Supplementary Material
"Political Repression and the Destruction of Dissident Organizations: Evidence from the
Archives of the Guatemalan National Police"
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Replication data are available at http://www.sullivanchristophermichael.com.

This section presents an instrumental variables analysis employing human rights NGO
reports to search for plausibly exogenous variation in repression. The section (1)
discusses the strengths and weaknesses of the approach, (2) presents evidence of
relevance and exclusion, (3) discusses the generalizability of local average treatment
effects, (4) reports the results, and (5) provides a replication across samples of high and
low-capacity municipalities. Results affirm the conclusions of the study.

This section reports the results from a series of analyses probing the robustness of the
study's primary results to the incorporation of variables derived from population-centric
approaches. The study's primary results prove robust to the inclusion of measures for
indiscriminate repression, territorial control, and leader's job insecurity.

This section presents a more extensive discussion of the data used in the study. The
potential sources of data bias are discussed alongside a more extensive review of the
sampling protocols. The section also presents the coding manual used to collect the data.

Section 1: Instrumental Variables Analysis

Recognizing the difficulty of drawing causal inferences with observational data, the study

supplements the cross-sectional time-series analyses (presented in the primary text) with

an instrumental variables (IV) design to identify how plausibly exogenous variation in

repression targeting mobilization activities or overt, collective challenges impacts dissent.

The approach follows several recent studies modeling the endogeneity of repression and

dissent (Francisco 1995; Moore 1995; Carey 2006; Young 2013; Ritter and Conrad

forthcoming). For the analysis, NGO reports documenting human rights abuse are used as

an instrument to identify as-if-randomization in the application of political repression.

Specifically, as its instrument the study utilizes the percentage of human rights abuses

identified by human rights NGOs (calculated as the number of abuses recorded in NGO

reports over the total number of abuses identified in the data).[1] Data on NGO human

rights reporting come from the publications of five human rights organizations—Human

Rights Watch, Amnesty International and three Guatemalan human rights groups. They

were compiled by the Center for Human Rights Research (CIIDH), a nongovernmental

organization based in Guatemala City that conducted a thorough review of published

human rights documents during Guatemala's truth and reconciliation processes (Ball et

al., 1999; Davenport and Ball 2002). The CIIDH data are used to generate monthly

measures of the percentage of human rights abuses recorded by NGOs in a municipality

for each of the preceding six months.[2]

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[1] The percentage of acts identified is used rather than the number of identified actions to reduce bias that might predispose the estimates towards more violent areas. There are no instances where this measure takes a value greater than 1.

[2] Because data on the publication date is not available for a large majority of the human rights reports, the date in which the reported violation took place is used to identify the human rights report, rather than the date in which the report was published. This seems to be relatively inconsequential as the available data suggests that the human rights reporting took place quickly. Among the reports for which data on the

The core criteria for a valid IV design require that (1) the instrument impact variation in the independent variables of interests (the relevance criterion), (2) that the instrument is independent of other causes of the dependent variable (exogeneity of the instrument), (3) and that the instrument only influences the dependent variable through its impact on the endogenous treatment (the exclusion restriction). Fortunately, researchers have recently devoted considerable attention to investigating the relationship between NGO reporting and government behavior, with numerous studies showing how naming and shaming can have important impacts on human rights practices. To begin, there is evidence that naming and shaming can increase international pressures to reduce human rights abuses, either in the form of sanctions or in the likelihood of humanitarian intervention (Keck and Sikkink 1998; Risse et al. 1999; Esarey and DeMeritt 2013; Murdie and Peksen 2013a; 2013b). There is also a variety of research that probes the relationship between naming and shaming and repression directly. Though results are split between those who see naming and shaming reducing repression (Franklin 2008; DeMeritt 2012; Krain 2012; Murdie and Davis 2012; Hendrix and Wong 2013) and those who see this behavior leading to a shift in tactics (Ron 1997; Hafner-Burton 2008), the evidence is consistent in concluding that there are observable changes in repressive practice.

With regards to the particular case of Guatemala, it is well documented how publication of human rights abuses threatened to end the flow of aid from the United States (e.g., Schirmer 1998; Keck and Sikkink 1999; Doyle 1999; Grandin 2004). President Carter, for example, cut off US military aid following the publication of

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publication date is available, 85% of the reports were published in the same month in which the reported violation took place. 93% were published within two months of the violation occurring. Less than 2% were reported more than six months after the violation occurred.

widespread human rights abuses by the Guatemalan government. And while President Regan famously increased US economic aid along with the provision of munitions and training to the Guatemalan Army, members of the State Department began to actively pressure for a complete cessation of US aid as greater evidence of the ongoing human rights abuses became public (Grandin 2004; Doyle 1999). Pressure further increased when the Inter-American Commission on Human Rights of the Organization of American States began actively researching human rights abuses in Guatemala in 1982, using survivor interviews documented by human rights NGOs as a principal source of evidence (Carmack 1988).

The case evidence suggests that while international pressures were not sufficient for ending repression in Guatemala, the Guatemalan government was acutely aware of pressures from human rights groups and actively adjusted its strategy in order to minimize the documentation of repressive behavior. Information from within the Guatemalan security apparatus (e.g., Guberek 2012; Schirmer 1998) and the U.S. state department (Doyle 1999) identify both the awareness of the Guatemalan government to NGO monitoring of human rights abuses and conscious attempts to limit publications of abuse. Schirmer (1998, 247) documents a particularly telling example from an interview with a former army officer:

- This [squatting] must be dealt with intelligently so it doesn't cause waves in the international press.
- Q: How do you mean "intelligently"
- Such as paying them off and not as they have been dealt with before— you know, cleansed.
- Q: Why the shift in tactic?
- Because we are more conscious of the international coverage now. For the loans. But we are not so certain that this [less repressive method] will in fact work.

In many cases, the shift in strategy that followed NGO reporting involved efforts to conceal repression, either through shifting its timing or location or through adopting tactics that were less easily monitored (Guberek 2012). For example, Schirmer (1998, 247) notes how following the case discussed in the interview, twelve leaders of the squatting movement were abducted, and "their tortured bodies were later discovered in garbage bags in the city dump." The Guatemalan government also turned its repression to target human rights monitors (Anderson 1989). Human rights organizations were so severely repressed that in 1978 international human rights NGOs began to pull out of the country and establish their offices in Mexico or Costa Rica (Sikkink 2007, 138).

