Since World War II, it has been a global priority to *stop* ongoing, large-scale state repressive activity – hereafter LSSR (i.e., government sponsored, violent action that is both widespread and systematic).[1] With this objective in mind, policymakers, NGOs and activists have suggested as well as implemented a number of policies, including military intervention, economic sanctions, naming/shaming, international law, preferential trade agreements, political democratization and non-violent direct action – all are presumed to diminish state repression because they increase the costs of such activity. What (if anything) works? What stops LSSR? Despite the significant amount of interest in this question, we still know very little about it (e.g., De Waal and Conley-Zilkic 2006; Bellamy 2015). The primary reason for this limitation is that almost all empirically rigorous work has been focused on understanding what accounts for *variation* within levels of repression (i.e., what moves repressive behavior up or down some scale) and what impact diverse cost-producing factors have had on this phenomenon. While related and providing important insights, this variation work is largely focused on something quite different from termination in terms of theory, data structure and appropriate estimation.[2]

To shed light on this important topic, the current paper draws upon prior research but shifts the focus from variation *in* repression levels to termination *of* repressive spells.[3] Here, we argue that not all costs are comparable to political authorities in terms of their centrality to state repression and that political actors involved in relevant activity are more likely to stay with enacted behavior unless something major changes in their environment.[4] Among existing policies put forward to increase costs and decrease repression, we maintain that the controversial and often maligned policy of political democratization is the factor most likely to stop LSSR as it involves the most significant cost that could be imposed – i.e., changing who is in charge, establishing/expanding accountability as well as increasing mass participation in the political

process.

In an effort to assess our argument, we investigate a new dataset of 239 high-*repression spells* from 1976 to 2007. From our analysis, we find that democratization is the only factor that consistently stops LSSR. In contrast, and equally as important as what we find support for, results disclose that factors believed to impact repression in the literature as well as within diverse policy circles have essentially no *direct* impact on spell termination.[5] To further understand what stops LSSR, we push deeper into the causal chain and examine what influences democratic transition. Here, we find that specific nonviolent campaigns are linked to a greater likelihood of democratization, suggesting that they have an important indirect role to play in stopping LSSR while having little direct impact on spell termination viewed on their own.

With these findings, our work calls for a re-evaluation of the policies currently put forward to stop LSSR, advancing an approach that is more oriented toward disrupting/perturbing institutions and those who have a vested interest in repressive policies. Additionally, our work calls for a re-evaluation of how repression is conceptualized and studied. In part, the shift from variation to spell termination marks an important pivot for research that not only better reflects the nature of how large-scale repressive behavior is enacted but further helps to more accurately identify the factors that are associated with driving it/bringing it to an end. Such a shift has the added benefit of bringing the systematic study of state repression more closely in line with work conducted on other forms of large-scale political violence (such as interstate and civil war) – this work has long employed the conception of spells and termination with the distinct theoretical insights, units of analysis and estimated strategies that accompany them (e.g., Hegre 2004).

Repression and Its Variation

A large literature has emerged which seeks to understand what explains variation in state repressive action – that is movements up or down some scale of magnitude/scope/lethality regarding government coercion and forceful behavior directed against those within their territorial jurisdiction (e.g., Appel 2016; Conrad and Moore 2010; Davenport 1995; 2007a; Davenport and Armstrong 2004; Demeritt 2012; Fariss and Schnakenburg 2014; Hafner Burton 2005; 2008; Henderson 1991; Hibbs 1973; Hill and Jones 2014; Keith et al. 2009; Krain 1997; 2005; Lupu 2013; Mitchell and McCormick 1988; Mitchell et al. 2013; Nordas and Davenport 2013; Poe and Tate 1994; Ritter 2014).[6] This work has been useful in advancing understanding of the relevant phenomenon. Nonetheless, the research here has developed some characteristic features that have directed and consequently limited scholarly effort.

For example, the primary focus of variation work has been in identifying explanations and variables that if changed would increase the "operational" (e.g., salaries, training and weapons) and/or "political" costs (e.g., removal from office or damaged reputation) of repressive action.[7] In these contexts of raised cost, the use of coercion and force is expected to be reduced because political authorities as well as their agents are expected to limit any losses that might be incurred from engaging in relevant behavior. Toward this end, research has consistently highlighted a variety of variables that are believed to have a negative impact: e.g., military intervention (e.g., Kathman and Wood 2011; Krain 2005), economic sanctions (e.g., Peksen 2009; Wood 2008), naming/shaming (e.g., Hafner-Burton 2008; Franklin 2008; Murdie and Davis 2012), international law and courts (e.g., Appel 2016; Simmons 2009), preferential trade agreements (e.g., Hafner-Burton 2005, 2009; Hill and Jones 2014; Spilker and Boehmelt 2012), democratization (e.g., Davenport 1996; Zanger 2000) and, more recently, civil resistance (e.g.,

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Chenoweth et al. 2017). Accordingly, these factors comprise the leading policy put forward to address the topic advocated by governments, NGOs and INGOs the world over.

Although the idea of costs and the diverse measures used to operationalize them are useful, there are some limitations with this work as it relates to the objectives of terminating LSSRs. At the most basic level, while understanding what reduces repressive action across a range of values is relevant to the topic of interest, this is not the same thing as "ending" a sustained spell/campaign/episode of repressive action (like with terminating interstate or civil war). A major part of the difference here is conceptual. One implicit point made in existing work is that political authorities make repeated as well as detailed decisions regarding whether or not and/or how to apply repression and do so at regular intervals (i.e., every year). This however overlooks the difficulty of sustaining repressive action which, in turn, tends to underestimate the difficulty in shifting relevant practices over time. As portrayed, variation research maintains that decision makers are always open to changes in their political, economic and demographic environment. It is possible though that decision makers are unlikely to change after a policy has been developed/implemented and that they would require something dramatic in the environment to shift in order to prompt alterations in calculations, perceptions and behavior. In addition to this assumption, variation research also treats all levels of state repression as being equally interesting, assuming (again generally implicitly) that the various steps up/down the relevant scale are influenced in the same manner by explanatory factors.

