significant at 10%; *significant at 5%; **significant at 1%. Reported Pseudo R^2 is McKelvey & Zavoina's.

G.3 HI and the relationship between mass violence and elite purges

While I do not think horizontal inequality should theoretically matter as a confounder for elite purges, I nonetheless controlled for horizontal inequality in study 2 for completeness sake. As can be seen from Table A.11 below, Minority Discrimination seems to have a very weak negative relationship to elite purges that doesn't attain convential significance. Minority Discrimination does attain significance at the 10% level in the larger sample without militias as can be seen from column 3 of Table A.11 but do note that the 10% level is less

meaningful for larger samples. Overal, the inclusion of Minority Discrimination does not affect the main relationship between genocidal consolidation and elite purges.

[Table A.11 about here]

Table A.11: Probit on Elite Purges for Genocidal Consolidation and Counter-Guerrilla Mass Violence Spells with Minority Discrimination

	I Elite Purges	II Elite Purges	III Elite Purges	IV Elite Purges‡	V Elite Purges‡
Mass Indiscriminate Violence $_{t-1}$	.46*				
	(.19)				
Genocidal Consolidation $_{t-1}$ (non-counter-guerrilla)		.96**	.77**		
		(.21)	(.22)		
Genocidal Consolidation (non-counter-guerrilla)				1.36**	1.28**
				(.20)	(.33)
Counter-guerrilla Mass Violence $_{t-1}$		-.04	-.14		
		(.24)	(.15)		
Counter-guerrilla Mass Violence				-.09	.01
				(.39)	(.48)
Irregular Conflict	-.60**	-.47**	-.24*	-.32†	-.23
	(.18)	(.17)	(.10)	(.19)	(.29)
Population	.06	.05	.07†	.07	-.03
	(.06)	(.05)	(.04)	(.05)	(.10)
GDP per Capita	.04	.13	.00	.25*	.24
	(.10)	(.10)	(.10)	(.11)	(.19)
Polity	-.08†	-.08†	-.04	-.06	.07
	(.04)	(.04)	(.04)	(.04)	(.13)
Minority Discrimination	-.10	-.13	-.22†	-.17	.14
	(.16)	(.16)	(.11)	(.17)	(.30)
Leader Tenure	-.02	-.02†	-.02*	-.02	-.01
	(.01)	(.01)	(.01)	(.01)	(.01)
New Leader (incl. transition year)	.35**	.37**	.50**	.21	.12
	(.13)	(.14)	(.08)	(.15)	(.34)
Militias	.37	.31		.13	-.41
	(.23)	(.23)		(.23)	(.42)
Constant	-1.47	-2.07†	-1.35†	-3.18**	-2.07
	(1.18)	(1.12)	(.71)	(1.15)	(2.30)
R^2	.154	.170	.115	.179	.196
Obs.	535	535	1025	536	119

Probit analysis with robust country clustered standard errors in parentheses.
†significant at 10%; *significant at 5%; **significant at 1%. Reported Pseudo R^2 is McKelvey & Zavoina's.
‡First year omitted for each genocidal consolidation spell.

G.4 HI and Genocidal Consolidation, Adverse Fates, and Irregular Removals

In order to account for Horizontal Inequality in study 3, I included Minority Discrimination in the propensity score estimation of the study, which resulted in worse balance but otherwise did not meaningfully change results. Table A.12 below shows the balance of matching specifications with Horizontal Inequality and Table A.13 reprints the original balance specifications of Table A.7 to ease comparison. Overall, the original model in the paper has better balance than any of the specifications with horizontal inequality. Also, none of the models with horizontal inequality fully comply with the balance requirements. The model with a caliper of .1 and 50 nearest neighbors, similar to the main model in the paper, comes very close to complying with balance requirements with a B that is only slightly over 25.

I therefore redid the main analysis with a caliper of .1; 50 nearest neighbors; and horizontal inequality included in the estimation of the propensity score. The outcomes of the analysis compared to the outcomes of the original analysis are in table A.14 below. As can be seen from table A.14, the average treatment effects of Genocidal Consolidation are generally the same, but the variances increase slightly as balance decreases. As a result, imprisonment falls just short of conventional significance (p=.056). This is not surprising given that imprisonment is less robust to changes of specification as has been noted in the paper and appendix. It doesn't change any of the substantive conclusions in the paper that suggests sizable and robust effects of genocidal consolidation on death and sizable, but less robust, effects of genocidal consolidation on Imprisonment and Internal Irregular Exits.

I would like to note that if horizontal Inequality had presented as an important confounder in study one, it obviously should also have entered into the main model in the paper. However, given that horizontal inequality doesn't meaningfully change outcomes of the original model and also doesn't meaningfully improve the models as indicated by the likelihood ratio test and Wald test, however, I'm confident that the more parsimonious and model as presented in the paper works well. In sum, controlling for Horizontal Inequality over all studies does not meaningfully change results.