Following from this, one important consideration with regards to the exogeneity of the instrument has to do with whether NGO's ability to measure human rights was correlated in some way with prior levels of repression. In addition to the repression that was directed at the NGOs there may be more indirect connections between prior repression and the ability of NGOs to report abuse. Areas with the most repression are also areas where NGOs would have a harder time documenting abuses. While it is true that some of the NGO reporting was done based on witness statements taken from populations fleeing mass violence, assuming that NGOs were able to record human rights abuses in a manner that was uncorrelated with repression may be unfounded.

With regards to dissident behavior and NGO reporting, the exclusion criterion necessitates that NGO reporting only influence overt, collective challenges through its impact on repression. While there is a great deal of evidence to suggest the government was sensitive to the information reported by human rights NGOs, it is less obvious that the publication of NGO reports detailing governmental human rights abuses impacted the

strategic behavior of dissidents. The presence of human rights organizations has been

shown to help to boost mobilization (Murdie and Bhasin 2011; Bell et al. 2012; Bell et al.

2014). This is particularly true when they are active in domestic politics, but even from

abroad human rights organizations can boost domestic protests. But such accounts stress

the impact of human rights NGOs on domestic protest through their presence, not through

their reporting. And it is not obvious from existing work how variation in the reporting of

human rights abuses might influence domestic dissent. For human rights reporting to

influence dissidents, they would have to possess the information, strategic interest, and

capacity to respond to its publication (Dunning 2012). It could be argued that human

rights reporting signals to activists that the NGO community is present and supports their

endeavors (compare Vreeland 2008). To the extent this is true, it would reduce our

confidence in the exclusion of the instrument. It would also bias the estimates of

repression's effects in a positive direction, effectively increasing the probability of

rejecting Hypothesis 1 (regarding the repression of mobilization activities) while

decreasing the probability of rejecting Hypothesis 2 (regarding the repression of overt,

collective challenges).

With regards to the Guatemalan case, in many instances dissent operated in

extremely rural regions of the country, where news of the publication of an NGO report

might take months to reach. When news of an NGO report did reach dissidents, it is not

clear how they might have responded to its publication. Recall that the instrument is not

measuring the number of reports or even the number of abuses reported (though these

variables are surely related), but the percentage of abuses committed that were recorded

by human rights organizations. Whereas NGO human rights reports inform the state that

there are human rights groups operating in a locale that have the capacity to monitor repressive behavior, the publication of a report does not provide similarly strategic information for dissidents. It is perhaps plausible that the publication of information on human rights abuses might (a) incentivize dissidents to move into a locale to engage in challenges where they might use the publication as a rallying call or (b) incentivize dissidents to move out of a local to avoid future repression. But these two potential outcomes are founded on the belief that without the publication of the report, dissidents and their potential supporters would remain ignorant of repression. Because dissidents are the targets of repression, they are likely to have first hand knowledge of human rights violations that supersedes the publication of any human rights reports. Then there is a question of capacity. Mobilization is a highly localized activity that is dependent on both prior activity and context (e.g., McAdam 1986; Gould 1995). As a result, mobilizers have limited capacity for picking up and strategically transplanting themselves to protest sites that might appear superior.

Still, returning to the general population, it could be argued that NGO publications motivate villagers to join dissident organizations. In this case, the publication of NGO reports could provide some information to the citizens about important outcomes, such as their likelihood of victimization and who can be expected to prevail in the civil conflict. But it is important to consider that civilians are most often the source that NGOs rely on to generate reports on government repression. In this case, the pathway of information would flow in the other direction, from citizens to NGOs, and so the publication of the report would not tell the citizens any new information about local repression. But it might still provide some signals about international or domestic support

for challenging the government, and in this case could motivate citizens who were attempting to sit on the sidelines to begin participating in challenger organizations.

It is also important to consider other possible causal pathways. For example, social movements advocating for human rights may influence human rights reporting. To the extent that the human rights groups were simultaneously engaging in overt collective challenges, experiencing repression, and publishing reports, the instrument would no longer be exogenous of the relationship between past and present overt, collective challenges. However, human rights NGOs made up only a small fraction of the organizations experiencing repression in Guatemala (see CEH 1998). Within the AHPN data, less than 1% of challenger behaviors were identified as being associated with human rights groups (ANONYMIZED).

Finally, it should be noted that the assumptions of the model require only that the instrument be conditionally independent of alternative causal pathways. Models A2-3 and A5-A6 in Table AIII include additional controls for the (local and regional) presence of human rights NGOs, which goes some distance towards addressing the considerations discussed above.[3]

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[3] There are also limitations affiliated with the introduction of controls into IV models. This is discussed below.

Table AI: P-Values for Balance Tests of Pre-treatment Covariates

	Repression of Mobilization	Repression of Overt, Collective Challenges	Repression of Mobilization	Repression of Overt, Collective Challenges
Overt, Collective Challenges	0.266	0.195	0.451	0.630
Mobilization	0.108	0.490	0.286	0.561
Spatial Lag Overt Collective Challenges	0.485	0.187	0.997	0.908
Spatial Lag Mobilization	0.520	0.171	0.393	0.916
Democracy	0.472	0.164	0.728	0.129
% Indigenous	0.530	0.236		
% Literate	0.558	0.293		
Population	0.178	0.273		
First Differencing the Variables			Y	Y

Support for the as-if randomization of repression yielded by the documentation of human rights abuses in NGO reports can be found in Table AI. The table presents analyses that examine a series of models regressing instrumented measures of repression on pre-treatment covariates. The results present evidence of the degree of pre-treatment balance, which Dunning (2012, 239-241) identifies as "the central quantitative tool used to validate natural experiments." The pre-treatment measures included in the table refer to relevant covariates measured during the municipality-month prior to the assignment of treatment. The table reports p-values for the statistical significance of the difference in means of the treatment and control groups. Here, we can see that areas where repression

and was not applied (as a function of where NGOs did and did not report abuse) had comparable pre-treatment characteristics. If the assignment mechanisms is truly as-good-as random, treatment and control areas should have roughly equal to one another prior to treatment (and the p-values should be insignificant).