Such positions are problematic for two reasons. First, state repression that is extremely violent, widespread and systematic (e.g., genocide) has consistently been viewed as being quite different from government coercion/forceful behavior that is limited in terms of scope and lethality (e.g., protest policing). Indeed, these differences have prompted distinct levels of

attention from international legal bodies, human rights NGOs and media, distinct laws (e.g., genocide and crimes against humanity) as well as distinct fields of study (e.g., genocide and mass atrocity). In line with this, it seems reasonable that LSSR would be influenced by different explanatory factors than less violent, smaller-scale as well as less frequent behavior and that it would respond to different costs.

Second, particular levels of state repression might involve very different amounts of inertia and "stickiness" than others (e.g., Hafner-burton 2005). For example, it is likely the case that levels of repression that involve a larger number of actions, actors, targets as well as degree of violence are potentially less likely to be stopped. To assume that different levels of repressive action are comparably influenced by the same independent variables (a position currently maintained) is likely to underestimate the difficultly with which LSSR can be terminated once underway. This realization prompts us to reflect on how best to conceive and study the topic.

Repression as a Spell

In an attempt to address the issues raised above, we maintain that violent, widespread, systematic state repressive action *is* essentially the end product of 1) political authorities who design the basic contours of the relevant policy, 2) the thoughts and discussions of coercion-wielding institutions who work out the more specific details of the repressive effort as well as 3) the actions of the relevant government agents who engage in repressive behavior and the norms of practice that develop after they have begun. Here, potentially thousands of individuals and actions come to be involved.[8] This is a different view than one where the decision calculus and repression are viewed as events (e.g., Francisco 1996) or as an evaluation of distinct attributes regarding diverse activities (e.g., Wood and Gibney 2010). In both of these circumstances, we maintain that the process of repression is somewhat obscured and simplified.

Drawing upon these insights, we wish to highlight that repressive behavior in general but especially the violent, widespread and systematic behavior being referenced above suggests that there is a lot involved with getting governments to use this behavior in the first place and once set on the relevant path there is a lot involved with getting governments to stop. This should actually come as no surprise to researchers of state repression. Prior research has referred to this phenomenon with a variety of labels that suggests such a framing (e.g., "bureaucratic inertia" [Gurr 1986a], "the law of the instrument" [Gurr 1986b], "the weight of the past" [Davenport 1996] and "stickiness" [Hafner-Burton 2005]).[9] Additionally, numerous scholars find lagged repression statistically significant– in fact, when included within estimated models results usually disclose that this is the most significant variable of all those considered.[10] While discussed, however, what has been missing from previous empirical literature is the explanation of what lags and stickiness mean for how we think about as well as study state repression.

On this point, we argue that explicit attention to the factors noted above is important because it deviates from prior work which normally conceives of the process involved as being one that can be easily switched by only introducing just the "right" type of cost/variable. This is not likely with our conception. Indeed, if the repressive process is difficult to shift once underway, then the cost-producing variable required to perturb the relevant process would need to be quite powerful. Without significant change, the repressive process would not be shifted in any manner and repressive behavior (i.e., the spell) would continue.

What (if anything) imposes a significant enough cost on political systems engaged with repression that might shift behavior when applied? Given the situation described above, we argue that democratization holds the greatest promise for stopping an ongoing spell of LSSR because of its ability to fundamentally alter the government engaged in relevant behavior. Generally, this

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is because democratization impacts both political authorities as well as coercive agents. To explain this relationship further, we discuss in the next section what is involved with democracy and democratization as well as what is involved with state repression.

Concepts and Connections

In our research, *democracy refers to a particular form of institutional arrangement where some constituency (i.e., group of citizens) selects another group of individuals (i.e., politicians/representatives) to make decisions concerning the general direction of the relevant political unit* (e.g., Cheibub et al. 2010; Coppedge 2013, chapter 2; Held 1997; Hill 2016). Democracy thus represents a particular set of relationships and practices where democratization represents a shift from a position of lesser toward a position of greater democracy. Now, exactly how leaders are chosen or which actors are highlighted varies.

Most centrally connected with the concept employed here, scholars focus on the *existence/quality of elections*, what *percentage of the population has an opportunity to participate* and whether *political parties* contesting one another represent the population. Each one of these practices and/or institutions plays a role in the function of democracy. Reliance on representatives and parties is clearly distinct from direct variants with regard to how immediate/explicit the connection is between what citizens want and how they attempt to hold political authorities accountable for meeting their needs, but with regard to this relationship the representatives/parties are specifically tasked with this objective. That said, while these institutions can hold authorities accountable for their actions, these are less direct forms of democracy. [11]

In contrast to democracy and democratization with its focus on influencing individuals at the pinnacle of the political hierarchy, repressive action is focused on influencing individuals at

the nadir. Specifically, repression refers to when security forces engage in a particular type of coercion and force against those under the jurisdiction of the relevant political authority in an effort to control the target's behavior and/or beliefs. This includes arrests, beatings, banning, torture, disappearances and killing. It is often assumed that those in charge of implementing repression are automatically beholden/accountable to political authorities – thereby linking the leaders of the political system to the behavior of the security apparatus, but this is not always the case. Indeed, non-responsiveness and self-serving interests are the central claim of one of the newer theories used to explain state repression (e.g., Mitchell 2004; Demeritt 2012).