Table A.12: Balance of various matching specifications with Horizontal Inequality in propensity score

Specification	Mean bias	B: under 25	R: between .5 and 2
Caliper .01	11.4	no (44.2)	yes (0.55)
Caliper .02	9.5	no (36.2)	yes (0.99)
Caliper .05	22.7	no (75.0)	no (0.26)
Caliper .1	28.3	no (90.7)	no (0.17)
Caliper .05 & 50 nearest neighbors	8.9	no (26.6)	yes (1.25)
Caliper .1 & 50 nearest neighbors	8.5	no (25.2)	yes (1.37)
Caliper .2 & 50 nearest neighbors	9.3	no (39.7)	yes (0.98)

B: standardized differences in means. Should be less than 25.
R: variance ratios. Should be between .5 and 2

Table A.13: Balance of various matching specifications

Specification	Mean bias	B: under 25	R: between .5 and 2
Caliper .01	8.3	no (29.0)	no (0.47)
Caliper .02	8.2	no (38.7)	no (0.31)
Caliper .05	22.2	no (67.9)	no (0.24)
Caliper .1	31.2	no (98.2)	no (0.17)
Caliper .05 & 50 nearest neighbors	7.4	no (26.6)	yes (1.20)
Caliper .1 & 50 nearest neighbors	7.6	yes (23.5)	yes (1.39)
Caliper .2 & 50 nearest neighbors	9.4	no (30.4)	yes (1.53)

B: standardized differences in means. Should be less than 25.
R: variance ratios. Should be between .5 and 2

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Table A.14: Average treatment effect of Genocidal Consolidation on Adverse Leader fates with and without horizontal inequality

	Original model without Horizontal Inequality	Model with Horizontal Inequality
	50:n with .1 caliper	50:n with .1 caliper
Death	-.09**	-.09**
	(.03)	(.03)
Prison	-.05*	-.05†
	(.02)	(.03)
Exile	-.09	-.10
	(.11)	(.12)
External Irregular Exit	.12	.14
	(.14)	(.14)
Internal Irregular Exit	-.18†	-.21†
	(.11)	(.11)
Irregular Exit (any)	-.07	-.07
	(.17)	(.17)

Average treatment effect of Genocidal Consolidation with bootstrapped standard errors in parentheses.
†significant at 10%; *significant at 5%; **significant at 1%.

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Table A.15: Leader observations matched to Sindikubwabo (pscore 0.018)

leader	*country*	*year*	*pscore*
Sankara	Burkina Faso (Upper Volta)	1983	.01343037
Brezhnev	Russia (Soviet Union)	1964	.01354715
Costa de Silva	Brazil	1968	.01359647
H. Aliyev	Azerbaijan	1994	.01379476
Odria	Peru	1954	.01415805
Caetano	Portugal	1974	.01448112
Gottwald	Czechoslovakia	1951	.01449367
Cerezo	Guatemala	1988	.01481856
Siad Barre	Somalia	1970	.01483597
Bierut	Poland	1952	.01484007
Ruiz Cortines	Mexico	1954	.01486938
Konan Bedie	Cote d'Ivoire	1995	.01527506
Jiang Zemin	China	1998	.01533403
Chiang Kai-shek	Taiwan	1967	.01538445
Sardar Mohammad Daud Khan	Afghanistan	1973	.01541189
Sadat	Egypt	1971	.01555953
Conte	Guinea	1985	.01569803
Mohammad Yusuf	Afghanistan	1963	.01574221
Lamizana	Burkina Faso (Upper Volta)	1980	.01607314
Kaunda	Zambia	1990	.01609562
Houphouet-Boigny	Cote d'Ivoire	1980	.01610291
Zerbo	Burkina Faso (Upper Volta)	1981	.01636345
Van Huong	Vietnam, Republic of	1964	.01665548
Kountche	Niger	1974	.01713455
Magloire	Haiti	1952	.01766939
Ankrah	Ghana	1967	.01766956
Diori	Niger	1961	.01769119
Birendra	Nepal	1972	.01785327
Sihanouk	Cambodia (Kampuchea)	1968	.01793536
Castello Branco	Brazil	1964	.01830896
Heng Samrin	Cambodia (Kampuchea)	1980	.01832355
Al-Hafiz	Syria	1966	.01838184
Mohan Rana	Nepal	1951	.01840057
Tribhuvan	Nepal	1952	.01840057
Lopez Mateos	Mexico	1960	.01840355
Prem	Thailand	1981	.0186815
Momoh	Sierra Leone	1987	.0191253
Majano Ramos	El Salvador	1979	.01932179
Kim Il-Sung	Korea, People's Republic of	1992	.01943147
Al-Khatib	Syria	1970	.01945153
Rafsanjani	Iran (Persia)	1990	.01955054
Gursel	Turkey (Ottoman Empire)	1960	.01974123
Najibullah	Afghanistan	1990	.01987418
Ntibantunganya	Burundi	1995	.02050732
Ibn Yahya Hamid	Yemen (Arab Republic of Yemen)	1952	.02082556
Tito	Serbia (Yugoslavia)	1953	.02117422
Senghor	Senegal	1962	.02129216
Lamizana	Burkina Faso (Upper Volta)	1966	.02136134
Saud	Saudi Arabia	1961	.02158138
Obote	Uganda	1969	.02182113

Table A.16: Leader observations matched to Amin (pscore 0.022)