The first thing to note in looking at the table is that instrumented measures of repression targeting mobilization activities and repression targeting overt, collective challenges appear uncorrelated with pre-treatment measures of overt, collective challenges as well as pre-treatment measures of mobilization. The left two columns represent the results of a series of IV regression models regressing the two "treatments"— instrumented measures of repression targeting mobilization activities and repression targeting overt, collective challenges—on pre-treatment covariates. The right two columns replicate this analysis except that the equations employ first-differenced changes in time variant independent and dependent variables. While one cannot directly test the instrument's exclusion, the table presents indirect evidence suggesting that prior to the application of treatment the exogenous influence of human rights reporting on repression was uncorrelated with dissident behavior.

With regards to the previous discussion about how NGO reporting may be influenced by prior levels of repression (the exogeneity assumption), the fact that the national-level measure of democracy falls just short of conventional standards for statistical significance becomes important to note. This suggests that there may be some relationship between formal national democratic institutions and the access NGOs have to report human rights abuse. Because democracy is also one of the key determinants previously identified to shape human rights abuse, the possibility that democracy has

some correlation with NGO reporting patterns opens up a strong challenge to the interpretation of the IV results. To the extent that this relationship can be controlled for and conditional exogeneity can be identified, there is may be some veracity to the results. Still, as Dunning (2012, 249) notes, introducing controls into the IV models, "does not rise to the standard of as-good-as-as-if-random."[4]

One remaining note concerns issues of state capacity. As a general rule, repression targeting mobilization activities is positively correlated with state capacity (as measured by municipal population and literacy rates), which suggests that such behavior is more likely to be observed in high capacity regions.[5] While the instrumented measures of repression appear uncorrelated with measures of capacity (as identified in Table II), several steps were taken to address the general relationship among these variables. First, the measures of state capacity are included as control variables in the analyses below. Second, the analysis is replicated below on subsamples of 'high capacity' and 'low capacity' municipalities. The implications for generalizing the argument are discussed in the discussion section of the primary text.

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[4] I thank the editor and R1 for their thoughtful consideration of these important points.
[5] Interestingly, when looking at zero-inflated negative-binomial models, state capacity is correlated with the count of repression directed at mobilization, but not with its occurrence.

Table AII: Instrumental Variables First-Stage Impacts of Human Rights Reporting on Repression

	Repression of Mobilization	Repression of Overt, Collective Challenges	Repression of Mobilization	Repression of Overt, Collective Challenges
% Of Human Rights Abuses Documented in NGO Reports				
Lag 1 Month	-0.298* (0.129)	0.069*** (0.021)	0.503** (0.172)	-0.043*** (0.009)
Lag 2 Months	0.024 (0.080)	0.054*** (0.014)	0.171 (0.116)	-0.007 (0.009)
Lag 3 Months	0.520*** (0.121)	-0.139* (0.055)	0.538*** (0.152)	-0.183** (0.065)
Lag 4 Months	0.315*** (0.080)	0.169*** (0.045)	-0.331*** (0.089)	0.310*** (0.100)
Lag 5 Months	0.363*** (0.065)	0.323*** (0.095)	-0.144* (0.067)	0.146*** (0.045)
Lag 6 Months	2.232*** (0.698)	0.080*** (0.017)	1.522** (0.484)	-0.248** (0.079)
First Differencing the Time Variant Independent and Dependent Variables			Y	Y
F-Statistic	91.08	14.13	60.89	9.73

Coefficients and standards errors (in parentheses). * $p<.05$, ** $p<.01$, ***$p<.001$ (Two-Tailed Test).

Table AII presents the first stage results for two IV equations employing lagged

measures of the percentage of human rights abuses documented in NGO reports to

instrument how repression (targeting mobilization or overt, collective challenges) impacts

overt, collective challenges. In discussing validity of the instrument, it is also worth

noting that the measure of NGO reports employed in this IV analysis is not able to differentiate between repression directed at mobilization and repression directed at overt, collective challenges. Thus, while six measures of the percentage of abuses documented are employed as instruments (lags from t-1 through t-6), the analyses essentially use a single instrumental variable to predict to different instrumented variables. In considering this issue, two points should be discussed. First, the two forms of repression may have different propensities for appearing in NGO reporting. Because repression directed at mobilization occurs underground, this type of repression leaves fewer witnesses. By contrast, repression directed at overt, collective challenges may be more easily observed by outside parties. Second, NGO human rights reporting may influence repression directed at mobilization and repression directed at overt, collective challenges in dissimilar ways. This is directly related to the first concern because repression directed at clandestine mobilization activities may actually increase in response to NGO reporting, while the state may be less inclined to commit repression directed at overt, collective challenges. For these reasons, it would be preferable to be able to measure the NGO reporting measure as two proportions—one for repression of mobilization and another for repression off overt, collective challenges. Unfortunately, the NGO data do not record the targets of repression with sufficient detail to reliably separate out repression directed against mobilization from repression directed at overt, collective challenges.

For each of the six lags employed as instruments, Table AII presents their coefficients, standard errors, and levels of statistical significance. The table also presents the F-Statistics for the joint significance of the six instruments. The left two columns present evidence from equations employing direct measures, while the right two present

results from first differenced equations.[6] Across the models, the majority of the lagged instruments appear significantly correlated with measures of political repression. It is worth noting, however, that the identified effects of the different lagged NGO reporting measures fluctuate dramatically across time, across models, and across repression types. In some settings these values are positive and significant, in others they are negative and significant. As a result, the precise effect of NGO reporting on repression is not entirely clear. The F-statistics for joint significance are highly significant across the four equations. These measures range from 9.75-91.08. While Sovey and Green (2012) recommend an F-statistic of at least ten to have confidence in the combined significance of multiple instruments, this metric is admittedly arbitrary as it is impacted by both the number of instruments and the number of observations (see Dunning 2012, 241). For this particular test, the first-differenced equation for repression targeting overt collective challenges falls just slightly below ten (9.73), while the other three equations have F-statistics well above this cut off. This suggests that there is some significant connection between the percentage of human rights abuses documented during the previous six months and subsequent patterns of repression. But returning to the idiosyncratic signs and significance of these variables, it becomes difficult to decipher precisely what that effect is. This contrasts sharply with other efforts to employ IV models to separate out repression and dissent, such as Ritter and Conrad (forthcoming). In that paper, the expectation is that rainfall will negatively impact dissent and not affect repression. By contrast, here we see that there are a variety of different effects of NGO reporting on repression. Combined with the cross-national research on NGO reporting and repression

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[6] The left two columns were estimated simultaneously as were the right two. These results correspond to equations 7 and 10 in Table III of the main text.

discussed earlier, these first stage results beg for additional work into how reporting

shapes repressive behavior.