It is useful to distinguish the upwardly focused institution of democracy from the downwardly focused policy/behavior of repression because it is the ability of the former to disrupt the latter that determines how likely it is that large-scale repressive action will stop after being initiated. For example, with new leadership and mechanisms of accountability put in place (relative to the situation under authoritarianism), we expect democratization to lead to transformations in who occupies positions of power and how that power is used against those under their jurisdiction. Accordingly, this results in changes with the policies leaders wish to pursue, how they allocate resources, where they derive support as well as how they train, arm and issue commands related to repression. Indeed, it is in part through highlighting the differences from the previous regime that the newly democratized government establishes itself. Large-scale repressive policies are specifically targeted here for attention because they not only take up a significant amount of human as well as material resources but they have been found to quite varied impacts on behavioral challenges which could put the new political authorities in situations of direct threat through removal and/or death. Additionally, not only might the policy-making aspect of the repressive process be altered following democratization, but the willingness

of security force agents to comply with directives might also change. Acknowledging that support for the prior government and its policies have shifted (presumably including repression), relevant agents (within the recently formed democratizing contexts) might withdraw from their participation in the previous government's repressive behavior out of fear that they could invite rebellion and/or diminish support for themselves within the new political system. In short, democratization *directly* works to stop state repression through replacing decision-makers outright, altering the way that they are selected, held accountable as well as providing leaders with an opportunity to distinguish themselves from the prior regime and define themselves politically in the eyes of the citizenry. Moreover, democratization *indirectly* prompts repressive agents to stop repressive behavior by altering the relationship between principals (leaders) and agents (security force institutions) as well as the basis for political legitimacy held by the former.

This is not to say that political leaders and agents of repression become more pacific and peaceful – although this is possible. Rather, this is to say that given a shift in the political terrain these actors take advantage of the opportunity provided to them to create an identity that is distinct from the group of individuals who were recently displaced. Differentiation through policy change is one of the more effective ways to accomplish this. Such an argument is consistent with some research that advances the claim that "(c)ontentious politics lies at the heart of political development because it powerfully and persistently shapes how political alliances and rivalries will be defined. At its core, politics is not about individuals pursuing material gains, or internally cohesive economic classes pursuing shared wealth. It is about factions striving and struggling for political power, advantage and survival" (Slater 2010: 276). Interestingly, as the imposition of repression is believed to assist one cohort struggle, survive and thrive, the withdrawal of repression is believed to do the same.

Now, we should be clear: maximalist conceptions of democracy/democratization tend to conflate structural discussions about how leaders are selected with a particular constellation of preferred policy outputs like state repression (e.g., Hill 2016). Our approach to the topic is comparably more *minimalist*, and in line with diverse theorists, policy-makers, activists and citizens, we view democracy/democratization as simply and straightforwardly a mechanism of selection. Such a position we view as reasonable because it allows us to gauge the impact of specific institutions/practices on specific government behavior, avoiding tautology. Such a position also reflects the long-standing opinion of theorists, activists and citizens from around the world who view democracy as a (and perhaps "the") solution to the problem of state repressive action. Such an opinion is also consistent with the quantitative literature which consistently has produced results that democracy decreases repression. Clearly though this impact has not been found to be as simple as is often thought. Indeed, some nuanced and sophisticated research reveals consistent support for only certain levels of democracy decreasing repressive behavior (e.g., Bueno De Mesquita et al. 2005; Davenport 2007b), a few pieces show that movements toward democracy reduce repression (e.g., Davenport 1999; Zanger 2000) and some work discloses that full democracies can avoid repression only when behavioral challenges are not present (e.g., Davenport 2007b; Conrad and Moore 2010).

Our approach is distinct from existing literature on repression in that once the LSSR has been implemented, we do not expect most variables and the diverse policies associated with them to have much of impact on government coercion and force. This is because we maintain that they do not profoundly disrupt the decision-making and implementation processes involved. Indeed, in our conception, only democratization significantly disrupts the inner workings of the

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coalition of policy makers and security force agents that forms around large-scale applications of repressive action.[12]

Research Design

Conceptually, a *spell of large-scale state repression* is defined as the duration of time when government violence is above a certain level of severity/lethality and scope (i.e., size of the targeted population). The threshold used to determine a spell was derived from the Political Terror Scale (hereafter PTS) – the most frequently used indicator of repression in existing scholarship, and the most relevant to our discussion as it simultaneously addresses the lethality of behavior enacted as well as scope of the population being targeted (Wood and Gibney 2010).

Operationally, the PTS measure is coded on an ordinal 1 to 5 scale. Scores of 1 and 2 correspond to countries where political torture, beatings, and murder are extremely rare and isolated. In contrast, a significant change in repression/human rights violation is noted above level two which corresponds to the interest of citizens, lawyers, theorists, activists and advocates alike. Indeed, with a score of 3, there is unlimited detention for political views, extensive political imprisonment, execution and brutality. A score of 4 means that murders, disappearances, and torture have expanded to large numbers of people for political reasons. A score of 5 represents an environment where state terror of the worst kind has expanded to the whole population, not just those engaged in political activities.

Based on these definitions and given our focus, the threshold to determine a spell is a PTS score of 3 and above.[13] This LSSR continues until the PTS drops below 3 for at least two consecutive years. The coding rule is consistent with prior work that looks at state repression in the worst situations (e.g., Hafner-Burton 2005). There is also face validity to using 3 and above to create the high repression spells. Comparing high-repression spells to the genocide/politicide

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data compiled by the Political Instability Task Force (2010), we find that all cases of genocides/politicides overlap at some point with our universe of LSSR.