leader	*country*	*year*	*pscore*
Kountche	Niger	1974	.01713455
Magloire	Haiti	1952	.01766939
Ankrah	Ghana	1967	.01766956
Diori	Niger	1961	.01769119
Birendra	Nepal	1972	.01785327
Sihanouk	Cambodia (Kampuchea)	1968	.01793536
Castello Branco	Brazil	1964	.01830896
Heng Samrin	Cambodia (Kampuchea)	1980	.01832355
Al-Hafiz	Syria	1966	.01838184
Mohan Rana	Nepal	1951	.01840057
Tribhuvan	Nepal	1952	.01840057
Lopez Mateos	Mexico	1960	.01840355
Prem	Thailand	1981	.0186815
Momoh	Sierra Leone	1987	.0191253
Majano Ramos	El Salvador	1979	.01932179
Kim Il-Sung	Korea, People's Republic of	1992	.01943147
Al-Khatib	Syria	1970	.01945153
Rafsanjani	Iran (Persia)	1990	.01955054
Gursel	Turkey (Ottoman Empire)	1960	.01974123
Najibullah	Afghanistan	1990	.01987418
Ntibantunganya	Burundi	1995	.02050732
Ibn Yahya Hamid	Yemen (Arab Republic of Yemen)	1952	.02082556
Tito	Serbia (Yugoslavia)	1953	.02117422
Senghor	Senegal	1962	.02129216
Lamizana	Burkina Faso (Upper Volta)	1966	.02136134
Saud	Saudi Arabia	1961	.02158138
Obote	Uganda	1969	.02182113
Naguib	Egypt	1953	.02197862
Ntare	Burundi	1966	.02209019
Deby	Chad	1991	.02233039
Costa Gomes	Portugal	1975	.02233339
Nasser	Egypt	1954	.02280031
Nabiyev	Tajikistan	1992	.02285616
Boumedienne	Algeria	1967	.02361141
Paul Kagame	Rwanda	1995	.02392355
Mainassara	Niger	1998	.02417588
Franjieh	Lebanon	1975	.02432686
Duvalier, Jean-	Haiti	1973	.02466251
Khan Noon	Pakistan	1958	.02484569
Georghiu-Dej	Rumania	1952	.0260192
Le Duan	Vietnam, Democratic Republic of	1977	.02650094
Abacha	Nigeria	1993	.02664866
Keita	Mali	1965	.02672435
Hee Park	Korea, Republic of	1961	.02672497
Rhee	Korea, Republic of	1954	.02693352
Mejia Victores	Guatemala	1984	.02693402
Dos Santos	Angola	1995	.02699283
Ahidjo	Cameroon	1980	.02702871
Moi	Kenya	1982	.02702873
Yameogo	Burkina Faso (Upper Volta)	1962	.02756293

Table A.17: Leader observations matched to Milosevic (pscore 0.031)

leader	*country*	*year*	*pscore*
Saud	Saudi Arabia	1961	.02158138
Obote	Uganda	1969	.02182113
Naguib	Egypt	1953	.02197862
Ntare	Burundi	1966	.02209019
Deby	Chad	1991	.02233039
Costa Gomes	Portugal	1975	.02233339
Nasser	Egypt	1954	.02280031
Nabiyev	Tajikistan	1992	.02285616
Boumedienne	Algeria	1967	.02361141
Paul Kagame	Rwanda	1995	.02392355
Mainassara	Niger	1998	.02417588
Franjieh	Lebanon	1975	.02432686
Duvalier, Jean-	Haiti	1973	.02466251
Khan Noon	Pakistan	1958	.02484569
Georghiu-Dej	Rumania	1952	.0260192
Le Duan	Vietnam, Democratic Republic of	1977	.02650094
Abacha	Nigeria	1993	.02664866
Keita	Mali	1965	.02672435
Hee Park	Korea, Republic of	1961	.02672497
Rhee	Korea, Republic of	1954	.02693352
Mejia Victores	Guatemala	1984	.02693402
Dos Santos	Angola	1995	.02699283
Ahidjo	Cameroon	1980	.02702871
Moi	Kenya	1982	.02702873
Yameogo	Burkina Faso (Upper Volta)	1962	.02756293
Traore	Mali	1991	.02823241
Oueddei	Chad	1980	.02973791
AL-Sallal	Yemen (Arab Republic of Yemen)	1962	.029796
Sarit	Thailand	1958	.03015962
Rios Montt	Guatemala	1983	.0301985
Rawlings	Ghana	1984	.03122675
Rakhmonov	Tajikistan	1997	.03212028
Ben Ali Bourguiba	Tunisia	1962	.03332228
Ayub Khan	Pakistan	1959	.03382697
Obasanjo	Nigeria	1976	.03385386
Burhanuddin Rabbani	Afghanistan	1996	.03451655
Sanya	Thailand	1973	.03529962
Roberto Urdaneta Arbelaez	Colombia	1952	.03535698
Duvalier, Francois	Haiti	1968	.03543226
Koroma	Sierra Leone	1997	.03584538
Hassan II	Morocco	1971	.03608992
Taraki	Afghanistan	1978	.03680521
Ahmed	Bangladesh	1990	.03778442
Taylor	Liberia	2003	.03855642
Moshtaque Ahmed	Bangladesh	1975	.03927835
Lon Nol	Cambodia (Kampuchea)	1970	.04014765
Banda	Malawi	1967	.04019502
Sardar Mohammad Daud Khan	Afghanistan	1955	.04022341
Mahmud Khan Ghazi	Afghanistan	1952	.04034081
Ndadaye	Burundi	1993	.04036791

Table A.18: Leader observations matched to Gowon (pscore 0.046)