Table AIII: Instrumental Variables Second-Stage Estimation Overt, Collective Challenges

	A1	A2	A3	A4	A5	A6
Repression of Mobilization	-0.555* (0.241)	-0.721*** (0.203)	-0.711** (0.249)	-0.974*** (0.232)	-0.721*** (0.203)	-0.711** (0.250)
Repression of Overt, Collective Challenges	17.345*** (2.946)	4.015* (1.824)	4.028** (1.856)	4.608* (2.165)	4.016* (1.824)	4.028* (1.856)
Controls		Y	Y		Y	Y
Annual Fixed Effects			Y			Y
Departmental Fixed Effects			Y			Y
First Differencing the Variables				Y	Y	Y
Anderson LR Statistic	93.063	26.401	26.201	25.266	26.401	26.201
P-Value	0.000	0.001	0.001	0.001	0.001	0.001
Hansen's J Statistic	3.227	1.646	1.540	1.841	1.646	1.540
P-Value	0.521	0.801	0.819	0.765	0.801	0.819
N	34,775	34,775	34,775	34,775	34,775	34,775

Coefficients and standards errors (in parentheses). * p<.05, ** p<.01, ***p<.001 (Two-Tailed Test).
Instruments: *% of Human Rights Abuses Reported by Human Rights NGOs* (six lags, t-1 – t-6)
Controls: *lagged overt, collective challenges, lagged mobilization, lagged repression of overt, collective challenges, lagged repression of mobilization, spatial-temporal lag of overt, collective challenges, spatial-temporal lag of mobilization, indigenous, literate, population, local NGO presence, regional NGO presence, level of democracy*

Table AIII presents the second stage results of the IV models estimating

repression's impact on overt, collective challenges. Across the six models, standard errors

are clustered by municipality (Dunning 2012, 175-178). Models are estimated using the

Generalized Method of Moments, which displays greater efficiency for calculating

heteroskedastic error terms produced by cross-sectional time-series research designs

(Hansen 1982).

The second stage in the IV models estimates the local average treatment effect

(LATE) of seemingly exogenous variation in repression on overt, collective challenges.[7]

The table also presents further evidence supporting both the relevance criterion and the

exclusion restriction. Along with the primary results of each model, results from

Anderson likelihood-ratio test of instrumental relevance are presented. In each case, the

significant p-values indicate that the excluded instruments are relevant for explaining

variance in the endogenous regressors. Sargan-Hansen J-Statistics of overidentification

are also presented. These tests are commonly used to examine the potential for statistical

endogeneity to influence the results. In each case, the p-values for these tests remain far

above conventional standards of statistical significance, supporting the validity of the

instruments.

Model AI in Table AIII estimates the equation with no control variables. Dunning

presents compelling arguments that IV models without controls are preferable, at least in

situations where the as-if randomization inspired by the instrument is plausible. If the

instrumented repression is truly exogenous, and not simply conditionally exogenous, then

the model without control variables should yield consistent estimates of repression's

LATE. The evidence above suggests that the model's instrumented repression was

independent of pretreatment measures of the control variables, but to check the validity of

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[7] With any instrumental variables model, attention needs to be paid to the types of units affected by the exogenous instrument and how they may or may not differ from the broader population of potentially treated units. Further discussion of the local generalizability of the LATE estimates as well as the generalizability of these estimates to cases outside Guatemala can be found below.

this assertion Models A2-A3 include the control variables identified above along with controls for local and regional NGO presence. Local NGO presence is a dichotomous measure of whether an NGO published a report about a given municipality in a given year. Regional NGO presence is measured as a spatial lag of the local NGO presence measure, which is calculated in the same manner as the spatial lags of dissident behavior discussed above. Models A4-A6 assume the same structure as models A1-A3, except that they estimate first-differenced changes in the principal time variant independent and dependent variables.

Turning to the results, Hypothesis 1 predicts that repression directed against mobilization activities will be negatively related to overt, collective challenges. Looking at Model A1, repression targeting mobilization activities is estimated to reduce overt, collective challenges at a rate of one fewer challenge for every two mobilization activities repressed. This effect is substantively significant. And these results are largely consistent across model specifications, which suggests the time-series data were reasonably stochastic. For five of the six specifications, repression of mobilization activities is estimated to reduce overt, collective challenges at a rate of about one fewer overt, collective challenge for every two mobilization activities repressed. Model A4 estimates a slightly larger impact, with an estimated effect of about one fewer overt, collective challenge for every mobilization activity repressed.

The second hypothesis predicts that repression targeting overt, collective challenges will be positively related to overt, collective challenges. Looking at the estimated LATE for repression targeting overt, collective challenges, the results support this contention. Model A1 estimates large substantive effects. In this case, repressing

overt, collective challenges is estimated to lead to 17 more overt, collective challenges than would have occurred had repression not occurred. While the results are consistent across specifications, the substantive size of this affect appears to be something of an outlier when looking at the results of the other models. In Models A2-A6, a single act of repression targeting overt, collective challenges is estimated to lead to around four additional overt, collective challenges than would be predicted had repression not taken place.

Combined, the results support both sets of hypotheses and suggest that political repression can have divergent effects depending on the types of organizational behavior it targets. Repression targeting mobilization activities is related to decreased challenger behavior, while repression targeting ongoing challenges is associated with increased dissent.

Local Average Treatment Effects

The IV models estimate the local average treatment effect (LATE) of seemingly exogenous variation in repression. With any instrumental variables model, attention needs to be paid to the types of units affected by the exogenous instrument and how they may or may not differ from the broader population of potentially treated units. In this case, the models estimate the effect of only those types of repression that could have been influenced by human rights reporting. In the language of IV analysis, this estimates the LATE for "compliers" (Dunnig 2012, 136-143; 290-293; Imbens 2009). Units in which repression would have been applied regardless of how NGOs had documented past human rights abuses (e.g., large military operations that were underway or covert and

undetectable activities) as well as repression that would never have been applied conditional on NGO reporting are not affected by our instrument and thus do not factor in the analysis (e.g., pacified sites).