Primary Response Variable

While spells are the unit of analysis, our dependent variable is the duration of an LSSR. Adopting the coding rule noted above, the data contains a total of 239 repression spells that produce a total of 2,527 observations (i.e., spell years) for the period between 1976 and 2007 (both completed by the last year as well as those still underway). A complete list can be found in the Appendix.[14]   In these data, the average duration of spells is 11 years, while the minimum length is one year and the maximum is 32. Derived from our interest in LSSR termination, we focus on the probability (or hazard) that repression ends in a year, given that it has survived up to that time.[15]

Explanatory Variables of Interest

Consistent with existing research, we include numerous explanatory variables that are believed to impose costs on the repressive process.

Military Interventions. To measure this variable, we use Kisangani and Pickering's update of the Pearson and Baumann international military interventions (IMI) data set (Kisangani and Pickering 2008; Pearson and Baumann 1993). Drawing on previous work, we focus on three types: 1) "pro-government" - where the intervention supports the government and/or opposes rebel/opposition groups, 2) "anti-government" - where the intervention opposes the government and/or supports the rebel/opposition groups, and 3) "neutral" - where interventions are coded as non-supportive and/or impartial. We code a binary variable for each intervention type and lag these variables by one year in all models. In the data, there are 59 pro-government interventions

(24%), 73 anti-government interventions (30% of all spells), and 55 neutral-interventions that occur during spells (23%).

Economic Sanctions. To code this variable, we use monthly data from the Threat and Impositions of Sanctions (TIES) project (Morgan et al. 2006). As designed, we only include sanctions that are targeted at human rights violations and/or modifying contentious behavior related to conflict and repression undertaken by the government. Based on this, we create a binary variable, lagged one-year, that equals 1 when a third party enacted economic sanctions on a government involved in a repression spell, and zero otherwise. In the data, 32 spells involve economic sanctions (13%).

Naming and Shaming. To capture this phenomenon, we use data from Lebovic and Voeten (2006) on resolutions by the United Nations Commission on Human Rights (UNHCR). As conceived, a binary variable equals 1 if the Commission condemned a state for committing human rights violations and zero otherwise. This variable is lagged one-year in the models presented below. The UNHCR has adopted resolutions in 63 spells (26%).

International Law. We include two treaty-based variables: the International Covenant on Civil and Political Rights (ICCPR) and the Convention Against Torture (CAT). Both measures are lagged one-year and equal 1 if the state in question ratified the relevant treaty, and zero otherwise (Hill and Jones 2014). In our data, 116 (48%) and 163 (68%) spells have governments that have ratified the CAT and ICCPR, respectively.

Preferential Trade Agreements. We include a measure that equals 1 if the state in question is a member of at least one PTA in a given year, and zero otherwise (Spilker and Bohmelt 2012; Mansfield and Milner 2012). In line with the other international factors identified

above, this variable is lagged one-year in the all the models presented below. In our data, we have 71 spells with at least one PTA (29%).

Foreign Imposed Regime Change (FIRC). We use data from Downes and Monten (2013) for this variable. The authors define FIRC as when, a democratic state forcibly removes the leader of another state and then makes concrete efforts to promote democracy in the targeted state over the next five years (2013, 109). We employ a binary indicator that equals 1 if a FIRC occurs, and zero otherwise. This variable is coded a 1 in 23 observations in the data (.5%).

Democratization. To measure this variable we use an indicator of democracy from Przeworski et al. (2000) and most recently updated by Cheibub et al. (2010). Within these data, a regime is classified as democratic if it meets four conditions: 1) the population or a popularly elected body select the executive; 2) the legislature is popularly elected; 3) at least two parties participate in the elections; and, 4) when there is alternation (i.e., an incumbent executive is replaced by new executive via elections) the electoral rules must be identical to the ones that brought the incumbent to power in the first place. Following Cheibub et al. (2010), the democratization variable equals 1 the year the regime changes from a dictatorship to a democracy plus five additional years as long as the state remains a democracy. For example, if a state democratized in 1990, the variable is coded a 1 in years 1990 through 1995. There are 50 cases of democratization in our sample from 1976 to 2007 (20%).[16]

Non-violence. To address the recent interest with civil resistance, we also include a measure from the Nonviolent and Violent Campaigns and Outcomes (NAVCO) data project (Chenoweth and Stephen 2011). This measure is a binary variable that equals 1 when a campaign primarily using nonviolent methods is ongoing, and zero otherwise. [17] Consistent with our

interests, we only include campaigns that target regime change or significant institutional reform (i.e., improve human rights practices).

Control Variables. Following standard practice, we employ several control variables in our study (e.g., Poe and Tate 1994; Davenport 2007b; Hill and Jones 2014). It should be noted that these variables have never been employed to analyze repression spell termination because the topic has not been examined yet. It is possible therefore that the effects differ between the different units of analysis.

First, we include a measure of *economic development* operationalized as GDP per capita (based on purchasing power parity) lagged one year (World Bank 2008). In line with existing research, we expect higher values to be associated with shorter repressive spells as well-resourced governments should be quicker and more efficient at finishing what they set out to do.