leader	*country*	*year*	*pscore*
Yameogo	Burkina Faso (Upper Volta)	1962	.02756293
Traore	Mali	1991	.02823241
Oueddei	Chad	1980	.02973791
AL-Sallal	Yemen (Arab Republic of Yemen)	1962	.029796
Sarit	Thailand	1958	.03015962
Rios Montt	Guatemala	1983	.0301985
Rawlings	Ghana	1984	.03122675
Rakhmonov	Tajikistan	1997	.03212028
Ben Ali Bourguiba	Tunisia	1962	.03332228
Ayub Khan	Pakistan	1959	.03382697
Obasanjo	Nigeria	1976	.03385386
Burhanuddin Rabbani	Afghanistan	1996	.03451655
Sanya	Thailand	1973	.03529962
Roberto Urdaneta Arbelaez	Colombia	1952	.03535698
Duvalier, Francois	Haiti	1968	.03543226
Koroma	Sierra Leone	1997	.03584538
Hassan II	Morocco	1971	.03608992
Taraki	Afghanistan	1978	.03680521
Ahmed	Bangladesh	1990	.03778442
Taylor	Liberia	2003	.03855642
Moshtaque Ahmed	Bangladesh	1975	.03927835
Lon Nol	Cambodia (Kampuchea)	1970	.04014765
Banda	Malawi	1967	.04019502
Sardar Mohammad Daud Khan	Afghanistan	1955	.04022341
Mahmud Khan Ghazi	Afghanistan	1952	.04034081
Ndadaye	Burundi	1993	.04036791
Izetbegovic	Bosnia-Herzegovina	1994	.04059918
Fujimori	Peru	1992	.04133606
Kinigi	Burundi	1994	.04436257
Deng Xiaoping	China	1981	.04642861
Al-Iryani	Yemen (Arab Republic of Yemen)	1967	.04670118
Babangida	Nigeria	1990	.04686154
Hun Sen	Cambodia (Kampuchea)	1991	.04958304
Salem Aref	Iraq	1964	.0496974
Hua Guofeng	China	1976	.05003653
Ngo Dinh Diem	Vietnam, Republic of	1960	.05074912
Nkrumah	Ghana	1962	.05098971
Karrim Kassem	Iraq	1961	.05189723
Smith	Zimbabwe (Rhodesia)	1969	.05329501
al-Khalifa	Sudan	1964	.05380331
Shishakli	Syria	1952	.05418871
Salazar	Portugal	1961	.05605122
Saw Maung	Myanmar (Burma)	1991	.05646395
Habre	Chad	1982	.0575605
Than Shwe	Myanmar (Burma)	1992	.05825007
Laurent Kabila	Congo, Democratic Republic of (Zaire)	1998	.05837314
Nimeiri	Sudan	1973	.0600489
Franco	Spain	1957	.06312947
Meles Zenawi	Ethiopia	1991	.06337283
Al-Assad H.	Syria	1980	.06350393

Table A.19: Leader observations matched to Suharto (pscore 0.050)

leader	*country*	*year*	*pscore*
Oueddei	Chad	1980	.02973791
AL-Sallal	Yemen (Arab Republic of Yemen)	1962	.029796
Sarit	Thailand	1958	.03015962
Rios Montt	Guatemala	1983	.0301985
Rawlings	Ghana	1984	.03122675
Rakhmonov	Tajikistan	1997	.03212028
Ben Ali Bourguiba	Tunisia	1962	.03332228
Ayub Khan	Pakistan	1959	.03382697
Obasanjo	Nigeria	1976	.03385386
Burhanuddin Rabbani	Afghanistan	1996	.03451655
Sanya	Thailand	1973	.03529962
Roberto Urdaneta Arbelaez	Colombia	1952	.03535698
Duvalier, Francois	Haiti	1968	.03543226
Koroma	Sierra Leone	1997	.03584538
Hassan II	Morocco	1971	.03608992
Taraki	Afghanistan	1978	.03680521
Ahmed	Bangladesh	1990	.03778442
Taylor	Liberia	2003	.03855642
Moshtaque Ahmed	Bangladesh	1975	.03927835
Lon Nol	Cambodia (Kampuchea)	1970	.04014765
Banda	Malawi	1967	.04019502
Sardar Mohammad Daud Khan	Afghanistan	1955	.04022341
Mahmud Khan Ghazi	Afghanistan	1952	.04034081
Ndadaye	Burundi	1993	.04036791
Izetbegovic	Bosnia-Herzegovina	1994	.04059918
Fujimori	Peru	1992	.04133606
Kinigi	Burundi	1994	.04436257
Deng Xiaoping	China	1981	.04642861
Al-Iryani	Yemen (Arab Republic of Yemen)	1967	.04670118
Babangida	Nigeria	1990	.04686154
Hun Sen	Cambodia (Kampuchea)	1991	.04958304
Salem Aref	Iraq	1964	.0496974
Hua Guofeng	China	1976	.05003653
Ngo Dinh Diem	Vietnam, Republic of	1960	.05074912
Nkrumah	Ghana	1962	.05098971
Karrim Kassem	Iraq	1961	.05189723
Smith	Zimbabwe (Rhodesia)	1969	.05329501
al-Khalifa	Sudan	1964	.05380331
Shishakli	Syria	1952	.05418871
Salazar	Portugal	1961	.05605122
Saw Maung	Myanmar (Burma)	1991	.05646395
Habre	Chad	1982	.0575605
Than Shwe	Myanmar (Burma)	1992	.05825007
Laurent Kabila	Congo, Democratic Republic of (Zaire)	1998	.05837314
Nimeiri	Sudan	1973	.0600489
Franco	Spain	1957	.06312947
Meles Zenawi	Ethiopia	1991	.06337283
Al-Assad H.	Syria	1980	.06350393
Mwinyi	Tanzania/Tanganyika	1988	.06496955
Batista	Cuba	1958	.06801001

Table A.20: Leader observations matched to Al-Bashir (pscore 0.055)