It is important also to consider the context of the case when discussing the implications of the "local" on the estimated LATE. In this case two structural variables appear to be potentially significant. The first is the regime structure. Throughout the ten years under review, the country oscillated between a hybrid regime and full autocracy. As democracy has been shown to significantly reduce human rights abuses (e.g., Davenport 2007a; 2007b; Davenport and Armstrong 2004), the estimated impact of the types of repression being analyzed in this study might differ from those employed in democracies. Similarly, past research indicates that civil war is a strong predictor of escalated repressive behavior (e.g., Davenport 2007b; Valentino et al. 2004). Yet, while the Guatemalan government was severely repressive and relatively unchecked by democratic institutions, it does appear to have been responsive to the publication of human rights abuses. It is an empirical question whether their responsiveness to NGO publications is relatively consistent with more democratic or less conflict ridden states (thus yielding more generalizable LATE estimates), though there is evidence to suggest that non-democratic regimes such as Guatemala's are more responsive to NGO reporting (e.g., Hendrix and Wong 2013), while civil war states may be less responsive (e.g., Hafner-Burton 2008). To the extent that these two issues overlap (i.e., the potential differences between complier repression and other repression, and the responsiveness of conflict ridden, autocratic states to alter repressive practices following NGO reporting), the LATE

estimates become less generalizable. Clearly, additional research needs to be done on this topic, but one should consider these issues as the results of the IV analysis are interpreted.

Replication in High/Low Capacity Municipalities

State Capacity in Guatemala was extremely unevenly distributed. In some sections, such as the capital region, the coastal shipping areas, and the lowlands, the state was strongly present. But in other areas, such as the indigenous highlands, the Guatemalan state only truly emerged during the decade under study.

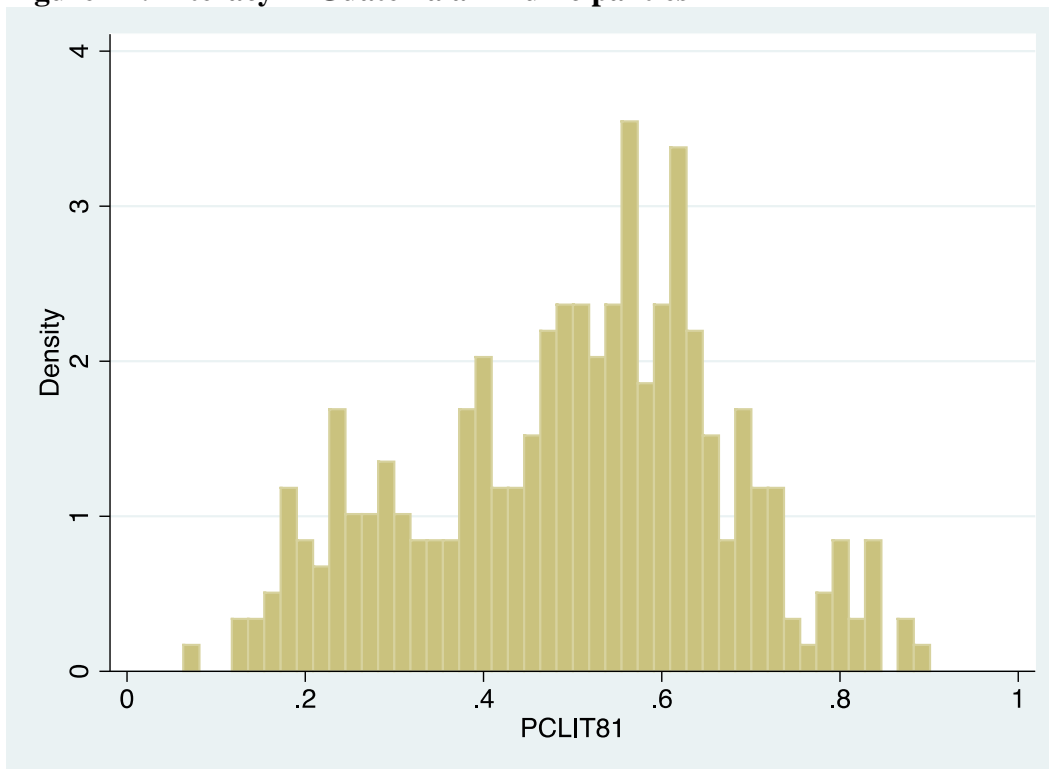**Figure AI: Literacy in Guatemalan Municipalities**

Figure AI presents evidence to this effect. Looking at a key indicator of state presence—the literacy rate of municipalities in 1981—the figure presents a histogram displaying the bi-modal distribution of state capacity. One cluster of municipalities has a

literacy rate centering around 25%, while a larger segment of the country can be represented by the cluster of municipalities with literacy around 60%.

Because repression requires a state presence (and because repression targeting mobilization may require an even greater degree of state capacity), it is important to investigate the generalizability of the argument into cases of both high and low state capacity. Table AIV represents a replication of Models A5 and A6 from Table AIII above. In Models A7 and A8, this replication is conducted exclusively for municipalities with literacy less than the mean (50%). Models A9 and A10 represent this same analysis exclusively for municipalities with literacy rates above the mean.

Table AIV: High/Low Capacity Replication of Instrumental Variables Second-Stage Estimation of Overt Collective Challenges

	A7	A8	A9	A10
	Low Capacity		High Capacity	
Repression of Mobilization	-1.997 (6.567)	-0.082 (0.249)	-0.381*** (0.061)	-0.390*** (0.058)
Repression of Overt Collective Challenges	6.920+ (4.995)	3.404* (1.763)	1.255 (1.207)	1.186 (1.252)
Controls	Y	Y	Y	Y
Annual Fixed Effects		Y		Y
Departmental Fixed Effects		Y		Y
First Differencing the Time Variant Independent and Dependent Variables	Y	Y	Y	Y
N	18,522	18,522	22,428	22,428
Coefficients and standards errors (in parentheses). + p<.10, * p<.05, ** p<.01, ***p<.001 (Two-Tailed Test).				

Instruments: *% of Human Rights Abuses Reported by Human Rights NGOs* (six lags, t-1 – t-6)
Controls: *lagged overt, collective challenges, lagged mobilization, lagged repression of overt, collective challenges, lagged repression of mobilization, spatial-temporal lag of overt, collective challenges, spatial-temporal lag of mobilization, indigenous, literate, population, local NGO presence, regional NGO presence, level of democracy*

The results from the two subsample replications prove substantively interesting. Across all four replications, the point estimates are in the direction predicted by the theory, consistent with the results presented in the earlier analyses. But when the sample is restricted to only municipalities with below average, the significance of the negative impact of repression targeting mobilization disappears. In this replication, repressing overt, collective challenges is still associated with significant increases in challenges, but there is so much noise associated with the estimate of the impact of repression targeting mobilization that it is impossible to distinguish the impact of this variable from zero.