We include a dummy variable for whether the state in question is involved in an *ongoing civil war*. The variable uses the UCDP conflict data lagged one year and equals 1 when a state is in a war (based on the 25 battle death threshold) and zero otherwise (Themnér and Wallensteen 2012). We anticipate that conflict will be associated with longer LSSR spells as behavioral challenges increase government efforts to suppress them as well as deal with the population. Related to this is a variable to account for *civil war termination.* Given the strong relationship between behavioral challenges and repression identified in the literature, this provides an important alternative explanation for spell termination; when behavioral challenges stop, repression should stop as well.

 In line with the domestic democratic peace proposition (e.g., Davenport 2007b), we include a measure for *the level of democracy* as described by Cheibub et al. (2010). We expect democracies will have shorter spells (if any at all) as these political systems have alternative

means for resolving conflicts as well as greater concerns for popular accountability. Related, recent literature suggests that domestic rule of law and in particular an *independent judiciary* constrains governments. We therefore include the Linzer and Staton (2015) latent measure of judicial independence, lagged one-year.[18] Following from the discussion of democratic institutions, we expect that spells with independent judiciaries will be shorter.

Two demographic variables are also considered; we include the *logged population* of the state (lagged one year) taken from the World Bank Development Indicators (2012). The expectation is that logged population should prolong state repression as it represents a general demographic threat. Second, drawing on Nordas and Davenport (2013), we include a measure of *youth bulges* from Urdal (2006, 608) defined as "large cohorts in the ages 15–24 relative to the total adult population." We expect large youth bulges to prolong repressive spells as they magnify the degree of potential and actual behavioral challenge.[19]

Model Specification

Different from the repressive variation approach but consistent with the logic of spell termination found in interstate and civil wars, we employ the semi-parametric Cox Proportional Hazards estimator with robust standard errors, clustered on the country to test our argument.[20] To facilitate the interpretation of the findings, we present our results in hazard ratios (exponentiated coefficients). Here, ratios above one increase the probability of the hazard occurring (i.e., spell termination), while values below one decrease the likelihood of a repressive spell ending.[21]

As designed, we estimate five equations: one baseline model that only includes the control variables and four models that account for different specifications. We use a p-value of .05 or lower as the standard to assess the statistical significance of the independent variables.

Empirical Findings

In Table 1, Model A, we test a baseline model that includes some of the leading explanations for state repression. Consistent with the existing literature, civil conflict, population, and youth bulges are all below one and statistically significant, suggesting that once started these make LSSRs go on longer. We further see that the hazard ratios for civil war termination and independent judiciary are greater than one and statistically significant. This suggests that when a civil conflict recently terminated as well as when the state in question has a more independent judiciary, repressive spells are shorter. In contrast, we find little support for democracy and economic development, which are normally always statistically significant and substantively important in studies of repressive variation (e.g., Davenport 2007; Hill and Jones 2014). These findings make sense because democracies and developed economies are less likely to start spells in the first place (results not shown). We find no support for our measure of civil resistance which differs from prior expectations about behavioral threats.

In Table 1, Model B, we explore the impact of diverse variables on the termination of LSSR that are believed to have an impact on state repression. The statistical results suggest that most of these variables have little systematic impact on the termination of large-scale state repressive behavior.[22] For example, military intervention, naming/shaming, the international law variables, PTAs and non-violence direct action all fail to reach standard levels of significance. Economic sanctions *prolongs* rather than shorten spells, which is consistent with some findings in the existing literature (Peksen 2009; Wood 2008) but generally against most expectations. Additionally, we find that FIRC are associated with shorter spells. While this result is interesting with potentially important policy implications, it is worth noting that there are only two cases in

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the data where a FIRC led to spell termination (i.e., Grenada and Panama). Consequently, the finding should be interpreted with some caution.

Table 1, Model C, we present the model with democratization, our variable of theoretical interest. As expected, the findings indicate that democratization increases the likelihood that an LSSR spell terminates. The hazard ratio is 2.112 and significant (p<.01), indicating that a repression spell is *112% more likely to end when a state has democratized* within the last five years.

In Table 1, Columns D and E, we present the full specification with the control variables along with the relevant factors generally found in a repressive model. Note that we present two models – one with and one without the UN naming/shaming variable due to missing data with this variable that reduces the total number of observations by almost 600. In Table 1 Column D, we present the model with the naming/shaming variable included. The results are largely consistent with the specifications from above, although in this model independent judiciary and economic sanctions are no longer statistically significant. More importantly, the hazard ratio for democratization is greater than one and significant, indicating (again) that democratization is associated with shorter spells. Also consistent with the results above, the variables that are most highlighted in discussions regarding the raising of costs with state repression have essentially no impact on the duration of repressive spells.

In Table 1, Column E, we present the full model without the UN naming/shaming variable. Here, once more we find that democratization is statistically significant and associated with reducing spell duration. The control variables are largely consistent with the previous models. In line with the baseline model, independent judiciary returns to statistical significance, although sanctions remain insignificant.

(Insert Table 1 About Here)

As revealed, the findings are generally in line with our theoretical expectations that it takes a fundamental shift/perturbance to the political apparatus in order to stop governments from continuing to violate human rights on a large-scale. While important and providing an important clue to how repression ends, we recognize that democratization is very difficult to achieve as a policy in its own right. Consequently, this makes changes in democracy worthy of attention so that the process by which LSSR spells are ended can be clearer. To begin a discussion on this issue, we briefly examine the determinants of democratic transitions among highly-repressive, autocratic regimes.

Clearly, democratization is not an easy policy. Scholars disagree on the causes of democratization, as existing research has found very few (if any) consistent predictors of such transitions (e.g., Gassebner et al. 2012; Ulfelder 2013). Further, we are interested in a different set of cases than most work on the subject. Here, our interest is limited to highly-repressive autocrats, while the existing literature studies democratization across all types of nondemocratic regimes. Nonetheless, and despite these challenges, we draw on the extant literature, and our interest in spell termination to model democratic transitions in LSSR. In particular, we assess the determinants of democratic transitions using the same set of variables from the spell termination model plus three additional variables based on the democratization literature.

