Charles H. Anderton* and John R. Carter

A New Look at Weak State Conditions and Genocide Risk

Abstract: We present a rational choice model depicting a regime's incentive to allocate resources to fighting rebels and killing civilians when it perceives an internal threat to its political or territorial control. The model guides our empirical inquiry of risk factors for genocide onset. Based on logit methods applied to a pooled sample of 155 countries over the period 1955–2006, we find that key variables highlighted in the theoretical model elevate genocide risk. Specifically, measures of threat, anocratic Polity scores, new state status, and low income significantly increase genocide risk, which we interpret as consistent with weak state perspectives on mass atrocity. Moreover, the threat measures matter even after controlling for internal war and the anocracy result holds even after removing components of the Polity dataset that are “contaminated” with factional violence, including genocide. Extensions of the empirical analysis to a variety of alternative measures, variables, and estimators show the robustness of weak state risk factors for genocide.

Keywords: rational choice theory, genocide, threat, weak state, Polity, anocracy, new state, income

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1 Introduction

Following Raphael Lemkin's (1944) path-breaking work on Nazi atrocities in Europe during World War II, a small number of scholars from history, sociology, psychology, political science, and other disciplines began to try to understand the origins of the Holocaust. Their work laid the foundation for scholarship on additional mass atrocities, which were sometimes compared and contrasted to

***Corresponding author: Charles H. Anderton**, Professor of Economics, College of the Holy Cross, 1 College St., Worcester, MA 01610, USA, Tel.: +508-793-3441, E-mail: canderto@holycross.edu

John R. Carter: Professor Emeritus, College of the Holy Cross, 1 College St., Worcester, MA 01610, USA

the Holocaust, but also studied as stand-alone cases. By the 1980s, a critical body of scholarship on mass atrocities coincided with emerging academic programs on the Holocaust and other genocides, which heralded the establishment of the field of genocide studies. By the 1990s, datasets and statistical studies of mass atrocities began to appear. On the policy front, the Convention on the Prevention and Punishment of the Crime of Genocide was entered into force by the UN General Assembly in 1951. More recently, the 1994 Rwandan genocide and atrocities in Sudan-Darfur in the 2000s heightened the sensitivity of government leaders to genocide prevention. In 2002 the International Criminal Court was established to prosecute individuals for genocide, war crimes, and crimes against humanity, and member states at the UN 2005 World Summit agreed to principles known as the Responsibility to Protect (R2P) meant to safeguard populations from genocide, war crimes, crimes against humanity, and ethnic cleansing.¹

Despite over a half century of scholarship and important public policy initiatives on mass atrocities, there are fewer than three dozen published large-sample statistical studies of genocide and mass killing risk, which is small compared to the well over 500 hundred such studies for interstate conflict, and well over one hundred each for civil war and terrorism.² Moreover, there are very few constrained optimization models of mass atrocity risk and, to the best of our knowledge, few if any guide empirical inquiry into risk factors for genocide. The purpose of this article is to help fill these shortcomings. Specifically, we use a constrained optimization model to identify conditions under which killing civilians would be an “optimal” choice by a regime that perceives an internal threat to its political or territorial control. The model guides our construction of hypotheses about risk factors for genocide onset, which we empirically test using logit methods for a pooled sample of 155 countries over the period 1955–2006.

To preview our main results, the theoretical model and empirical analyses suggest that internal threat against the state; weak state conditions, specifically, poorly developed political institutions, new state status, and low income; and discrimination elevate genocide risk. Especially prominent in the article are efforts to advance the treatment of weak state conditions in empirical studies of genocide risk. We maintain that “intermediate political systems” (i.e., anocracies relative to democracies and autocracies) are critical in understanding genocide risk, but

¹ Meierhenrich (2014, 3–55) provides a rich summary of the development of the field of genocide studies.

² Geller and Singer (1998) summarize risk factors for interstate conflict based on more than 500 empirical studies. For reviews of the civil war empirical literature see Collier and Hoeffler (2007), Dixon (2009), Blattman and Miguel (2010), and Hoeffler (2012), and for the terrorism empirical literature, Enders and Sandler (2011).

previous research has not adequately tested this potential risk factor. Most previous empirical articles on civilian atrocity risk use Polity data to measure a state's political system. This poses a potential problem because components of Polity include measures of factional violence, including genocide. Following Vreeland's (2008) critique of this issue in the civil war literature, we adjust the Polity data to remove the "contaminated" components. Based on the adjusted Polity data, we find strong evidence of an inverted-U relationship between adjusted Polity scores and genocide risk. A second weak state variable, new state status, has been virtually ignored in previous empirical research on genocide risk, but we find that it is a significant and substantive risk factor for genocide. We also find, as do many other empirical studies, that low per capita income correlates to greater civilian atrocity risk, but we add to the literature a new possible explanation for the inverse relationship, namely, that intentional killing of civilians is an inferior input. In addition to our core regressions, we run numerous robustness checks involving alternative measures of our variables, additional variables, and other estimation methods such as pooled logit with cubic polynomial, random effects logit, rare events logit, and Cox proportional hazard. We find that our results are generally robust over these alternatives. In the conclusion we discuss how the risk factors identified here might inform policies to prevent genocide through both short-term actions and longer-term structural change.

2 Overview of the empirical literature

Anderton (2014a,b) briefly reviews rational choice models in the civilian atrocity literature, which will not be repeated here. Instead, we summarize the developing empirical literature on civilian atrocity risk and seriousness. Table 1 provides a summary of 28 published large-sample empirical studies of civilian atrocity risk.³ The first column lists the author(s) while the next three identify each study's dependent variable(s), dependent variable data source, and time period. Subse-

³ Table 1 is close to an exhaustive list of *published* cross-section studies of civilian atrocities at the time of this writing. Excluded are empirical studies of civilian atrocity risk for *specific* countries such as Balcells (2010) and Herreros and Criado (2009) on the 1936–1939 Spanish civil war, Bundervoet (2009) on the October 1993 massacre in Burundi, Fielding and Shortland (2012) on Peru in the 1980s and 1990s, Grandi (2013) on Italy right after World War II, Humphries and Weinstein (2006) on Sierra Leone in the early 2000s, McDoom (2013), Verpoorten (2012a), and Verwimp (2003) on the 1994 Rwandan genocide, and Schneider, Bussmann, and Ruhe (2012) on Bosnia from 1992–1995. For insightful coverage of the nature of micro data and research for a specific genocide (i.e., Rwanda), see Verpoorten (2012b).

Table 1: Selected published large-sample empirical studies of civilian atrocity risk and seriousness.

Author(s)	Dependent Variable (s)	Dependent Variable Dataset	Time Period	(1) Internal Conflict	(2) Democracy/ Autocracy/ Regime	(3) New State	(4) Income per capita ^a	(5) Discrim- ination	(6) Cold War	(7) Trade	(8) Fractionalization/ Polarization
GENOCIDE											
Armstrong and Davenport 2008	Genocide fatalities	Other authors	1946–2000	X	X ^d		X				
Aydin and Gates 2008	Genocide occurrence	Harff (2003)	1955–2011		X				X		X
Besaŋçon 2005	Genocide occurrence and fatalities	PITF-G	1960–1999				X				
Colaresi and Carey 2008	Genocide onset	Harff (2003)	1955–2003	X	X				X		X
Goldsmith et al. 2013	Genocide onset	PITF-G	1988–2003	X	X ^d			X			X
Harff 2003	Genocide onset	Author's own research	1955–1997	X	X				X		X
Kathman and Wood 2011	Genocide fatalities	PITF-G	1955–2005	X	X		X		X		
Krain 1997	Genocide onset, fatalities, duration	Other authors	1948–1982	X	X				X		X
Krain 2005	Genocide fatalities	PITF-G	1955–1997	X	X				X		X
Krain 2012	Genocide fatalities	PITF-G	1976–2008	X	X				X		X
Krain 2014	Genocide fatalities	PITF-G	1976–2008	X	X ^d				X		X

(Table 1: Continued)