When looking at the subsample of high capacity municipalities, the results are exactly the opposite. In this sample, repressing mobilization is associated with significant reductions in future challenges, while repressing overt collective action does not appear to significantly increase collective challenges.

The results do not critically threaten the theoretical predictions identified in the main text, but they do provide some important clues about context that are useful when thinking about extensions emerging from this work. It appears to be the case that repression directed at mobilization is most consistently associated with decreases in overt collective challenges when mobilization is repressed in sites where the state has the greatest capacity. In such settings repressing overt, collective challenges does not significantly increase collective challenges, but it does not significantly decrease them

either. Conversely, when looking at low capacity municipalities, the repression of mobilization does not appear to have its same significant negative relationship with overt collective challenges. Here, repressing mobilization is not associated with any significant change in challenger behavior. But when overt, collective challenges are repressed in low capacity areas, we see significant positive increases in future challenges.

As noted in the discussion section of the main text measures of state capacity are somewhat endogenous to the success or failure of repression. But the results emerging from this replication suggest that the impact of various repressive campaigns may be contingent on the underlying amount of state capacity operating in an area. Repression is most effective from the perspective of the government when it (a) is targeted at mobilization and (b) occurs in a high capacity area. Repression leads to its greatest increase in challenger behavior when it is (a) targeted at overt, collective challenges and (b) occurs in a low capacity area. Future research will need to investigate how governments make particular decisions about applying repressive force under different structural conditions and in response to different sets of challenger behaviors.

Empirical Comparison to Population-centric Approaches

This section presents a preliminary analysis comparing the organizational-targeting approach to variables derived from population-centric arguments. Because the organizational-targeting approach contrasts with an entire set of population-centric arguments and theories, a full comparison is not possible within this supplementary appendix. However, it is possible within this text to consider some of the more prominent arguments and conduct some initial comparative analyses. Three tests are examined here: The first two assess the theoretical primacy of the organizational-targeting approach by (1) comparing the effects of indiscriminate repression in situations where mobilization was and was not ongoing as well as (2) comparing the effects of organizational-targeting variables in situations where territorial control was strong or weak. The third test considers recent research on the endogeneity of repression/dissent by introducing a control for the leader's expected job insecurity.

Table AV: Empirical Comparison to Population Based Variables

	A11	A12	A13	A14	A15	A16	A17
Repression of Mobilization	-0.129*** (0.034)	-0.135*** (0.011)		-0.128*** (0.011)	-0.134*** (0.013)	-0.329*** (0.015)	-0.110*** (0.011)
Repression of Overt, Collective Challenges	0.237*** (0.034)	0.281*** (0.055)	-0.063 (0.039)	0.237*** (0.035)	0.322*** (0.046)	0.263*** (0.048)	0.219*** (0.032)
Massacres	0.048*** (0.011)	0.059*** (0.018)	0.021 (0.012)	0.121*** (0.013)	0.084*** (0.017)	0.250*** (0.025)	0.137*** (0.012)
Territorial Control				-0.011 (0.014)	-0.089* (0.039)	-0.007 (0.025)	0.002 (0.012)
Leader's Job Insecurity							0.289*** (0.059)
Additional Controls	Y	Y	Y	Y	Y	Y	Y
Annual Fixed Effects	Y	Y	Y	Y	Y	Y	Y
Department-Level Fixed Effects	Y	Y	Y	Y	Y	Y	Y
		Prior Mobilization	No Prior Mobilization		Low Territorial Control	High Territorial Control	
N	35,750	15,394	20,356	35,850	17,875	17,875	35,750

Coefficients and standards errors (in parentheses). * p<.05, ** p<.01, ***p<.001 (Two-Tailed Test). All models incorporate municipal-level random effects as well as department-level and annual fixed effects. Models include controls for lagged measures of

Overt, Collective Challenges and *Mobilization* as well as *Spatial-temporal Lag of Overt, Collective Challenges, Spatial-temporal Lag of Mobilization, Indigenous, % Literate, Population, Democracy* All models include controls for lagged measures of *Overt, Collective Challenges* and *Mobilization.*

Table AV presents the results of the empirical comparison between variables from the organizational-targeting and population-centric arguments. Models A11-A13 consider a key variable derived from research on the effects of repression on popular participation—indiscriminate repression. Numerous works have indicated that indiscriminate repression can lead to backlash against the state.[8] The models replicate Model 3 in the primary text, with the only difference being the inclusion of a measure of *Massacres,* which serves as an indicator for indiscriminate repression. Data for *Massacres* are taken from ANONYMIZED, who compiled a list of massacres identified within Guatemala's *Commission for Historical Memory* (CEH) report. The CEH defined a massacre as an "indiscriminate attack" involving "the execution of five or more people, in the same place, as part of the same operation and whose victims were in an indefensible state."[9] The variable is measured as a lagged count of the number of *Massacres* occurring in a municipality-month and ranges from 0-12.

Results from Model A11 show that the inclusion of this variable into the study's analysis does not affect the core findings regarding repression directed at mobilization or repression targeting overt, collective challenges. As predicted by the population-centric approach, *Massacres* are positively and significantly correlated with subsequent overt, collective challenges. This suggests that incriminate repression is indeed ineffective and motivates civilians to side with challengers. But the inclusion of this variable does not affect the organizational-targeting variables. In this case, *Repression of Mobilization* is remains significantly correlated with a decline in overt, collective challenges, while the *Repression of Overt, Collective Challenges* is significantly correlated with increased

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[8] See Kalyvas and Kocher 2006 for a review
[9] Quoted in Mezquita 2000, p 6

dissent. Comparing the size of the effects, the models predict that the escalatory effect of selectively targeted *Repression of Overt, Collective Challenges* is stronger than that for indiscriminately targeted *Massacres.*

The results from Models A12-A13 replicate this same analysis in two settings—where mobilization was ongoing and where it was not. Whether or not mobilization was ongoing is operationalized as a dichotomous state: mobilization was ongoing where at least one act of mobilization was observed in a municipality within the past six months; mobilization was not ongoing where there were no observed acts of mobilization during this time period. As can be seen in Model A12, in situations where mobilization was ongoing, *Massacres* perform as expected by the population-centric approach. However, in Model A13, where mobilization was not ongoing, *Massacres* fall short of statistical significance (if only marginally). This provides preliminary evidence suggesting that mobilization is necessary for indiscriminate violence to become ineffective. At the same time, the *Repression of Overt, Collective Challenges* variable also loses statistical significance when mobilization is not ongoing. This evidence further supports the importance of mobilization in the production of collective challenges.