leader	*country*	*year*	*pscore*
Ayub Khan	Pakistan	1959	.03382697
Obasanjo	Nigeria	1976	.03385386
Burhanuddin Rabbani	Afghanistan	1996	.03451655
Sanya	Thailand	1973	.03529962
Roberto Urdaneta Arbelaez	Colombia	1952	.03535698
Duvalier, Francois	Haiti	1968	.03543226
Koroma	Sierra Leone	1997	.03584538
Hassan II	Morocco	1971	.03608992
Taraki	Afghanistan	1978	.03680521
Ahmed	Bangladesh	1990	.03778442
Taylor	Liberia	2003	.03855642
Moshtaque Ahmed	Bangladesh	1975	.03927835
Lon Nol	Cambodia (Kampuchea)	1970	.04014765
Banda	Malawi	1967	.04019502
Sardar Mohammad Daud Khan	Afghanistan	1955	.04022341
Mahmud Khan Ghazi	Afghanistan	1952	.04034081
Ndadaye	Burundi	1993	.04036791
Izetbegovic	Bosnia-Herzegovina	1994	.04059918
Fujimori	Peru	1992	.04133606
Kinigi	Burundi	1994	.04436257
Deng Xiaoping	China	1981	.04642861
Al-Iryani	Yemen (Arab Republic of Yemen)	1967	.04670118
Babangida	Nigeria	1990	.04686154
Hun Sen	Cambodia (Kampuchea)	1991	.04958304
Salem Aref	Iraq	1964	.0496974
Hua Guofeng	China	1976	.05003653
Ngo Dinh Diem	Vietnam, Republic of	1960	.05074912
Nkrumah	Ghana	1962	.05098971
Karrim Kassem	Iraq	1961	.05189723
Smith	Zimbabwe (Rhodesia)	1969	.05329501
al-Khalifa	Sudan	1964	.05380331
Shishakli	Syria	1952	.05418871
Salazar	Portugal	1961	.05605122
Saw Maung	Myanmar (Burma)	1991	.05646395
Habre	Chad	1982	.0575605
Than Shwe	Myanmar (Burma)	1992	.05825007
Laurent Kabila	Congo, Democratic Republic of (Zaire)	1998	.05837314
Nimeiri	Sudan	1973	.0600489
Franco	Spain	1957	.06312947
Meles Zenawi	Ethiopia	1991	.06337283
Al-Assad H.	Syria	1980	.06350393
Mwinyi	Tanzania/Tanganyika	1988	.06496955
Batista	Cuba	1958	.06801001
Plaek Pibulsongkram	Thailand	1952	.0714955
Nyerere	Tanzania/Tanganyika	1967	.0718507
Malenkov	Russia (Soviet Union)	1953	.0724378
Rojas Pinillia	Colombia	1953	.07318705
Malloum	Chad	1975	.07334639
Thanin Kraivichien	Thailand	1976	.07339376
Sangad	Thailand	1977	.07585032

Table A.21: Leader observations matched to Kayibanda (pscore 0.073)

leader	*country*	*year*	*pscore*
Izetbegovic	Bosnia-Herzegovina	1994	.04059918
Fujimori	Peru	1992	.04133606
Kinigi	Burundi	1994	.04436257
Deng Xiaoping	China	1981	.04642861
Al-Iryani	Yemen (Arab Republic of Yemen)	1967	.04670118
Babangida	Nigeria	1990	.04686154
Hun Sen	Cambodia (Kampuchea)	1991	.04958304
Salem Aref	Iraq	1964	.0496974
Hua Guofeng	China	1976	.05003653
Ngo Dinh Diem	Vietnam, Republic of	1960	.05074912
Nkrumah	Ghana	1962	.05098971
Karrim Kassem	Iraq	1961	.05189723
Smith	Zimbabwe (Rhodesia)	1969	.05329501
al-Khalifa	Sudan	1964	.05380331
Shishakli	Syria	1952	.05418871
Salazar	Portugal	1961	.05605122
Saw Maung	Myanmar (Burma)	1991	.05646395
Habre	Chad	1982	.0575605
Than Shwe	Myanmar (Burma)	1992	.05825007
Laurent Kabila	Congo, Democratic Republic of (Zaire)	1998	.05837314
Nimeiri	Sudan	1973	.0600489
Franco	Spain	1957	.06312947
Meles Zenawi	Ethiopia	1991	.06337283
Al-Assad H.	Syria	1980	.06350393
Mwinyi	Tanzania/Tanganyika	1988	.06496955
Batista	Cuba	1958	.06801001
Plaek Pibulsongkram	Thailand	1952	.0714955
Nyerere	Tanzania/Tanganyika	1967	.0718507
Malenkov	Russia (Soviet Union)	1953	.0724378
Rojas Pinillia	Colombia	1953	.07318705
Malloum	Chad	1975	.07334639
Thanin Kraivichien	Thailand	1976	.07339376
Sangad	Thailand	1977	.07585032
Andom	Ethiopia	1974	.07641861
Van Thieu	Vietnam, Republic of	1965	.0785563
Neto	Angola	1976	.07917152
Ayatollah Khomeini	Iran (Persia)	1982	.0798302
Mahendra	Nepal	1960	.08345881
Biya	Cameroon	1984	.08508658
Museveni	Uganda	1987	.0865024
Yahya Khan	Pakistan	1971	.08799272
Buyoya	Burundi	2001	.08819561
Kenyatta	Kenya	1969	.0903295
Sattar	Bangladesh	1981	.09398866
Banti	Ethiopia	1975	.09734691
Stalin	Russia (Soviet Union)	1952	.09788647
Chissano	Mozambique	1991	.09993682
Strasser	Sierra Leone	1993	.1003473
Souvanna Phouma	Laos	1973	.10082024
Machel	Mozambique	1976	.10626824