Author(s)	Dependent Variable (s)	Dependent Variable Dataset	Time Period	(1) Internal Conflict	(2) Democracy/ Autocracy/ Regime	(3) New State	(4) Income per capita ^a	(5) Discrimination	(6) Cold War	(7) Trade	(8) Fractionalization/ Polarization
Montalvo and Reynal-Querol 2008	Genocide occurrence	Other authors	1960–1999		X		X				X
MASS KILLING											
Easterly, Gatti, and Kurlat 2006	Mass killing occurrence and fatalities	Authors' own research	1820–1998	X	X		X				X
Kim 2010	Mass killing occurrence and fatalities	Author's own research	1945–2007	X	X		X		X		X
Rummel 1995	Democide fatalities	Author's own research	1900–1987	X	X		X				X
Valentino, Huth and Balch-Lindsay 2004	Mass killing occurrence	Authors' own research	1945–2000	X	X						
BOTH GENOCIDE AND MASS KILLING											
Rost 2013	Genocide, democide, mass killing onset	PITF-G, UV, other authors	1976–2000 (various)	X	X ^d	? ^c	X	X	X	X	
Wayman and Tago 2010	Genocide and democide onset	Other authors	1949–1987	X	X						

(Table 1: Continued)

Author(s)	Dependent Variable (s)	Time Period	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variable	Variable Dataset	Internal Conflict	Democracy/ Autocracy/ Regime	New State	Income per capita ^a	Discrimination	Cold War	Trade	Fractionalization/ Polarization
LOWER-LEVEL CIVILIAN VIOLENCE										
DeMeritit 2012	Civilian violence occurrence and fatalities	PITF-A	X	X ^d						
Eck and Hultman 2007	Civilian violence occurrence and fatalities	UCDP-V	X	X ^d				X		
Fjelde and Hultman 2014	Civilian violence intensity	UCDP-GED	X			X				
Hicks et al. 2011	Civilian targeting index ^b	Based on UCDP-V	X							
Hultman 2010	Civilian violence fatalities	UCDP-V	X							
Hultman 2012 ^e	Civilian violence fatalities	UCDP-V	X	X ^d		X				
Querido 2009	Civilian violence occurrence and fatalities	UCDP-V	1989–2005 (Africa)			X				X
Wood 2010 ^e	Civilian violence fatalities	UCDP-V	1989–2004 X	X		X		X		
Wood 2014 ^e	Civilian violence fatalities	UCDP-V	1989–2009 X							X

(Table 1: Continued)

Author(s)	Dependent Variable (s)	Time Period	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Internal Conflict	Democracy/ Autocracy/ Regime	New State	Income per capita ^a	Discrimination	Cold War	Trade	Fractionalization/ Polarization
Wood, Kathman, and Grant 2012 ^e	Civilian violence and fatalities	UCDP-V	1989–2005	X	X	X				

Key: PITF-G=Political instability task force geno-politicide dataset. UV=Ulfelder and Valentino (2008) mass killing dataset.
PITF-A=Political instability task force worldwide atrocities dataset. UCDP-GED=Uppsala conflict data program geo-referenced event data.
UCDP-V=Uppsala conflict data program one-sided violence dataset.
^aSeveral studies do not include income per capita but other proxies for economic development. For example, Wayman and Tago (2010) and Rummel (1995) include energy consumption per capita and Goldsmith et al. (2013) and Harff (2003) have measures of infant mortality. Rost (2013) includes income per capita and infant mortality measures.
^bDefined as an actor's number of intentional killings of civilians divided by total fatalities from battles in which the actor was involved.
^cRost (2013) conducted a test for new states, but the results were not reported and were contrary to the author's hypothesis.
^dTested for inverted U or nonlinear relationship using unadjusted Polity data.
^eSeveral studies focus upon civilian atrocities perpetrated by rebel groups. See, for example, Hultman (2012), Wood (2010, 2014), and Wood, Kathman, and Grant (2012).

quent columns show selected independent variables in which an “X” indicates that the study included the corresponding variable.⁴ Note also that the studies are demarcated along three branches: genocide, mass killing, and lower-level intentional violence against civilians.

Studies belonging to the first branch, labeled under “Genocide,” focus on risk factors for genocide onset, occurrence, fatalities, and/or duration.⁵ These articles are instructive for their variation in samples, genocide measures, and results. For example, Krain (1997) constructs a sample of about 4000 country-year observations for the period 1948–1982 and includes 35 genocides identified in Harff and Gurr (1988). In his logit analysis, Krain discovers that civil war is the strongest risk factor for genocide onset. International war and decolonization also increase risk while concentration of political power, trade, ethnic fractionalization, and extra-constitutional changes have minimal or insignificant effects. A second example is Harff (2003), who employs a case-control design for 126 countries, all of which experienced state failure sometime during the period 1955–1997. Of these failures, 35 led to genocide. Conditioned on state failure, Harff uses logit analysis to identify six key risk factors for genocide: magnitude of political upheaval, history of prior genocide, exclusionary ideology by the ruling elite, autocratic regime, ethnic minority elite, and low trade openness.

Studies along the second branch, labeled under “Mass Killing,” focus on large-scale civilian atrocities in which the victims are not necessarily from groups being targeted for elimination.⁶ For example, Easterly, Gatti, and Kurlat (2006) assemble a dataset spanning 1820–1998, which contains 163 episodes of mass killing in 71 countries. For each country and decade, a dummy variable indicating the occurrence of a mass killing is recorded, along with approximate deaths. The result is 902 observations in the full sample and 743 observations in a sample for the 20th century. For the latter sample, probit analysis indicates that mass

⁴ Table 1 does not provide an exhaustive list of independent variables in the empirical literature, but it does show most of the variables considered as well as the lack of consideration of new state.

⁵ In this article, we use the Political Instability Task Force genocide and politicide dataset, which defines genocide and politicide as: “[E]vents [that] involve the promotion, execution, and/or implied consent of sustained policies by governing elites or their agents – or in the case of civil war, either of the contending authorities – that result in the deaths of a substantial portion of a communal group or politicized non-communal group... In the case of genocide and politicide authorities physically exterminate enough (not necessarily all) members of a target group so that it can no longer pose any conceivable threat to their rule or interests” (Marshall, Gurr, and Harff 2010, 14).

⁶ Waller (2007, 14), for example, defines mass killing as “killing members of a group without the intention to eliminate the whole group or killing large numbers of people without a precise definition of group membership.”

killings are significantly less likely for countries in the top quartiles of income and democracy. For the full sample, there is also evidence that the risk of mass killing is higher during war and at intermediate levels of ethnic fractionalization. Another example along the second branch is Valentino, Huth, and Balch-Lindsay (2004), who consider why some wars lead to mass killings and others do not. The authors build a dataset for the period 1945–2000 encompassing 147 wars (inter-state, intrastate, and extra-state) and 30 mass killings during war. Logit analysis reveals that mass killing occurrence is positively related to the presence of guerilla war (as distinct from other types of war), magnitude of guerilla threat, and degree of civilian support for guerillas, and is negatively related to democracy.

The third branch in Table 1 includes studies of civilian atrocity occurrence or seriousness in which the atrocity datasets include many incidents of lower-level violence.⁷ For example, DeMeritt (2012) finds that naming and shaming activities of international organizations against civilian-abusing governments can lower the risk and severity of civilian violence. Another example is Hultman (2010), who finds that there is little evidence that peace operations dampen government killing of civilians and, disconcertingly, the presence of peacekeepers correlates to greater violence against civilians by rebel groups.

Our study clearly lies along the first of the three branches of Table 1 by focusing strictly on genocide. In the next section, we summarize the theoretical model that guides our selection of key variables and empirically-testable hypotheses. In the subsequent two sections we present data sources and apply logit analysis to identify significant risk factors for genocide onset.