Models A14-A16 continue the comparative analysis by considering another variable commonly employed within the population-centric approach—territorial control. The expectation from population-centric arguments is that territorial control facilitates selective repression, which should deter participation in overt, collective challenges (Kalyvas 2006). To measure territorial control, this study employs data from the CIA to identify the sites of permanent military and police headquarters operating during the period under review (Doyle 1999). The measure (*Territorial Control)* replicates Sexton

(2015), who operationalizes control as the average distance a municipality was from neighboring military or police bases. This follows from the logic that the government will be able to exert the greatest amount of control in locations where it has been able to establish a permanent base from which to police the local population and sanction defectors. Model A14 replicates Model A11, with the inclusion of the *Territorial Control* measure. Here, we see that territorial control is negative, but insignificantly associated with overt, collective challenges, which provides weak support for the argument that control facilitates policing and deterrence. *Massacres* remain positively and significantly related to overt, collective challenges, suggesting that controlling for *Territorial Control* should not impact the effect of indiscriminate repression.[10] More importantly for this study, the core organizational-targeting results also remain unaffected. *Repression of Mobilization* remains negative and significantly correlated with subsequent challenges, while *Repression of Overt, Collective Challenges* remains positively and significantly correlated.

Much as Models A12-A13 divided municipality-months based on prior mobilization, Models A15-A16 divide municipalities based on whether they were "Low Territorial Control" (Model A15) or "High Territorial Control" (Model A16) areas. This division is determined by whether the municipality was above or below the median level of *Territorial Control*.[11] The core variables from the organizational model—*Repression of Mobilization* and *Repression of Overt, Collective Challenges*—retain their direction, size, and significance in areas with both high and low levels of territorial control. This

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[10] The inconsistent results for the *Territorial Control* measure could indicate that this is an imperfect proxy for the concept. It must be noted that this concept is notoriously difficult to operationalize (cf., Kalyvas and Kocher 2009, 240-241).

[11] This measure is distributed relatively normally. The mean differs from the median by only 1.5%.

suggests that the impact of these variables is not conditional on the underlying amount control the state exerted in a given territory. Interestingly, *Territorial Control* gains some significance in areas with below average levels of control. This suggests that when compared to the bottom end of the distribution, areas with low to middle levels of *Territorial Control* experienced fewer overt, collective challenges. In the other half of the sample, however, *Territorial Control* no longer exerts a significant effect.

Finally, Model A17 addresses a key variable emerging from new work endogenizing the repression-dissent relationship: *Leader's Job Insecurity.* Existing work predicts that as a leader becomes less secure in office, s/he will become simultaneously more vulnerable to overt, collective challenges and more likely to turn to repression (Young 2013; Ritter 2014). To examine this proposition, the analysis employs data from Young (2013), who extends Cheibub's (1998) methodology for estimating leaders' job insecurity. The indicator is a national-level measure based upon factors including the leader's time in office, the national rate of economic growth, and the country's past executive turnover rates. Looking at the results from Model A17, this variable behaves as predicted. *Leader's Job Insecurity* is indeed correlated with increased rates of *Overt, Collective Challenges.* Critically, however, the inclusion of this variable in the model (like *Territorial Control* before it) does not impact the core results of the analysis.[12] *Repression of Mobilization* remains negatively and significantly correlated with dissent, while *Repression of Overt, Collective Challenges* remains positively and significantly correlated with dissent.

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[12] Because *Leaders Job Insecurity* is measured annually at the national level, it was not possible to split the sample of municipalities in a manner that was not significantly correlated with other national-level characteristics, such as regime type.

Section 3: The AHPN Data

Data from the AHPN are relatively unique. During the period in which they were composed, the authors of the reports had no belief that they would ever be made public. Moreover, they were discovered on accident and released without any form of Freedom of Information process through which sensitive information could be withheld or redacted. But like any data source, there remain potentials for bias. With data from the police, one needs to be conscious of parochial incentives that could lead to biased reporting for professional gains. With regards to the reporting of repression, we should be conscious of bias against the reporting of atrocities. Given the Carter administrations' emphasis on human rights, it is clear that the regime was consciously trying to improve its international human rights reputation by concealing evidence of massacres (Guberek 2012).[13] But with regards to the reporting of social movement behavior, the direction of bias is less clear. Individuals might develop an interest in overestimating the threat of the movement in order to increase their budget or justify their operations.[14] Or they might develop an interest downplaying movement behavior to demonstrate professional success. What is clear is that as a data source, the AHPN provides better detail on a far larger spectrum of political activity than other sources of information on the Guatemala conflict (e.g., Davenport and Ball 2002; ANONYMIZED).

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[13] In part because the police did not carry out these activities and in part because of potential reporting bias, there are few massacres captured in the police data. It is important to keep this in mind when interpreting the results. That said, one of the reasons that indiscriminate forms of state repression, like massacres, are said to have succeeded in Guatemala (Kalyvas 2006; Valentino 2004) is because they were directed at villages identified as sites of insurgent mobilization (Stoll 1993; ANONYMIZED). In many cases, upon learning of an insurgent presence in a village, the army would arrive with an ultimatum: either the village reformed itself into a civil defense unit under direct supervision of the military or the inhabitants would be killed (Kobrak 1996). In other cases, the army simply committed massacres without warning (e.g., Montejo 1995; Falla 1994).

[14] With this in mind, careful attention is paid in the research design to identify seemingly exogenous variation in the independent variable.

Sampling and Coding Procedures

To generate an events database of political activity from 10 million documents recorded in the AHPN, a multi-stage sampling procedure was carried out. First, I conducted a pilot study of the police archive in March 2010. Given the vast trove of documents, it would be impossible to read the full collection (compare Guzman et al. 2009; Price et al. 2009). It was also recognized that of the 10 million, the vast majority contained matters pertaining to criminal, rather than political, investigations (Morales Alvarado 2009). The pilot study was carried out to identify a sampling process that would allow the full study to efficiently identify the most relevant information.