First, we add a democratic diffusion variable to the model, measured as the proportion of neighboring states that have democratized in the previous two years using the Cheibub et al (2010) data. Several reasons exist for this inclusion. There is the demonstration effect where actors learn from and emulate neighboring countries that have undergone successful transitions (e.g., Bormann and Gleditsch 2012; Gledistch and Ward 2006). Scholars also suggest that

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neighboring democracies provide more resources to democratic movements, while others argue the costs of ruling for autocrats increase as democracy becomes more common among neighbors (e.g., Celestino and Gleditsch 2013).

Second, we add the number of previous democratic transitions in the country to the model again using the Cheibub et al (2010) measure. Similar to the demonstration effect, domestic actors will be more confident that they can successfully democratize when the state has a history of doing so, leading domestic actors to be more likely to push for such transitions in the current period (e.g., Gassebner et al. 2012).

Third, we include a variable that accounts for rainfall shocks in the model. The variable measures the annual deviation in precipitation from the 10-year moving average at the country-year level (Dell et al. 2012) and Salehyan and Hendrix (2014). [23] We take the absolute value of this variable because we expect both positive and negative shocks to be linked with democratization. Several scholars find, for example, that climate shocks such as droughts and/or floods can lead to a window of opportunity for democratic transitions due to its impact on economic growth (Aidt and Leon 2016; and Ciccone 2011). That is, climate shocks can lead to domestic dissent following economic downturns, which in turn, can lead some autocratic regimes to make democratic concessions to the opposition, leading to transitions.

In line with the existing literature on democratic transitions, we recode the response variable such that it equals 1 if the state democratized in the current year, and zero otherwise.[24] Drawing on the same research, we also use dynamic probit to estimate our models in which we present the results for the sample estimated on repressive, autocratic regimes (Przeworski et al. 2002; Boix 2003; and Dunning 2008). This allows us to calculate the likelihood that a

government in a repressive spell that started the year as an autocratic regime transitions to a democracy by the end of the year.

(Insert Table 2 About Here)

The three new variables are all statistically significant and in the expected direction. Most interestingly, we find that civil resistance is associated with a higher probability of democratization. These results are consistent with several other studies that find a relationship between nonviolent movements and democratization (Bratton and Van de Walle 1997; Celestino and Gledistch 2013; Chenoweth and Stephan 2011; Ulfelder 2005). Along with our perturbance argument, scholars suggest that civil resistance is successful because it leads to mass participation, destabilizing and undermining autocratic rulers and ultimately causing them to make concessions/movements towards democracy to offset the increased pressure on them (Celestino and Gledistch 2013; Chenoweth and Stephan 2011).[25]

In contrast and consistent with our earlier findings, we find little support for factors that generally predominate discussions about stopping repression. All three types of military interventions show no statistical relationship with democratic transitions. This is in line with previous studies on the topic that have found very little systematic support for external interventions on democratization (i.e., Bueno de Meqsquita and Downs 2006; Downes and Monten 2013; Pickering and Peceny 2006). The other variables highlighted in the literature also show no systematic relationship.

Considering Endogeneity and Selection

Related to the above discussion, some readers may be concerned that our results concerning democratization suffer from selection effects and that these findings are spurious. To address this, we use instrumental variable (IV) regression to account for the omitted variable

problem that is the source of the potential endogeneity issue here. IV regression requires that a variable meet two assumptions to be considered a valid instrument: 1) it must be correlated with democratization but 2) it must be uncorrelated with the error term in the primary model (and subsequently spell termination) conditional on the independent variables. While recognizing how difficult it is to find a valid instrument, we employ the rainfall shocks variable from above as an instrument.[26] As noted above, existing literature finds that climate shocks are linked with democratic transitions, satisfying the strength of instrument criteria. At the same time, this variable meets the exclusion restriction assumption because we see no theoretical reason to believe that rainfall is correlated with the error term (and thus spell termination) except through the independent variables which satisfies this assumption.[27]

(Insert Table 3 About Here)

As you can see in Table 3, the results suggest that democratization is associated with a greater likelihood of termination, even when accounting for the potential endogeneity issue. While we recognize the challenges that come with estimating IV regression and we should therefore interpret the results with some caution, they do suggest that the primary finding that democratization is associated with a greater likelihood of spell termination is robust to concerns related to selection effects.

Conclusion

Our research began with an interest in understanding what stops ongoing large-scale state repression (or LSSR). Despite immense humanitarian, political, academic and popular interest in the topic as well as similar research in the areas of interstate and civil war, spells are something not previously explored in the field of state repression. To investigate this line of inquiry, we developed a theoretical argument where it was maintained that, once initiated, LSSR is not likely

to change unless the decision-makers and implementers of repressive behavior were affected by some factor that significantly raised costs like democratization. Without a cost that perturbed the repressive process, it was simply not expected that a spell would end.

From our investigations of 239 high-repressions spells and generally in line with our argument, we find that democratization does indeed significantly contribute to the termination of repressive spells. To stop LSSR, it is clear that those interested in this outcome need to strongly consider how best to move the government of interest toward democracy. Attempting to understand the process involved in greater detail, additional results disclose that non-violent movements tend to increase the likelihood of democratization. Interestingly, we find that these movements have little direct impact on termination themselves.