3 A constrained rational choice model and hypotheses of genocide risk

3.1 “Rationalist” motives for killing civilians

Assume a threatened regime “produces” a certain amount of political or territorial control (designated by Q) by using two “inputs”: fighting rebels (R) and killing civilians (C). It is clear that the threatened regime can attempt to achieve control (Q) by directly contesting the rebels (R), but why might it intentionally kill civilians (C)? Scholars have pointed to several answers to this question, the most

⁷ The Uppsala Conflict Data Program, for example, defines one-sided violence against civilians as “the use of armed force by the government of a state or by a formally organized group against civilians which results in at least 25 deaths” (Eck and Hultman 2007; Pettersson 2012, 2).

prominent being that during intrastate war or other internal crises, civilians can provide critical resources to rebels such as financing, information, personnel, and safe havens (Kalyvas 1999; Valentino 2004, ch.3; Valentino, Huth, and Balch-Lindsay 2004, 383–385). By attacking civilians, a threatened regime may perceive that it is undermining a key source of support for the rebels. A related motive for a regime to attack (or threaten to attack) civilians is to prevent them from defecting to the rebels and instead to support the regime. This tactic, of course, can backfire and drive civilians to support the rebels, but this possibility raises the risk that civilians will be attacked. This is especially true in areas where *the regime is losing territorial control*. The loss of territory means the loss of a critically valuable resource in that territory, namely, civilians. It is a “dual loss” because the civilians are lost to the regime and they come to be controlled by the enemy and thus leveraged against the regime. Like the retreating general who destroys oil wells and other assets to deny them to the enemy, so can a retreating regime contemplate the mass destruction of civilians. Another related motive to destroy civilians is to maximize resource extraction and/or create political or ideological homogeneity within geographic areas that are deemed strategically significant to the regime in its contest with actual or possible future rebels (Valentino 2004, ch. 3).

3.2 Optimal choice and demand conditions for killing civilians

Anderton (2014a) uses the following specific functional form model to simulate conditions under which a threatened regime would choose to kill civilians:

$$\begin{aligned} \max_{C, R} Q &= A\sqrt{C} + \left(\frac{R^\delta}{\delta}\right) A > 0, \delta > 1, \\ \text{s.t. } I &= P_c C + P_r R \end{aligned} \quad (1)$$

where A is a productivity parameter for killing civilians (C), δ is a productivity parameter for fighting rebels (R), I is the regime’s resources or income, and P_c and P_r are the respective costs or prices of killing civilians and contesting rebels. In their civil war study, Fearon and Laitin (2003) identify three variables that characterize weak states: (1) anocracy, (2) new state status, and (3) low income. In the model in (1), income is present in the form of parameter I . The other two weak state variables, anocracy and new state status, can be theoretically connected to the productivity parameter δ . States that are institutionally strong will be effective in contesting rebels, everything else the same, which will be reflected in a relatively high δ value. Institutionally weak states, however, will have a relatively low δ value.

Table 2 shows simulations of (1) for specific values for A , δ , I , P_c , and P_r . The table illustrates “optimal” choice or demand conditions for killing civilians. Rows

1–4 show that greater institutional weakness of the regime (lower δ), increases the quantity demanded for civilian killing (C^*). Rows 3, 5, and 6 show that lower income (I) and output (Q) correlate to a greater quantity demanded for civilian killing. Hence, civilian killing is an inferior input for the parameter values in Table 2. Rows 3 and 7–9 show the “law of demand,” namely, that an decrease in the cost or price of killing civilians (P_c) leads to an increase in C^* . Rows 3 and 10–12 show that a higher cost or price of contesting rebels leads to substitution into civilian killing such that C^* rises. More generally, rows 3 and 7–12 imply that killing civilians and contesting rebels are gross substitutes for the parameter values shown because $\frac{\partial C^*}{\partial P_r} > 0$ and $\frac{\partial R^*}{\partial P_c} > 0$.

3.3 Hypotheses of genocide risk

The model in (1) and the results in Table 2 suggest empirically testable hypotheses of genocide risk. The starting point of the theoretical model is a regime’s perception of threat to its political or territorial control. Such losses can be perceived as highly deleterious to authorities because they may lose most or all of their privileges and face retribution (including incarceration or execution) following a loss of power. It is the perception of loss of control by an authority group that activates the constrained rational choice model in (1) (i.e., brings it into being) and, in so

Table 2: Simulations of the rational choice model.

Row	Killing civilians productivity parameter A	Contesting rebels productivity parameter δ	Income I	Cost or price of killing civilians P_c	Cost or price of contesting rebels P_r	Optimal civilian killing C^*	Optimal rebel contestation R^*	Optimal political or territorial control Q^*
1	5	2.00	10	1.0	1.0	0.06	9.94	50.63
2	5	1.75	10	1.0	1.0	0.20	9.80	33.25
3	5	1.50	10	1.0	1.0	0.67	9.33	23.09
4	5	1.25	10	1.0	1.0	2.24	7.76	17.84
5	5	1.50	8	1.0	1.0	0.88	7.12	17.36
6	5	1.50	6	1.0	1.0	1.34	4.66	12.49
7	5	1.50	10	0.9	1.0	0.83	9.25	23.32
8	5	1.50	10	0.8	1.0	1.07	9.15	23.61
9	5	1.50	10	0.7	1.0	1.42	9.01	23.98
10	5	1.50	10	1.0	1.1	0.92	8.26	20.61
11	5	1.50	10	1.0	1.2	1.23	7.31	18.72
12	5	1.50	10	1.0	1.3	1.64	6.43	15.81

doing, brings the possibility of civilian killing into the authority group's calculations. Hence, everything else the same, we hypothesize:

H1: The greater the threat against a state's political authorities, the greater the risk of genocide onset.

As noted in rows 1–4 of Table 2, as the regime's productivity parameter δ declines (i.e., it becomes weaker institutionally vis-à-vis rebels), it chooses more killing of civilians, everything else the same. According to Fearon and Laitin (2003), anocracies will be institutionally weak relative to autocracies and democracies. Hence, Table 2 suggests an inverted-U relationship between political system and killing civilians in which “intermediate political systems”, i.e., anocracies, will be at greater risk than autocracies and democracies, everything else the same:

H2: Anocracy relative to autocracy and democracy will increase the risk of genocide onset (inverted-U hypothesis).

New state status also suggests institutional weakness and a correspondingly high risk for civilian killings. For example, Fearon and Laitin (2003) maintain that new states tend to have untested military forces, which means that they would be relatively unproductive in contesting rebels should war come. Moreover, new states may be recognized as states by international organizations, but not necessarily by population groups in regions that were opposed to the current group before it came into power (Hironaka 2008, 75). Furthermore, according to Rotberg (2003, 19), new states often do not have a large number of experienced professionals and bureaucrats within the regime, thus making them vulnerable to external shocks and internal crises. Everything else the same, we hypothesize:

H3: New state status will increase the risk of genocide onset.

Table 2 also shows an inverse relationship between income/output and civilian killing in rows 3, 5, and 6. Hence, everything else the same, we hypothesize:

H4: Low income will increase the risk of genocide onset.

The purpose of our empirical inquiry is, conditional on threat, to focus upon weak state conditions and genocide risk, but we also want to consider other independent variables noted in Table 1. Another important variable to consider when assessing genocide risk is discrimination. Genocide scholars have shown how people from an in-group can come to disassociate with and dehumanize people from an out-group through severe discrimination (Fein 1977, 1979, 1993; Waller 2007). Such “othering” of an out-group can have a variety of possible impacts on the authority group's rational choice calculations modeled in (1). For example, an aggrieved group might be prone to start a rebellion against the state or join a rebellion already under way. This could raise the cost or price to the state of

contesting rebels, which could lead to more civilian killing, everything else the same, which was shown in rows 3 and 10–12 of Table 2. Furthermore, discrimination could affect an authority group's price of killing civilians. If a potentially targeted civilian group faces substantial discrimination, it would be relatively easy for the authorities to recruit perpetrators against the out-group, everything else the same. The inverse relationship between the cost of killing civilians and the "optimal" amount of civilian killing is demonstrated in rows 3 and 7–9 of Table 2. We hypothesize:

H5: Greater discrimination will increase the risk of genocide onset.

The cost or price of killing civilians may have been relatively low during the Cold War owing to the superpower stalemate. In the post-Cold War period, the price of killing civilians may be higher, everything else the same. For example, new anti-atrocity institutions emerged in the post-Cold War period (e.g., International Criminal Court, R2P) and about 75% of all UN peace missions began after 1989, which encompasses only about 33% of the years in which the UN has existed (Anderton 2014a). We hypothesize:

H6: The Cold War period will correlate to a greater risk of genocide onset.