Table A.22: Leader observations matched to Pol Pot (pscore 0.089)

leader	*country*	*year*	*pscore*
Hun Sen	Cambodia (Kampuchea)	1991	.04958304
Salem Aref	Iraq	1964	.0496974
Hua Guofeng	China	1976	.05003653
Ngo Dinh Diem	Vietnam, Republic of	1960	.05074912
Nkrumah	Ghana	1962	.05098971
Karrim Kassem	Iraq	1961	.05189723
Smith	Zimbabwe (Rhodesia)	1969	.05329501
al-Khalifa	Sudan	1964	.05380331
Shishakli	Syria	1952	.05418871
Salazar	Portugal	1961	.05605122
Saw Maung	Myanmar (Burma)	1991	.05646395
Habre	Chad	1982	.0575605
Than Shwe	Myanmar (Burma)	1992	.05825007
Laurent Kabila	Congo, Democratic Republic of (Zaire)	1998	.05837314
Nimeiri	Sudan	1973	.0600489
Franco	Spain	1957	.06312947
Meles Zenawi	Ethiopia	1991	.06337283
Al-Assad H.	Syria	1980	.06350393
Mwinyi	Tanzania/Tanganyika	1988	.06496955
Batista	Cuba	1958	.06801001
Plaek Pibulsongkram	Thailand	1952	.0714955
Nyerere	Tanzania/Tanganyika	1967	.0718507
Malenkov	Russia (Soviet Union)	1953	.0724378
Rojas Pinillia	Colombia	1953	.07318705
Malloum	Chad	1975	.07334639
Thanin Kraivichien	Thailand	1976	.07339376
Sangad	Thailand	1977	.07585032
Andom	Ethiopia	1974	.07641861
Van Thieu	Vietnam, Republic of	1965	.0785563
Neto	Angola	1976	.07917152
Ayatollah Khomeini	Iran (Persia)	1982	.0798302
Mahendra	Nepal	1960	.08345881
Biya	Cameroon	1984	.08508658
Museveni	Uganda	1987	.0865024
Yahya Khan	Pakistan	1971	.08799272
Buyoya	Burundi	2001	.08819561
Kenyatta	Kenya	1969	.0903295
Sattar	Bangladesh	1981	.09398866
Banti	Ethiopia	1975	.09734691
Stalin	Russia (Soviet Union)	1952	.09788647
Chissano	Mozambique	1991	.09993682
Strasser	Sierra Leone	1993	.1003473
Souvanna Phouma	Laos	1973	.10082024
Machel	Mozambique	1976	.10626824
Zia	Pakistan	1977	.10716416
Mobutu	Congo, Democratic Republic of (Zaire)	1960	.11422419
Saddam Hussein	Iraq	1984	.1180095
Ershad	Bangladesh	1982	.12391642
Kasavubu	Congo, Democratic Republic of (Zaire)	1962	.12404231
Marcos	Philippines	1972	.12839896

Table A.23: Leader observations matched to Micombero (pscore 0.120)

leader	*country*	*year*	*pscore*
Salazar	Portugal	1961	.05605122
Saw Maung	Myanmar (Burma)	1991	.05646395
Habre	Chad	1982	.0575605
Than Shwe	Myanmar (Burma)	1992	.05825007
Laurent Kabila	Congo, Democratic Republic of (Zaire)	1998	.05837314
Nimeiri	Sudan	1973	.0600489
Franco	Spain	1957	.06312947
Meles Zenawi	Ethiopia	1991	.06337283
Al-Assad H.	Syria	1980	.06350393
Mwinyi	Tanzania/Tanganyika	1988	.06496955
Batista	Cuba	1958	.06801001
Plaek Pibulsongkram	Thailand	1952	.0714955
Nyerere	Tanzania/Tanganyika	1967	.0718507
Malenkov	Russia (Soviet Union)	1953	.0724378
Rojas Pinillia	Colombia	1953	.07318705
Malloum	Chad	1975	.07334639
Thanin Kraivichien	Thailand	1976	.07339376
Sangad	Thailand	1977	.07585032
Andom	Ethiopia	1974	.07641861
Van Thieu	Vietnam, Republic of	1965	.0785563
Neto	Angola	1976	.07917152
Ayatollah Khomeini	Iran (Persia)	1982	.0798302
Mahendra	Nepal	1960	.08345881
Biya	Cameroon	1984	.08508658
Museveni	Uganda	1987	.0865024
Yahya Khan	Pakistan	1971	.08799272
Buyoya	Burundi	2001	.08819561
Kenyatta	Kenya	1969	.0903295
Sattar	Bangladesh	1981	.09398866
Banti	Ethiopia	1975	.09734691
Stalin	Russia (Soviet Union)	1952	.09788647
Chissano	Mozambique	1991	.09993682
Strasser	Sierra Leone	1993	.1003473
Souvanna Phouma	Laos	1973	.10082024
Machel	Mozambique	1976	.10626824
Zia	Pakistan	1977	.10716416
Mobutu	Congo, Democratic Republic of (Zaire)	1960	.11422419
Saddam Hussein	Iraq	1984	.1180095
Ershad	Bangladesh	1982	.12391642
Kasavubu	Congo, Democratic Republic of (Zaire)	1962	.12404231
Marcos	Philippines	1972	.12839896
Buhari	Nigeria	1985	.1322218
Mengistu Marriam	Ethiopia	1977	.13476937
Khrushchev	Russia (Soviet Union)	1955	.14250956
Ziaur Rahman	Bangladesh	1976	.14392789
Selassie	Ethiopia	1960	.14602386
Ne Win	Myanmar (Burma)	1962	.14778366
Tombalbaye	Chad	1971	.16015122
Sukarno	Indonesia	1960	.16916785
Thanon Kittakachorn	Thailand	1971	.17911089

Table A.24: Leader observations matched to Mao Tse-Tung (pscore 0.225)

leader	*country*	*year*	*pscore*
Marcos	Philippines	1972	.12839896
Buhari	Nigeria	1985	.1322218
Mengistu Marriam	Ethiopia	1977	.13476937
Khrushchev	Russia (Soviet Union)	1955	.14250956
Ziaur Rahman	Bangladesh	1976	.14392789
Selassie	Ethiopia	1960	.14602386
Ne Win	Myanmar (Burma)	1962	.14778366
Tombalbaye	Chad	1971	.16015122
Sukarno	Indonesia	1960	.16916785
Thanon Kittakachorn	Thailand	1971	.17911089
Mobutu	Congo, Democratic Republic of (Zaire)	1965	.18900013