In contrast to much research investigating variation in repression (i.e., movement up and down some scale), we find that many factors believed to impose a cost on the repressive process have little systematic impact on spells of large-scale state repression. This non-result may be surprising to many – especially since this deviates from numerous studies, popular perception and common policy prescriptions put forward to address the problem. We find the result unsurprising, however, given: 1) the sticky nature of the repressive process once underway and 2) the general inadequacy of many policies with perturbing those involved in state repression. From these findings, numerous implications emerge.

Implications for researchers. Influenced by the current study, scholars interested in understanding why/when LSSR ends should prioritize democratization into their standard repertoire of solutions and independent variables. These findings reinforce the work of the "domestic democratic peace" where the level of democracy and, to a much lesser extent, certain increases in the level of democracy have served as a (or "*the*") pacifying influence on state

repression. This effort should be expanded. Our research also suggests that we should not dismiss conventional approaches to internationally-oriented humanitarianism, but we should think more creatively about exactly which, how and when they matter. For example, they may interactively have an influence or perform in some delayed fashion. Additionally, although we began an assessment of selection effects above, it is clear that there should be more attention given to the issue of why large scale state repression starts in the first place and which countries select into this group.

Implications for policymakers, advocates and activists. It is further clear from our work that those interested in stopping state repression should focus their efforts on facilitating democratization. Now, this is not to say that such an outcome is easy. Rather, it is to say that there is some rigorous empirical grounds for such a pursuit and that such a finding should be conducted as the merits of democratic promotion are debated. How does one facilitate democratization? We find that facilitating democratically-oriented social movements might work but there may be other answers/pathways. Addressing this topic is likely among the most meaningful subjects and controversies in political science (e.g., Moller and Skaaning 2012). We hope that this article has facilitated as well as inspired the continued pursuit of this topic as we find it linked to one of the largest concerns for humankind – ending large scale state repression.

Biographical Statements

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Tables

Table 1: Termination of High-Repression Spells

	A	B	C	D	E
Democratization			**2.112*** **(.499)**	**1.985*** **(.585)**	**2.122*** **(.546)**
Civil Resistance	0.750 (0.426)			0.541 (0.378)	0.584 (0.346)
Civil Conflict	0.251* (0.0914)			0.264* (0.121)	0.284* (0.102)
GDP per Capita	1.000 (0.0000211)			1.000 (0.0000291)	1.000 (0.0000235)
Population	0.733* (0.0412)			0.742* (0.0516)	0.725* (0.0413)
Youth Bulge	0.955* (0.0119)			0.950* (0.0162)	0.945* (0.0139)
Democracy	1.031 (0.239)			0.865 (0.223)	0.874 (0.200)
Rule of Law	3.354* (1.636)			2.406 (1.264)	3.547* (1.699)
Civil War Termination	6.737* (3.727)			6.575* (4.004)	6.054* (3.378)
FIRC		8.957* (3.449)		2.698* (1.340)	2.935* (1.382)
PTA		1.504 (0.729)		1.703 (0.776)	1.765 (0.608)
Economic Sanctions		0.324* (0.170)		0.425 (0.211)	0.374 (0.194)
Anti-government Interventions		0.476 (0.194)		0.603 (0.228)	0.682 (0.232)
Pro-government Interventions		0.869 (0.280)		0.905 (0.279)	0.875 (0.251)
Neutral Interventions		0.611 (0.212)		0.597 (0.234)	0.637 (0.263)
UN Naming and Shaming		0.586 (0.187)		0.707 (0.218)	--
CAT		1.285 (0.319)		1.112 (0.302)	0.864 (0.197)
ICCPR		0.821 (0.165)		0.788 (0.167)	0.841 (0.160)
Observations	2311	1836	2432	1827	2300

Based on Cox Regression. Robust standard errors in parentheses, clustered on country. * $p < .05$

Table 2: Determinants of Democratization

	A
Civil Resistance	3.066[*]
	(0.604)
Rainfall Shocks	0.427[*]
	(0.175)
Past Democratic Transitions	1.978[*]
	(0.357)
Neighbor Democratization	15.22[*]
	(5.860)
Sanctions	-1.180[*]
	(0.564)
Anti-government Interventions	-0.223
	(0.424)
Pro-government Interventions	0.703
	(0.458)
Neutral Interventions	-0.0962
	(0.516)
CAT	-0.0366
	(0.555)
ICCPR	-0.0953
	(0.461)
Civil Conflict	1.011[*]
	(0.415)
GDP per Capita	-0.000520[*]
	(0.000156)
Population	-0.0784
	(0.164)
Youth Bulge	-0.0330
	(0.0599)
Rule of Law	5.502[*]
	(1.267)
Civil War Termination	-0.889
	(1.113)
Constant	-3.776
	(2.801)
Observations	1573

Robust standard errors in parentheses, clustered on country. [*] $p < .05$

Table 3: Termination of High-Repression Spells, IV Regression

	A
Democratization	**3.708***
	(0.464)
Civil Resistance	-0.756*
	(0.140)
FIRC	-0.215
	(0.421)
PTA	0.117
	(0.172)
Sanctions	-0.0658
	(0.315)
Anti-government Interventions	-0.121
	(0.106)
Pro-government Interventions	-0.176
	(0.117)
Neutral Interventions	0.0643
	(0.183)
CAT	0.0346
	(0.0716)
ICCPR	0.00187
	(0.0949)
Civil Conflict	-0.341
	(0.245)
GDP per Capita	0.0000273*
	(0.0000102)
Population	-0.0596
	(0.104)
Youth Bulge	-0.0117
	(0.0156)
Rule of Law	-0.468
	(0.407)
Civil War Termination	0.202
	(0.412)
Constant	0.506
	(1.102)
Observations	2267

Robust standard errors in parentheses, clustered on country. $p < .05$. Time variables omitted.