Two other variables in Table 1, trade and ethnic fractionalization/polarization, have received mixed results in empirical studies of civilian atrocities. Hence, we consider them in our extended analysis in Section 6. Because countries in the international system vary widely in size, we include population as a control variable in all our regressions. We make no hypothesis about the relationship between genocide onset and population but note that population has been a control variable in other mass atrocity empirical studies (e.g., Aydin and Gates 2008; Colaresi and Carey 2008; Easterly, Gatti, and Kurlat 2006; Kim 2010; Krain 2012, 2014; Valentino, Huth, and Balch-Lindsay 2004).⁸

4 Empirical methods

4.1 Model and variables

Guided by the discussion in the previous section, we specify our initial empirical model as:

⁸ Some authors hypothesize a positive relationship between population and mass atrocity risk or seriousness. Krain (2014, 36), for example, indicates that a large population can strain a state's resources and present a greater number of people that can be killed.

$$\begin{aligned} \text{Prob}(\text{Onset}=1) = & \Lambda(\beta_1 + \beta_2 \text{Population} + \beta_3 \text{Threat} + \beta_4 \text{Monopoly_Control} + \\ & + \beta_5 \text{Monopoly_Control_Squared} + \beta_6 \text{New_State} + \beta_7 \text{Income} \\ & + \beta_8 \text{Discrimination} + \beta_9 \text{Cold_War}), \end{aligned} \quad (2)$$

where Λ is the logistic cumulative distribution function defined on country-year observations.⁹ As has been learned in the more fully developed civil war literature, occurrence as the dependent variable conflates issues of onset and termination. We adopt the country-year as our unit of analysis, rather than regimes and/or longer time intervals, which also mirrors successful practice in civil war research. And we use 1955–2006 for our time frame, which carries us as close to the present as our data sources permit. Here we indicate the measures for the variables in (2), highlighting alternative measures later as needed. Table 3 presents descriptive statistics.

Table 3: Descriptive statistics.

Variable	Mean (Std. Dev.)	Minimum	10th percentile	Median	90th percentile	Maximum
Genocide onset	0.005 (0.073)	0	0	0	0	1
Population	2.155 (1.494)	−1.490	0.245	2.075	4.072	7.173
Threat	0.239 (0.755)	0	0	0	1	4
Monopoly control (Adjusted Polity)	5.627 (4.913)	0	0	6	12	13
New state	0.050 (0.218)	0	0	0	0	1
Income	1.312 (1.265)	−1.729	−0.340	1.271	3.078	4.401
Discrimination	2.270 (1.670)	0	0	3	4	4
Political discrimination	2.110 (1.659)	0	0	3	4	4
Economic discrimination	1.878 (1.579)	0	0	2	4	4
Cold war	0.562 (0.496)	0	0	1	1	1
Openness	60.762 (41.101)	1.480	18.350	52.730	111.970	428.950
Fractionalization	0.451 (0.258)	0	0.094	0.484	0.787	0.900
Polarization	0.558 (0.245)	0	0.184	0.628	0.851	0.968
War	0.115 (0.319)	0	0	0	1	1
Real Growth	0.042 (0.060)	−0.661	−0.013	0.042	0.097	1.063
Internet	2.981 (10.393)	0	0	0	6.131	104.891

N=5913 except for *Fractionalization* and *Polarization* (5897), *Internet* (5843), and *Real Growth* (5457). Definitions and sources are indicated in the text.

⁹ Most other empirical studies of civilian atrocity occurrence or onset have used logit (see, e.g., Aydin and Gates 2008; Colaresi and Carey 2008; Eck and Hultman 2007; Kathman and Wood 2011; Kim 2010; Krain 1997, 2005, 2012, 2014; Montalvo and Reynal-Querol 2008; Rost 2013; Valentino, Huth, and Balch-Lindsay 2004).

Onset: Our dependent variable is the probability of genocide onset, where *Onset* equals one in the initial year of a genocide and zero otherwise. We adopt the list of 41 genocides and politicides compiled by the Political Instability Task Force (PITF) in its State Failure Problem Set 1955–2010 (Marshall, Gurr, and Harff 2010). PITF takes care to distinguish analytically between civil wars and genocides, where the latter are characterized by “authorities’ systematic targeting of noncombatants” (Marshall, Gurr, and Harff 2010, 15). Since PITF includes both genocides and politicides in the same dataset and within the same definition (as shown in footnote 5), we make no distinction between genocides and politicides in our coding. This practice has been followed by others in the empirical literature who have used either Harff’s (2003) list of genocides and politicides or the PITF geno-politicide dataset (e.g., Aydin and Gates 2008; Besançon 2005; Colaresi and Carey 2008; Goldsmith et al. 2013; Kathman and Wood 2011; Krain 2005, 2012, 2014; Rost 2013). From this point forward we use the term “genocide” to encompass both genocides and politicides.

Population: Because countries in the international system vary widely in size, we include a measure of population as a simple control variable. Our measure *Population* is drawn from the Penn World Table (PWT) Version 7.1 (Heston, Summers, and Aten 2012) and is defined as the natural logarithm of population scaled in millions.

Threat: For a measure of threat, we turn again to PITF’s State Failure Problem Set 1955–2010, which gauges the magnitude of revolutionary and ethnic wars on a country-year basis. This source is suitable for several reasons. First, as noted above, PITF is deliberate in its analytical distinction between civil war and genocide. Second, PITF applies lower and hence more inclusive selection thresholds, meaning that PITF identifies more intrastate conflicts than do other prominent sources. And third, PITF estimates the *magnitude* of conflict over a larger range (five-point scale) relative to other major conflict data sources. This latter point is relevant when considering, not just the presence, but also the *magnitude* of potential threat against government authorities. As our results later will reveal, our measure of threat remains significant, even after controlling for the presence of war.

For each year of a revolutionary or ethnic war, PITF codes a five-point scale called *Magarea* based on estimates of the “portion of [a] country affected by fighting.” The scale takes on values of 0 (less than one-tenth of the country and no significant cities directly or indirectly affected), 1 (one-tenth of the country and/or one or several provincial cities directly or indirectly affected), 2 (more than one-tenth and up to one quarter of the country and/or the capital city directly or indirectly affected), 3 (from one-quarter to one-half the country and/or most

major urban areas directly or indirectly affected), and 4 (more than one-half the country directly or indirectly affected). From these data we construct our measure *Threat*. In cases of no war, we impute the value of zero; in cases of either revolutionary or ethnic war alone, we set *Threat* equal to the Magarea value; where both war types occur simultaneously, we set the measure equal to the maximum of the two Magarea values. For robustness, we also consider PITF's equivalently-constructed Magfight measure, which gauges the magnitude of combatants or activists opposing the state. By H1, we expect *Threat* to have a positive effect on the probability of genocide onset.

Monopoly control and monopoly control squared: Our measure of political control ultimately draws from the Polity IV Project: Political Regime Characteristics and Transitions, 1800–2010 (Marshall, Gurr, and Jaggers 2010). We begin with the widely used composite measure Polity2, which is a 21-point scale that ranges from –10 for full autocracy to +10 for full democracy. Polity2 is described in the user manual as a combined score revised such that cases of foreign interruption (with standardized authority score –66) are treated as missing, cases of interregnum or anarchy (–77) are set to zero, and cases of transition (–88) are interpolated when feasible. We make three modifications to the Polity score. First, we incorporate additional Polity2 observations available in Gleditsch (2008), where some missing scores are estimated, extrapolated, or drawn from an earlier version of Polity. Second, we modify the Polity2 protocol by treating cases of interregnum or anarchy (–77) as missing rather than imputing a neutral score of zero. Third, to facilitate interpretation as a measure of monopoly control, we simply translate the Polity2 score so that it ranges from 0 for full democracy to 20 for full autocracy.