Table A.25: Unmatched leader observations

leader	*country*	*year*	*pscore*
Suazo Cordova	Honduras	1985	.00005786
Azcona Hoyo	Honduras	1986	.00006607
Vacariou	Rumania	1992	.0000713
Hussein Bin Onn	Malaysia	1977	.00013287
Alessandri Rodriguez	Chile	1958	.00015328
Surya Bahadur Thapa	Nepal	1998	.00025155
Lokendra Bahadur Chand	Nepal	1997	.0002564
Plaza Lasso	Ecuador	1952	.00026416
Sher Bahadur Deuba	Nepal	1995	.00027109
Hussein Ibn Talal El-Hashim	Jordan	1991	.00028963
Girija Prasad Koirala	Nepal	1991	.00031024
Krishna Prasad Bhatterai	Nepal	1990	.00031881
Choonhavan	Thailand	1988	.00036716
Ponce Enriquez	Ecuador	1957	.00038976
Velasco Ibarra	Ecuador	1953	.00042368
Berisha	Albania	1996	.00046037
Gonzalez Videla	Chile	1951	.00046245
Ibanez Campo	Chile	1952	.00046245
Ioseliani	Georgia	1992	.00048724
Prio Socarres	Cuba	1951	.00053334
Borislav Paravac	Bosnia-Herzegovina	2003	.0005671
Rahman	Malaysia	1969	.00057329
Sarovic	Bosnia-Herzegovina	2002	.00057465
Radisic	Bosnia-Herzegovina	2001	.00058424
Jelavic	Bosnia-Herzegovina	1999	.00060325
Radisic	Bosnia-Herzegovina	1998	.00063635
Arturo Illia	Argentina	1964	.00066952
Ranariddh	Cambodia (Kampuchea)	1994	.00067234
Aristide	Haiti	2002	.00067695
Razak	Malaysia	1970	.00080003
Molina	El Salvador	1977	.00081369
Mahatir Bin Mohammad	Malaysia	1985	.00082163
Romero Mena	El Salvador	1978	.00082919
Ter-Petrosyan	Armenia	1995	.0009722
Diouf	Senegal	1981	.00097527
Ugarteche	Peru	1960	.00104282
Paz Estenssoro	Bolivia	1961	.00107534
Quadros	Brazil	1961	.00107974
Sanchez Hernandez	El Salvador	1972	.00123761
Akayev	Kyrgyz Republic	1997	.00128683
Rivera	El Salvador	1965	.00132788
Guido	Argentina	1963	.00134814
Paz Garcia	Honduras	1978	.0014101
Namphy	Haiti	1986	.0014661
Godoy	Dominican Republic	1966	.00163221
Serrano Elias	Guatemala	1991	.0016462
Cabral	Dominican Republic	1964	.00165309
Goulart	Brazil	1963	.00169551
Menderes	Turkey (Ottoman Empire)	1955	.00176831

Unmatched leader observations – continued from previous page

leader	*country*	*year*	*pscore*
Castro	Ecuador	1966	.00186192
Gambarov	Azerbaijan	1992	.00188097
Busia	Ghana	1970	.00189539
Castro	Honduras	1975	.00195077
Siphandon	Laos	1998	.00199854
Mohammed Ali	Pakistan	1955	.00207757
Melen	Turkey (Ottoman Empire)	1972	.00208376
Velasco Ibarra	Ecuador	1970	.00216469
Tommy Ray Franks	Iraq	2003	.00216972
Phounsavanh	Laos	1992	.00221515
Figueiredo	Brazil	1980	.00225298
Paulo Muwanga	Uganda	1980	.00226533
Jaruzelski	Poland	1982	.00227671
Muluzi	Malawi	2001	.00233448
Lopez Arellano	Honduras	1974	.00233693
Snegur	Moldova	1992	.00238438
Paz Estenssoro	Bolivia	1955	.00243984
Rahmen Aref	Iraq	1967	.00251955
Chiang Ching-Kuo	Taiwan	1978	.00253889
Shevardnadze	Georgia	1994	.00265134
Stroessner	Paraguay	1977	.00268821
Zine Al-Abidine Ben Ali	Tunisia	1988	.00269722
Chun Doo Hwan	Korea, Republic of	1981	.00279475
Arosemena Monroy	Ecuador	1963	.00289795
Perez Jimenez	Venezuela	1952	.00290593
Mkapa	Tanzania/Tanganyika	1996	.0030897
Arosemena Gomez	Ecuador	1967	.00311415
Choi Kuy Hay	Korea, Republic of	1980	.00317776
Geisel	Brazil	1975	.00324915
Natusch Busch	Bolivia	1979	.00330707
Pereda Asbun	Bolivia	1978	.00331119
Benjedid	Algeria	1980	.00345726
Lopez Portillo	Mexico	1976	.0035644
Husak	Czechoslovakia	1969	.00358691
Paul Bremer	Iraq	2004	.0035877
Yen Chia-Kan	Taiwan	1975	.00365457
Ovando Candia	Bolivia	1969	.00372262
Mubarak	Egypt	1981	.00380128
Pinochet	Chile	1973	.00384063
Guei	Cote d'Ivoire	1999	.00411449
Gierek	Poland	1971	.00417338
Poveda Burbano	Ecuador	1977	.00424534
Surya Bahadur Thapa	Nepal	2004	.00425229
Morales Bermudez	Peru	1975	.00426165
Pierre-Louis	Haiti	1956	.00428274
Bitat	Algeria	1978	.00442607
Alia	Albania	1986	.00462725
Mohammad Reza	Iran (Persia)	1979	.00462967
Torres	Bolivia	1970	.00463467
Hun Sen	Cambodia (Kampuchea)	1997	.00465841