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Endnotes

[1] Researchers have called the activities here many different things: e.g., genocide, politicide, democide, one-sided violence, human rights violations and atrocities but at the most elemental level behaviorally they involve the same actions of state actors: i.e., mass arrests, extra-judicial execution, disappearance and mass killing.

[2] Although Conrad and Moore (2010) and Bellamy (2015) are relevant and important, they only cover a small part of what is commonly meant by state repression.

[3] In choosing this new unit of analysis, we build on the finding that lagged state repression is generally the most important predictor of repression. Although often deemed uninteresting, the strong effect of lagged repression is crucial for understanding termination; it not only strongly suggests that repressive spells exist, but also suggests that the phenomenon is not best evaluated on a yearly basis.

[4] This insight represents an important departure from existing work which argues that repression can be decreased as long as actors impose the "right" costs authorities.

[5] Conrad and Moore (2010) and Bellamy (2015) do not consider a broader range of international policies and thus no direct comparison can be made.

[6] The research here is different from more events-based work which attempts to understand the number of times specific repressive activities occur (e.g., Davenport 1995). While noteworthy, this work is generally interested in a different aspect of repression.

[7] There has been relatively little academic research on the "benefits" of state repression.

[8] Such an orientation has a long tradition in more historical investigations. For example, Hilberg (1971) notes that the attempted destruction of the Jewish population during the Holocaust was not undertaken by some small cadre of individuals but rather a huge coalition of organizations.

[9] There are other literatures that prove to be relevant. Research on "veto players" highlights the point that once a government has adopted a policy, it is not easily changed as diverse agents will block attempted shifts. Literature on organizational decision-making argues that policies are more likely retained than shifted (e.g., Steinbruner 2002). Staw (1981) argues after the initial cost-benefit analysis, individuals and groups involved become connected with the decision on a psychological level and do their best to avoid thinking of it negatively.

[10] Moreover, in the historical literature, there are consistent references made to different repressive spells (e.g., COINTELPRO in the US or the "Dirty War" in Argentina).

[11] We recognize that other institutions affect leadership selection such as courts. We nonetheless focus on the most direct form of democratic accountability, elections.

[12] The significant-cost-raising orientation we offer here is complimentary to but distinct from the elimination of threats orientation offered in the literature (e.g., Davenport 1995; 2007a; Zanger 2000) which is more associated with benefits. While generally sympathetic to this argument and one of the co-authors of this piece has been intricately connected with this work (XXX), we believe that behavioral challenges/threats only provide part of the explanation for state repression.

[13] Some may be concerned that we are "losing" information contained within the original 5-category measure (effectively dichotomizing it) but the differences highlighted in the original data are irrelevant from the perspective of those interested in government repression that is violent, widespread and systematic.

[14] In the appendix, we estimate several models using different coding rules for high-repression spells. We find consistent results across the different models, suggesting that our results are not specific to the coding rules used.

[15] Spells that do not end by 2007 are treated as right-censored.

[16] It is recognized that the democratization variable is less relevant for democratic regimes. In the appendix, we examine the impact of liberal leadership changes in democratic states. We find that high-repression spells are more likely to terminate following liberal leader changes.

[17] The authors define a campaign as a series of observable, continuous, purposive mass tactics in pursuit of a political objective (NAVCO codebook, 2).

[18] We employ a rule of law measure from Keith et al. (2009) in the online appendix. Our results are consistent with that presented above.

[19] Several of our variables are recoded based on the five-year window that we use for democratization in order to determine if there are any lagged effects. The results are consistent with the main text (see appendix).

[20] In the appendix, we also estimate a stratified Cox Model where we stratify on the number of previous high-repression spells. We include a control variable that the counts the number of previous spells for each country. The results are consistent with those above.

[21] The key theoretical variable – democratization – does not violate the PH assumption across the models that we estimate in the main text and the robustness checks presented in the Supplementary Information.

[22] Interestingly, this is a finding generally consistent with Hill and Jones' work.

[23] While scholars find support for this relationship, we primarily include this variable because we employ it below in the instrumental variable regression models. The results are consistent with the above when we exclude this variable from the model.

[24] The results are consistent if we use the five –year democratization variable from the primary models above.

[25] These movements may also cause elite defections and greater support for the opposition if autocrats respond to them with increased levels of repression. We recognize that international actors may provide important assistance to their efforts. While acknowledging these connections, we maintain that this network serves more as support system than it does a primary determinant and thus we feel that it is more than appropriate to focus our attention on the local-level effort. This is consistent with the meta-review offered by Hill and Jones (2014).

[26] We believe that rainfall shocks is a stronger instrument than both past transitions and regional transitions because while rainfall is clearly exogenous, the latter are both endogenous to numerous factors that are likely also related to spell termination.

[27] The strength of the instrument is assessed using an F-test, in which the general rule of thumb is that the F-statistic should be greater than 10 (Staiger and Stock 1997). The F-statistic is 16.38 and thus satisfies this assumption. We unfortunately cannot assess the exclusion restriction assumption because the Hanson J statistic requires that the model be over-identified (i.e., more instruments than endogenous regressors). We only have one instrument and our model is not over-identified. Nonetheless, there is no reason to think that rainfall is directly correlated with spell termination. We estimate the IV regression using the ivprobit command in Stata. Using this estimator requires us to switch from the Cox Model to discrete duration models, such as probit with cubic polynomials of time. As a robustness check, we estimate our main models using probit regression. Our results are consistent with the main findings and the IV regression.