In an important critique, Vreeland (2008) shows that use of Polity data can be problematic because two of its five underlying components are sensitive to observed civil war or genocide. Specifically, Polity2 data contain an aggregation of five component scores pertaining to executive power (XCONST, XRCOMP, and XROPEN) and political participation (PARREG and PARCOMP) as shown in the Polity codebook (Marshall, Gurr, and Jaggers 2010). Vreeland shows that civil war or genocide enters the coding of the two participation components. For PARREG, for example, Vreeland quotes from an earlier codebook that under the category of factional/restricted are “polities in which political groups are factional but policies of genocide or politicide are routinely carried out against significant portions of the population” (Gurr 1990, 17). Moreover, such instances of political violence are assigned a coded numerical value toward the middle of the range of PARREG and PARCOMP, possibly generating a spurious inverted-U relationship between Polity and political violence. Vreeland shows that this can

be avoided by constructing an alternative index based only on the three executive components.

Following Vreeland, we construct an alternate measure by summing the remaining three uncontaminated Polity2 components and then applying the same protocols previously described for the Polity2 data. The resulting measure, labeled *Monopoly Control*, gauges monopoly power of the executive and ranges from 0 for perfect democracy to 13 for perfect autocracy. Our reconstructed Polity measure removes the contaminated elements and it focuses the measure on the executive decision-makers within the state because the three remaining components have to do with executive power. By H2, we expect *Monopoly Control* to display an inverted-U relationship in which anocracy shows a higher risk of genocide than autocracy and democracy.

We believe that our use of adjusted Polity data is important because use of contaminated Polity is widespread in the empirical civilian atrocities literature. Of the 22 empirical studies listed in Table 1 which include one or more measures of political regime, 20 use unadjusted Polity scores.¹⁰ Moreover, to the best of our knowledge none of the empirical studies in the civilian atrocity literature test for an inverted-U relationship between political regime and civilian atrocities using uncontaminated Polity data. Aydin and Gates (2008) identify the contamination issue and adjust for it by using uncontaminated components of Polity and Vanhanen's data on electoral participation, but they do not test for an inverted-U relationship between political regime and civilian atrocity risk.

New State: As we will show, our sample is an unbalanced panel of nations, many of which enter as new states. To allow for this entry, we define *New State* as a dummy variable equal to one if a country's age is 5 years or less and equal to zero otherwise. To the best of our knowledge, Rost (2013) is the only published article in the empirical genocide literature that directly tests for the effects of new states but he does not report his methods or empirical results. Rost indicates in an appendix that the *absence* of new states correlated to a *greater* genocide risk. By H3, however, we expect *New State* to have a positive effect on genocide onset.

Income: Our measure labeled *Income* is drawn directly from the PWT 7.1 and is the natural logarithm of real GDP per capita in thousands of chained 2005

¹⁰ Rummel (1995) uses non-Polity sources to measure political regime. Also, Colaresi and Carey's (2008) initial logit model includes only XCONST, which is uncontaminated. Similar to Aydin and Gates (2008), Rummel and Colaresi and Carey do not test for an inverted-U relationship between regime characteristics and genocide.

international dollars. By H4, we expect *Income* to have a negative effect on genocide onset.¹¹

Discrimination: We derive our measure of discrimination from the Minorities at Risk (MAR) Dataset (Minorities at Risk Project 2009). MAR tracks through 2006 the annual experience of close to three hundred communal groups in countries with populations of at least 500,000. For a given country-year, MAR determines five-point indexes of political and economic discrimination for each qualifying group. Political discrimination pertains to “exercise of political rights or access to political positions,” while economic discrimination refers to “access to desirable economic goods, conditions, or positions” (Gurr 2000, 109, 111). The values of the political index are 0 (no discrimination), 1 (under-representation due to historical neglect or restrictions but with remedial policies), 2 (under-representation due to historical neglect or restrictions but without remedial policies), 3 (under-representation due to dominant social practices but with neutral policies), and 4 (formal policies of exclusion and repression). The economic index is defined in a commensurate manner. For our purposes, it is worth noting that the index value of 4 “does not include repression during group rebellions” but “does include patterned repression when the group is not openly resisting state authority” (Codebook, 11).

From these data we define the measure *Discrimination* as follows. For a given country-year, if MAR shows no communal group at risk, then our measure is imputed the value zero; otherwise, it is assigned the highest level of political or economic discrimination determined by MAR for any qualifying group. Notice that we make no attempt to match a particular group in MAR with the victim group in a genocide case. Hence *Discrimination* measures the highest level of discrimination evidenced in a country against *any* group. We prefer this approach to measuring discrimination because we seek to capture the general discriminatory (“othering”) capacity of the state regardless whether groups facing discrimination

¹¹ Many other empirical studies of genocide risk control for income (see the Income column in Table 1). Most studies that include income report at least some negative and statistically significant coefficient estimates (e.g., Armstrong and Davenport 2008; Besançon 2005; Easterly, Gatti, and Kurlat 2006; Fjelde and Hultman 2014; Kathman and Wood 2011; Kim 2010; Montalvo and Reynal-Querol 2008; Querido 2009; Wood 2010), although Rost (2013) found that inclusion of income did not improve the predictive power of the model. Easterly, Gatti, and Kurlat (2006) found an inverted-U relationship between income and mass killing. In results available from the corresponding author, we did not find evidence of an inverted-U relationship between income and genocide onset.

become victims of genocide. By H5, we expect *Discrimination* to have a positive effect on genocide onset.¹²

Cold War: To allow for a systemic change in international relations, we define *Cold War* as a dummy variable equal to one for all years through 1989 and zero thereafter. By H6 we expect *Cold War* to have a positive impact on genocide onset.¹³

4.2 Sample and estimation

Sample construction begins with 178 independent states identified by Gleditsch and Ward (1999). The initial temporal range is 1955 through 2009 based on the coverage in the PITF list of genocides. To avoid conflating issues of onset and duration, we drop from the sample all country-years that involve ongoing genocides. The sample is further reduced by missing observations associated with particular measures included in the regressions. The resulting basic sample consists of 5913 observations involving 155 countries spanning the years 1955 through 2006. Included are 32 genocide onsets occurring in 24 countries.¹⁴ We structure the sample as an unbalanced panel and then estimate the model using logit regression. To reduce simultaneity issues, all right-hand variables except *New State* and *Cold War* are lagged 1 year.

12 Few published empirical studies of genocide risk include direct measures of discrimination. A notable exception is Rost (2013) who, like us, uses MAR data to measure political and economic discrimination. Our approaches and results differ. Rost includes mean values for political and economic discrimination against all groups in a country and dummy variables for high (3 or 4 on the MAR scale) political and economic discrimination. Instead, we select the maximum values of economic and political discrimination to represent the highest potential of a state's discrimination. Furthermore, we include a general measure of discrimination as well as the measures for economic and political discrimination. Rost finds that his measures of discrimination reduce the predictive power of his forecasting model. Our results reported below show that discrimination, and especially economic discrimination, are positive, significant, and robust to the inclusion of other variables and estimation techniques.

13 Table 1 shows seven empirical studies that consider Cold War. Krain's (2005, 2012, 2014) Cold War dummies are insignificant for genocide fatalities, while Kathman and Wood (2011) and Wood (2010) find Cold War to be negatively (and sometimes significantly) associated with genocide fatalities. Kim (2010) and Rost (2013) report positive and significant effects of the Cold War period on mass killing occurrence and genocide onset, respectively.

14 Our 32 onsets are Afghanistan (1978), Angola (1998), Argentina (1976), Burundi (1965, 1988, 1993), Cambodia (1975), Chile (1973), China (1959, 1966), Democratic Republic of Congo/Zaire (1964, 1977), El Salvador (1980), Ethiopia (1976), Guatemala (1978), Indonesia (1965, 1975), Iran (1981), Iraq (1988), Nigeria (1967), Pakistan (1971, 1973), Philippines (1972), Rwanda (1963,

5 Empirical results

5.1 Basic model

The estimated coefficients and cluster-robust standard errors for our basic model are reported in column (1) of Table 4. For convenience we show two-sided p-values, noting however that most of our hypothesis tests are properly one-sided. Consistent with our expectations, the measures for *Threat*, *New State*, *Discrimination*, and *Cold War*, all have statistically significant positive effects. Coefficient estimates for *Monopoly Control* and *Monopoly Control Squared* are also statistically significant and consistent with an inverted-U relationship between political regime and genocide onset. *Income* has its expected negative effect and is significant. The coefficient on *Population* has a positive sign (with no prediction) and is significant.