Unmatched leader observations – continued from previous page

leader	*country*	*year*	*pscore*
Frondizi	Argentina	1960	.00472723
Papadopoulos	Greece	1967	.00473793
Leon Carpio	Guatemala	1993	.00476737
Aziz	Saudi Arabia	1952	.00478989
Honecker	German Democratic Republic	1971	.00484169
Barrientos Ortuna	Bolivia	1968	.00484294
de los Santos	Dominican Republic	1963	.00493515
Balaguer	Dominican Republic	1969	.00500919
Sheikh Mujib Rahman	Bangladesh	1974	.00503059
Gomulka	Poland	1957	.00506891
Garcia Meza Tejada	Bolivia	1980	.00509709
Echeverria Alvarez	Mexico	1970	.00510593
Ochab	Poland	1956	.0051586
Al-Hamadi	Yemen (Arab Republic of Yemen)	1974	.0052792
Gutierrez	El Salvador	1980	.00532356
Novotny	Czechoslovakia	1957	.00536024
Abulfaz Elchibey	Azerbaijan	1993	.00536132
Siles Zuazo	Bolivia	1958	.00538312
Pascal-Troillet	Haiti	1991	.00543751
Banzer Suarez	Bolivia	1971	.00551568
Rafel Trujillo	Dominican Republic	1961	.00552402
Buyoya	Burundi	1992	.00560042
Faisal II	Iraq	1958	.00564037
Avril	Haiti	1988	.00572402
Rodriguez Lara	Ecuador	1972	.00581426
Mullah Omar	Afghanistan	2000	.00581914
Diaz Ordaz	Mexico	1965	.00586535
Aramburu	Argentina	1955	.00596667
Elias Hrawi	Lebanon	1989	.00597471
Barrientos Ortuna	Bolivia	1964	.00603284
Aoun	Lebanon	1988	.00612347
Nazimuddin	Pakistan	1951	.00614608
Erim	Turkey (Ottoman Empire)	1971	.00621277
Chiluba	Zambia	1997	.00626597
Velasco Alvarado	Peru	1968	.00632971
Chervenkov	Bulgaria	1952	.00651843
Phieu	Vietnam, Democratic Republic of	1997	.00652392
Cedras	Haiti	1993	.00672502
Abdul-Ilah	Iraq	1952	.00675722
Mugabe	Zimbabwe (Rhodesia)	1995	.00682799
Gemayel, Amin	Lebanon	1983	.00687582
Kadar	Hungary	1957	.00698131
El-Kudsi	Syria	1961	.00704745
Preval	Haiti	2000	.00706162
Abdul Zahir	Afghanistan	1972	.00707388
Bartolome Benoit	Dominican Republic	1965	.00708706
Rakoski	Hungary	1952	.00732143
Saleh al-Hashidi	Yemen (Arab Republic of Yemen)	1978	.00738591
Mohammed V	Morocco	1960	.00758458
Aleman Valdes	Mexico	1951	.00769634

Unmatched leader observations – continued from previous page

leader	*country*	*year*	*pscore*
Nur Ahmad Etemadi	Afghanistan	1967	.00783177
Toure	Guinea	1975	.00784382
Magana Borjo	El Salvador	1983	.00790109
Mohammad Hashim Maiwandwal	Afghanistan	1966	.00790532
Evren	Turkey (Ottoman Empire)	1980	.00803039
Zapotocky	Czechoslovakia	1954	.00804551
Lanusse	Argentina	1971	.00809687
Stevens	Sierra Leone	1978	.00829051
Do Muoi	Vietnam, Democratic Republic of	1991	.00847352
Perez Godoy	Peru	1962	.00850995
Kania	Poland	1981	.00894073
Faisal	Saudi Arabia	1964	.00897773
Nguyen Van Linh	Vietnam, Democratic Republic of	1986	.0090526
Kim Jong-Il	Korea, People's Republic of	1995	.00906779
Afeworki	Eritrea	1994	.00910334
Ulbricht	German Democratic Republic	1960	.00923858
Abubakar	Nigeria	1998	.0093477
Seni Pramoj	Thailand	1975	.0094866
Kriangsak	Thailand	1978	.00952828
Sarkis	Lebanon	1977	.00958328
Ongania	Argentina	1966	.00965944
Bagaza	Burundi	1987	.00966078
Castro	Cuba	1961	.00980941
Pote Sarasin	Thailand	1957	.00984362
Ceausescu	Rumania	1966	.01015093
Acheampong	Ghana	1972	.01058392
Seibou	Niger	1990	.01075667
Daniel Ortega	Nicaragua	1983	.01093185
Eyadema	Togo	1986	.01103562
Kebreau	Haiti	1957	.01123049
Lokendra Bahadur Chand	Nepal	2002	.01125078
Karimov	Uzbekistan	1993	.01160563
Zhivkov	Bulgaria	1965	.01162158
Mwambutsa	Burundi	1965	.01164893
Peron	Argentina	1951	.01172382
Patasse	Central African Republic	1996	.01184723
Campaore	Burkina Faso (Upper Volta)	1990	.01197472
Mohammad Mossadeg	Iran (Persia)	1953	.01240776
El-Atassi, N.	Syria	1967	.01244332
Phomivan	Laos	1990	.01260321
Medici	Brazil	1969	.01270973
J. P. Ouedraogo	Burkina Faso (Upper Volta)	1982	.01282547
Minh	Vietnam, Republic of	1963	.01300623