5.2 Monopoly control and inverted-U

As noted earlier, Vreeland's (2008) critique of the Polity contamination issue indicates that instances of political violence are assigned numerical values toward the middle of the Polity components PARREG and PARCOMP. Hence, unadjusted Polity data might identify a spurious inverted-U relationship between Polity and political violence in which anocracies appear to have a greater risk of conflict than democracies and autocracies, everything else the same. This is indeed what Vreeland found after adjusting the Polity data for two prominent *civil war* studies (i.e., Fearon and Laitin 2003; Hegre et al. 2001). To the best of our knowledge seven published studies test for an inverted-U relationship between Polity and *civilian atrocities*, with DeMeritt (2012), Hultman (2012), Goldsmith et al. (2013), and Rost (2013) finding evidence in favor of the relationship, but these studies

1994), Serbia/Yugoslavia (1998), Somalia (1988), Sri Lanka (1989), Sudan (1983, 2003), Syria (1981), and Uganda (1971). Omitted from the basic sample are Algeria (1962), Angola (1975), Bosnia (1992), and Sudan (1956) because their onsets occur in their initial year in the dataset, meaning that lagged data are unavailable; Iraq (1963) because their income data are unavailable in the PWT, Myanmar/Burma (1978) because their population and income data are unavailable in the PWT; Uganda (1980) because the onset occurs in the first year after a preceding genocide, so that lagged data are unavailable; Equatorial Guinea (1969) because discrimination data are unavailable in MAR; and Republic of Vietnam (1965) because data are unavailable in both the PWT and MAR.

Table 4: Basic model of genocide onset and weak state conditions.

	(1) Basic Model (Std. Error) [P-Value]	(2) With Unadjusted Polity (Std. Error) [P-Value]
Constant	-10.556*** (1.310) [0.000]	-11.203*** (1.487) [0.000]
Population	0.260*** (0.080) [0.001]	0.276*** (0.082) [0.001]
Threat	0.534*** (0.126) [0.000]	0.516*** (0.128) [0.000]
Monopoly control (adjusted polity)	0.476** (0.188) [0.011]	
Monopoly control square (adjusted polity)	-0.028** (0.013) [0.033]	
New state	0.885** (0.447) [0.048]	0.739 (0.451) [0.101]
Income	-0.430** (0.186) [0.021]	-0.428** (0.195) [0.028]
Discrimination	0.669*** (0.244) [0.006]	0.652*** (0.244) [0.008]
Cold war	1.352*** (0.499) [0.007]	1.372*** (0.485) [0.005]
Monopoly control (unadjusted polity)		0.442*** (0.158) [0.005]
Monopoly control square (unadjusted polity)		-0.017*** (0.006) [0.007]
Pseudo R ²	0.209	0.210
Log Likelihood	-157.391	-157.229
N	5913	5913
Countries	155	155
Onsets	32	32

Estimation is by pooled logit, with cluster-robust standard errors in parentheses and two-sided p-values in brackets. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$ (two-sided).

use unadjusted Polity data.¹⁵ Aydin and Gates (2008), however, adjust for the contamination issue, but they do not test the inverted-U hypothesis.

To the best of our knowledge, column (1) of Table 4 represents the first reported test in the empirical genocide literature of the inverted-U hypothesis using *adjusted* Polity data. Whereas Vreeland found that the inverted-U result vanished in his tests of civil war risk, we find significant evidence for an inverted-U relationship between adjusted Polity and genocide risk. We re-ran the same model using unadjusted Polity data and results are reported in column (2) of Table 4. Coefficient estimates for *Monopoly Control* and *Monopoly Control Squared* cannot be directly compared across the columns of Table 4 because adjusted Polity data are based on a 0–13 scale while unadjusted Polity follows a 0–20 scale. Nevertheless, column (2) also reports significant evidence for an inverted-U relationship. Hence, the risk of genocide onset is higher for anocracy relative to autocracy and democracy for both adjusted and unadjusted data. Although our coefficient estimates are not substantially different when we switch from adjusted to unadjusted Polity data, future research should use adjusted data. Consider first that the risk of genocide associated with intermediate Polity scores is greater for unadjusted relative to adjusted data.¹⁶ Moreover, in column (2) the positive coefficient estimate on *New State* ceases to be significant (albeit barely). Hence, use of unadjusted Polity may over-estimate genocide risk for anocratic levels of Polity and potentially affect the statistical significance of other variables.

¹⁵ Armstrong and Davenport (2008), Eck and Hultman (2007), and Krain (2014) do not find significant evidence for an inverted-U based on unadjusted Polity data. Eck and Hultman, however, find statistically significant evidence for a U-shaped (not an inverted U) relationship between political system and civilian fatalities in which their political system proxies are dummy variables constructed from unadjusted Polity data. When they re-run the model using unadjusted Polity scores, they find no statistically significant evidence of a U-shaped or inverted-U relationship.

¹⁶ Dividing the Polity scales into three parts would give us an approximate intermediate range of values for adjusted Polity of 4.5–8.5 on a 13-point scale. For unadjusted Polity, the equivalent intermediate range would be approximately 7–13 on a 20-point scale. For these intermediate ranges, the risk of genocide onset for adjusted Polity ranges from 0.00356 (at score=4.5) to 0.00554 (at score=8.5), with a maximum risk of 0.00554 (at score=8.5). For unadjusted Polity, risk probabilities range from 0.00369 (at score=7) to 0.00658 (at score=13), with a maximum risk of 0.00658 (at score=13). Note that the maximum risk of genocide onset for unadjusted Polity (0.00658) is 18.8% greater than for adjusted Polity (0.00554).

6 Extended analysis

6.1 Trade openness

As noted in Table 1, numerous empirical studies consider the effect of trade openness on civilian atrocity risk or seriousness. For a country's trade openness, we again draw from the PWT 7.1, defining *Openness* as exports plus imports as a percentage of real GDP. If genocide interrupts international trade flows and relationships, thereby making violence more costly, then genocide would be less likely for more economically integrated states, everything else the same. In Table 5 we repeat the basic model results in column (1) and then show in column (2) what happens when we enter our measure for openness. Note that the coefficient on *Openness* has a negative sign as predicted but is near zero and far from significant.¹⁷

6.2 Ethnic heterogeneity

Here we explore the familiar proposition that the risk of genocide is higher in ethnically diverse countries, where divisions are facilitated along group lines (Chalk and Jonassohn 1990; Fein 1977). Citing Horowitz (1985), Montalvo and Reynal-Querol (2008) emphasize the related proposition that genocide is more likely when a country is polarized, meaning that a large majority is set against a large minority. In their logit model of genocide occurrence, Montalvo and Reynal-Querol find no statistical effect for fractionalization but a strong positive effect for polarization.¹⁸ To test these propositions we compute measures of fractionalization and polarization based on country-specific, time-invariant

¹⁷ Although Harff (2003) and Rost (2013) report a significant negative relationship between trade openness and civilian atrocity risk or seriousness, most other empirical studies of civilian atrocities that have assessed trade report inconsistent or insignificant effects (Anderton 2014b).

¹⁸ Others test for the effects of heterogeneity on genocide or mass killing. For ethnic fractionalization, Krain (2005) and Kim (2010) generally find insignificant effects, Querido (2009) finds negative and significant effects, and Aydin and Gates (2008) find positive and significant effects. Easterly, Gatti, and Kurlat (2006) find a significant relationship between ethnic fractionalization and mass killing occurrence in one specification and, in another specification, no significant effect between ethnic fractionalization and mass killing severity. To the best of our knowledge, Montalvo and Reynal-Querol's original test of ethnic polarization and our tests in this article are the only tests of the variable in the empirical genocide literature.

Table 5: Alternative models and estimators of genocide onset.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Basic Model Repeated	With Openness Heterogeneity	With Ethnic Heterogeneity	With War	With Real Growth Rate	With Political and Economic Discrimination	With Internet Users Per 100	Pooled Logit With Cubic Polynomial	Random Effects Logit	Rare Events Logit
Constant	-10.556*** (1.310)	-10.147*** (1.521)	-10.691*** (1.669)	-10.554*** (1.309)	-10.715*** (1.433)	-10.173*** (1.152)	-10.540*** (1.377)	-10.929*** (1.741)	-10.585*** (1.322)	-9.963*** (1.209)
Population	0.260*** (0.081)	0.215** (0.110)	0.272** (0.108)	0.256*** (0.085)	0.297*** (0.093)	0.224*** (0.076)	0.274*** (0.083)	0.278*** (0.088)	0.262*** (0.124)	0.262*** (0.090)
Threat	0.534*** (0.126)	0.529*** (0.131)	0.537*** (0.132)	0.496* (0.290)	0.583*** (0.133)	0.575*** (0.123)	0.549*** (0.128)	0.540*** (0.129)	0.535*** (0.134)	0.537*** (0.131)
Monopoly control	0.476** (0.188)	0.484*** (0.189)	0.476** (0.189)	0.478** (0.192)	0.481** (0.200)	0.441** (0.182)	0.478** (0.193)	0.455** (0.189)	0.476** (0.233)	0.424*** (0.161)
Monopoly control squared	-0.028** (0.013)	-0.028** (0.013)	-0.028** (0.013)	-0.028** (0.013)	-0.029** (0.015)	-0.026* (0.013)	-0.029** (0.014)	-0.026** (0.013)	-0.028 (0.017)	-0.025** (0.012)
New state	0.885** (0.447)	0.891** (0.451)	0.862* (0.457)	0.892** (0.439)	1.055** (0.430)	0.721* (0.431)	0.893** (0.446)	1.142* (0.630)	0.887 (0.663)	1.016* (0.597)
Income	-0.430** (0.186)	-0.379** (0.178)	-0.402* (0.216)	-0.423* (0.216)	-0.405** (0.195)	-0.484*** (0.173)	-0.448** (0.190)	-0.459** (0.186)	-0.430** (0.203)	-0.419** (0.188)
Discrimination	0.669*** (0.021)	0.676*** (0.033)	0.673*** (0.063)	0.665*** (0.051)	0.642** (0.038)	[0.005]	0.644*** (0.018)	0.672*** (0.014)	0.670*** (0.034)	0.606** (0.026)
	[0.006]	[0.244]	[0.249]	[0.250]	[0.261]		[0.241]	[0.245]	[0.232]	[0.238]
		[0.005]	[0.007]	[0.008]	[0.014]		[0.007]	[0.006]	[0.004]	[0.011]

(Table 5: Continued)

	(1) Basic Model Repeated	(2) With Openness Heterogeneity	(3) With Ethnic Heterogeneity	(4) With War	(5) With Real Growth Rate Discrimination	(6) With Political and Economic Discrimination	(7) With Internet Users Per 100	(8) Pooled Logit With Cubic Polynomial	(9) Random Effects Logit	(10) Rare Events Logit
Cold War	1.352*** (0.499) [0.007]	1.260** (0.543) [0.020] -0.007 (0.010) [0.503]	1.374*** (0.516) [0.008]	1.349*** (0.500) [0.007]	1.563*** (0.548) [0.004]	1.375*** (0.518) [0.008]	1.460*** (0.523) [0.005]	1.320** (0.562) [0.019]	1.360*** (0.525) [0.010]	1.260** (0.513) [0.014]
Openness										
Fractionalization			0.471 (0.962) [0.624]							
Polarization			-0.294 (0.861) [0.733]							
War				0.127 (0.935) [0.892]						
Real growth					-2.468 (2.473) [0.318]					
Political discrimination						0.063 (0.299) [0.834]				

(Table 5: Continued)

	(1) Basic Model Repeated	(2) With Openness Heterogeneity	(3) With Ethnic Heterogeneity	(4) With War	(5) With Real Growth Rate Discrimination	(6) With Political and Economic Discrimination	(7) With Internet Users Per 100	(8) Pooled Logit With Cubic Polynomial	(9) Random Effects Logit	(10) Rare Events Logit
Economic discrimination						0.601*** (0.203) [0.003]				
Internet users per 100							-0.534 (0.567) [0.346]			
Genocide-free years								0.037 (0.109) [0.735]		
Genocide-free years squared								-0.005 (0.006) [0.938]		
Genocide-free years cubed								-0.000 (0.000) [0.940]		
Rho									0.009 (0.117)	
Pseudo R ²	0.209	0.212	0.209	0.209	0.223	0.220	0.223	0.210		
Log likelihood	-157.391	-156.811	-157.238	-157.376	-139.776	-155.101	-150.218	-157.053	-157.388	
N	5913	5913	5897	5913	5457	5913	5843	5906	5913	5913
Countries	155	155	154	155	141	155	153	155	155	155
Onsets	32	32	32	32	29	32	32	32	32	32

See Table 4. *p ≤ 0.10, **p ≤ 0.05, ***p ≤ 0.01 (two-sided).

ethnic group shares provided in Alesina et al. (2003). As shown in column (3) of Table 5, we find no statistically significant evidence that greater ethnic heterogeneity increases the risk of genocide.

6.3 War

In column (4) of Table 5, we include a dummy variable for internal war (ethnic or revolutionary) based on the PITF intrastate conflict dataset. Note that the coefficient estimate on *Threat* remains positive and significant, while the coefficient for *War* has the correct sign but is far from significant. In results available from the corresponding author, we find that when we substitute the *War* dummy for our measure of *Threat*, we obtain the correct sign and statistical significance for the *War* coefficient. However, that model has lower pseudo R-square and log likelihood relative to the basic model. Hence, we find that our more graduated index of *Threat* outperforms the war measure. We also ran the basic model with a Magfight measure of *Threat* that is constructed in a similar fashion to the Magarea index. Magfight performs essentially equally well to the Magarea measure (results available from corresponding author). Many others have controlled for the presence of war in their empirical studies of civilian atrocities.¹⁹ We find that PITF's Magarea and Magfight indexes generally outperform these war measures.

6.4 Real growth

Per capita income has been interpreted in multiple ways in empirical studies on conflict. For example, some interpret it as a proxy for the opportunity cost of forgone employment in the regular economy should one participate in the activities of a violence-producing organization (e.g., Collier and Hoeffler 2004). Others treat per capita income as one of the measures of a weak state (e.g., Fearon and Laitin 2003). Our objective is not to settle what might be the most appropriate

¹⁹ For example, Eck and Hultman (2007), DeMeritt (2012), and Rost (2013) use UCDP/PRIO data to construct various dummy variables for war. Krain (1997), Easterly, Gatti, and Kurlat (2006), and Wayman and Tago (2010) do the same using Correlates of War (COW) data. Kathman and Wood (2010) include a dummy variable for civil war based on data in Gleditsch et al. (2005). Valentino, Huth, and Balch-Lindsay (2004) use a dummy variable for identity conflict based on Licklider's (1995) data. The coefficient estimates for war in these studies are usually positive and most are statistically significant, but there are exceptions. For example, DeMeritt (2012) finds that her war dummy is negative and statistically significant.

interpretations of per capita income. Indeed, how it should be interpreted likely varies across conflict types and authors' samples. Nevertheless, we attempt to narrow the range of interpretation by controlling for the growth rate of real gross domestic product. Our measure, called *Real Growth*, is taken from Penn World Table (PWT) Version 8.0 (Feenstra, Inklaar, and Timmer 2013) and entered with a 1-year lag. We hypothesize that lagged real growth would capture economic conditions, and thus employment opportunities, in the regular economy. Hence, real growth would capture in part the opportunity cost of forgone employment, leading to an inverse relationship between real growth and genocide risk. Results reported in column (5) of Table 5 show that real growth has the predicted sign but is insignificant. Moreover, the coefficient estimate on per capita income remains negative and significant. We take these results as not ruling out the weak state interpretation of per capita income and the possibility that killing civilians has inferior input characteristics.²⁰

6.5 Political and economic discrimination

Recall that our measure of discrimination relies on two underlying measures – one being the highest level of political discrimination evidenced against any communal group within a country-year, and the other being the same for economic discrimination. Because the MAR measures are defined commensurately, we can examine whether the two forms of discrimination have similar effects on genocide. In Table 5 we show in column (6) what happens when we enter both measures of discrimination. Note that the effect of political discrimination is close to zero and insignificant, while the coefficient on economic discrimination is positive and significant. The two measures are highly correlated ($r=0.808$), which likely accounts for why the hypothesis of equal effects cannot quite be rejected at conventional significance levels ($\chi^2(1)=1.34$, $p=0.247$). It is evident, however, that

²⁰ In results available from the corresponding author, we used World Bank data on Gini coefficients and percent of the population living on <\$2.50 per day to see if income inequality and poverty might correlate to genocide risk. Owing to significant gaps in the data (e.g., no data prior to 1974 and very spotty data after 1974), only five genocide onsets remained in our sample after excluding missing observations, even after we extrapolated data over 5 year periods. We ran separate rare events logit regressions for the inequality and poverty measures, excluded Cold War because a preponderance of observations occurred in the post-Cold War period, and used a linear specification for Regime and cut New State to increase degrees of freedom. Coefficient estimates for inequality and poverty were statistically insignificant. We consider these empirical results preliminary and unreliable. Nevertheless, future research is warranted on possible connections between inequality, poverty, and genocide risk (see also Besançon 2005; Kim 2010).

the explanatory power is carried primarily by economic discrimination. In results available from the corresponding author, if economic discrimination is dropped, the log likelihood decreases markedly (from -155.101 to -159.461) and the pseudo R-square falls (from 0.220 to 0.198); yet if political discrimination is dropped, the two statistics hold stable (at -155.132 and 0.220).

Gurr and Moore (1997) found that economic as opposed to political discrimination significantly heightens the articulated grievances of minorities, but they provided no explanation for this finding. In our context pertaining to genocide risk, we have emphasized that discrimination can foster the “othering” of an out-group, which reduces the costs to an authority group of recruiting perpetrators. But economic discrimination can further reduce the costs of recruitment by rendering acceptable the seizure of victims’ wealth. Moreover, undercutting a victim group’s economic livelihood may be particularly damaging to the group’s viability and thus an important precursor to genocide (people can live without voting rights, but not bread). Thus, economic discrimination can especially elevate the risk of genocide.

6.6 Cold war and the internet

In our basic model in column (1) of Table 5, the positive and significant coefficient on the Cold War indicator is consistent with the hypothesis that tensions between the major powers prior to 1990 made cooperative intervention less likely and hence increased the risk of genocide. However, it is also intriguing to consider that the post-Cold War period roughly coincides with the emergence of new information technologies (e.g., satellite television, the globalization of CNN, the internet, mobile phones, social networking, etc.) that facilitate awareness of mass atrocities. Hence, leaders contemplating genocide might consider the greater awareness and risk of intervention that now exists relative to the Cold War period. One conjecture is that if this spread of information facilitated third-party reaction, the rapid advance of internet technology after 1989 could have lowered the risk of genocide and hence generated a positive coefficient on the Cold War dummy. Of course, governments can use information technologies to suppress citizens, including perpetrating civilian atrocities, so our conjecture is speculative. The important point is that, to the best of our knowledge, large-sample empirical civilian atrocity studies published to date have not assessed the possible impact of information technology. We check for this possibility by adding to our basic model a lagged measure of the number of internet users per 100 persons in a given country-year (World Bank 2011).

As shown in column (7) of Table 5, *Internet Users per 100* has a negative effect as conjectured but is not statistically significant, with a one-sided p-value of about 0.17. Also, before leaving column (7), note that when *Internet Users per 100* is added, the coefficient on *Cold War* increases from 1.352 to 1.460 and remains significant. Thus, controlling for internet technology, we still find evidence that genocide risk fell after the Cold War, other things equal.²¹

6.7 Other estimation issues

When applying logit to time-series cross-section data, temporal dependence can arise (Beck, Katz, and Tucker 1998), which in this context exists if the likelihood of genocide onset depends on a country's chronological experience regarding genocide. Among methods for modeling dependence, the application of a cubic polynomial is appealing for its simplicity and demonstrated performance. Following Carter and Signorino (2010), we add to our basic model a polynomial consisting of the number of prior genocide-free years and that same number squared and cubed. In column (8) of Table 5 we show that all three polynomial terms are statistically insignificant, and the joint hypothesis of zero coefficients cannot be rejected ($\chi^2(3)=0.800$, $p=0.849$). The coefficients on the other variables in the model are qualitatively unaffected.

For random effects, reported in column (9), the coefficient estimates are similar, and a Breusch-Pagan test that the within-country serial correlation ρ equals zero cannot be rejected at conventional significance levels ($\chi^2(1)=0.002$, $p=0.483$).²² Regarding fixed effects estimation, the method is severely stressed when the event of interest is rare (Beck and Katz 2001). This means that countries

21 In results available from the corresponding author, we substitute *Mobile Phone Users per 100* for *Internet Users per 100*, with results that are qualitatively similar. Pierskalla and Hollenbach (2013) find a positive effect of mobile phone technology on violent forms of collective action in Africa whereas Shapiro and Weidmann (2013) find a negative effect of such technology on violence in Iraq. Although our technology results are preliminary, further inquiry into the impact of information technology on mass atrocity risk and seriousness might be fruitful when the sample period is lengthened by the passage of time.

22 Column 9 of Table 5 shows that the inverted-U relationship between regime as measured by *adjusted* Polity data and genocide risk ceases to be significant under random effects logit, although it is close. In results available from the corresponding author, we ran random effects logit using *unadjusted* Polity and the inverted-U result was statistically significant. Since unadjusted Polity data contain factional violence (including genocide) for intermediate Polity scores, such data might lead to an over-estimation of the effect of anocracy on genocide risk, which is a key point that we seek to communicate in this article.

experiencing no genocide onset are effectively dropped because their observations contribute nothing to the likelihood calculation. More appropriate to our study is rare events logit, which corrects for possible underestimation of rare event probabilities in finite samples (King and Zeng 2001a,b; Tomz, King, and Zeng 1999). As shown in column (10), estimation by rare events logit leaves the coefficients and their significance qualitatively unaffected.²³

7 Conclusions

Our theoretical and empirical analysis points to six significant risk factors for genocide onset: (1) threat posed by rebels against a state, (2) anocracy, (3) new state, (4) low income per capita, (5) discrimination, particularly economic discrimination, and (6) Cold War conditions. Moreover, the risk factors are generally robust to alternative variables and estimation methods. On the other hand, we find no significant effects for trade openness and ethnic heterogeneity. We also investigate whether internet and mobile phone access lower genocide risk. Although the technology variables are insignificant, the estimated magnitudes are large enough to warrant future inquiry as internet and mobile phone usage widen. We interpret several of our results, particularly regarding threat, anocracy, new state status, and per capita income as consistent with weak state perspectives on violence within states. Moreover, our theoretical and empirical results do not rule out the possibility that killing civilians has inferior input characteristics.

In addressing genocide risk, the study here can be useful to scholars and policymakers alike. For future theoretical modeling of civilian atrocities, constrained optimization models can provide a valuable economic lens for generating testable hypotheses. For future empirical work, our results can contribute toward consensus on appropriate control variables and proxies for those variables in hypothesis testing. We believe it is important that future empirical studies on civilian atrocities incorporate adjusted Polity scores and new state status (or equivalents). From a policy perspective, the principle we would draw from our results is the need for policy interventions to holistically address the multifaceted dangers of anocratic political conditions, other weak state conditions (e.g., new state status and low income), and structures of discrimination, particularly economic discrimination. Such interventions include shorter-term policies to insure that regimes that perceive threat, new and emerging states, and states transitioning from autocracy or

²³ Results for estimation by Cox proportional hazard are qualitatively similar to the basic model and are available from the corresponding author.

democracy to anocracy not slide into civilian atrocities. Also required are longer-term policies that focus on structural changes to states embedded in anocratic political systems, low income, and structures of discrimination.

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