To conduct the pilot study, we engaged in a stratified random sampling process to identify what information was recorded in different portions of the archive.[15] Information in the AHPN is archived based on the organizational structure of the police force. Each file is indexed based on the office that the file was created for or sent to and there exists separate archive locations for each of the different offices of the police ranging from the 10 *cuerpos* that formed operational units of the police force, through the different specialized offices and up to the Director General (chief of police). Records produced by the various divisions of the police and stored in the AHPN cover the full spectrum of police activities. On one end lie the most intensive acts of political repression (such as a directive to capture a list of suspected subversives with their last known whereabouts) to

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[15] The pilot study also engaged in other sampling procedures, including clustered random sampling and truly random sampling. Results proved substantively similar.

the most mundane (such as the hundreds of thousands of orders for officers to appear before the court at a given time and place).[16]

Within this archive structure, the pilot study sampled documents at random from each of the different offices and *cuerpos*. Through this process, it was possible to decipher which office was receiving which type of information and how that information could be accessed. We discovered that more than 95% of the relevant documents (documents containing information on political behavior committed by members of civil society, social movements, political parties, security forces or members of the government) were located in two offices—the Director General's office and the Office for Coordinating Military and Police Activity.

With this knowledge, the next stage in the process was to generate an events database recording information on political activity identified in the records of these two offices. I began the full study in October 2010. Over the next eight months, the full study of the document read each of the more than 300,000 documents indexed by the archive under these two offices. In total, more than a quarter million pages were read encapsulating every file sent to either the Director General's office and the Office for Coordinating Military and Police Activity. From each file, the full study coded all politically relevant events into the database using a coding rubric that included nearly one hundred event types.[17] Each event entered into the dataset as individual rows, with the columns registering the different characteristics coded.

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[16] The reports vary significantly in length, from short missives from the field to lengthy investigations or security plans. Some of the longer documents contained multiple political activities, which enter into the database as separate events.

[17] In our reading of the documents from the police archive, we found that nearly all of the files could be classified as either 'political' or 'criminal/personal' in nature. Events in the latter category, included criminal investigations and procedures as well as personal struggles such as household conflict, were dismissed. For a critical discussion of this distinction see Tilly (2003, 21).

The final coding protocol is detailed below:

Guatemala Coding Protocol
08/10/10

A. Coder
B. Location of Event –

      Street

      City/Town

      Municipality

      Municipality # - Select from XLS file

      Department

C. Date of Event

      Day
      Month
      Year

D. Political Repression
      Select Yes or No based on the following event codes:

Repression was measured using the following codes: 5, 6, 7, 8, 9, 10, 33, 34, 35, 38, 44, 45, 46, 47, 57, 84, 85.

E. Mobilization –
      Select Yes or No based on the following event codes:
Note:
Mobilization was measured using the following codes: 21, 61, 71, 75, 76, 77, 79, 80, 81, 87

F. Overt Collective Challenges
      Select Yes or No based on the following event codes:
Note:

Overt Collective Challenges were measured using the following codes: 5, 6, 7, 8, 9, 12, 16, 19, 23, 25, 55, 57, 58, 78.

Event Types

0- **Unknown**
1- **Other** (*coding to be used sparingly; email Chris if you think you have an example*)
2- **Accident**

Harassment

3- **Verbal Harassment/Written Harassment/Death Threat**
(Perpetrator may be a civilian, a soldier, or a policeman)
4- **Warning** (more benign)

Individual and Group Violence

5- **Indiscriminate violence /Massacre:** Organized violence carried out intentionally and without regard for the individual identities of those killed. Generally involves the violation of multiple individuals simultaneously.
6- **Targeted Attacks/Killings—Political or Military Target**: Organized violence directed at an individual because of actions they took related to their political or organizational affiliations. The violated individual was unable or unlikely to retaliate. (Includes assassinations)
7- **Targeted Attacks/Killings—Sectarian Target**: Organized violence directed at an individual because of actions they took related to their sect. The violated individual was unable or unlikely to retaliate. (Includes assassinations)
8- **Brawl/Clash:** Two groups at protests; tumultuous back and forth (less than a battle); brawls happen in places like lunchrooms, bars, etc
9- **Battle:** Two **organized bodies** engaging in armed combat **Shootout**: Violence between two **disorganized** bodies, or between one **organized** body and one **disorganized** body
10- **Riot/Mob violence/Looting**

Marches, Protest and Political Events

11- **Debate/verbal argument/Non-violent confrontation** (more of a back and forth between parties)
12- **Political March** (usually focused on civil rights issues)
13- **Sectarian March** (focused primarily on the Catholic/Protestant divide)
14- **Vigil**
15- **Speech** - Public or Private

16- **Strike/Picket** (specifically about labor issues)
17- **Public gathering/Meeting**
18- **Ceremony/Symbolic display by Government**
19- **Ceremony/Symbolic display by protestors/dissidents**
20- **Funeral Procession** (usually more of a *preceding event*)
21- **Information distribution**: tabling, press conference, graffiti
22- **Hunger strike**
23- **Protest/Civil Disobedience/Barricades** (organized; e.g., sit-ins, smaller gatherings)
24- **Protest Ban** (political ban)
25- **Counter-protest** (acts in a sequential manner: "This group is protesting—so will we!)
26- **Complaint filing/Seeking legal advice**
27- **National Policy Change**
28- **Local Level Policy Change**: at the neighborhood, city level, etc.
29- **End of Ceasefire**
30- **Ceasefire/Peace talks/negotiations/international negotiations**
31- **Release of hostages/prisoners**
32- **Elections**

Policing

33- **Selective Arrest/Attempted Arrest**
34- **Non-Selective Arrest/Non-Selective Attempted Arrest**
35- **Mass Arrest** (indiscriminate in nature)
36- **Informed/cooperated with Police/Military or Asked to inform/cooperate with Police/Military**
37- **operations con junta con el exercisio**
38- **Police (or Army) Road Block/Checkpoint/Patrol** [a patrol is not defined as harassment]
39- **Police Search**
40- **Police curfew** (usually more of a *preceding event*)
41- **Chase/Police Chase**
42- **State Surveillance**
43- **Police Abuse during detention/arrest**
44- **Protest Policing/Riot policing- No Live Rounds** (the policing in question must be within the bounds of legal protest/riot police work, even if distasteful)
45- **Protest Policing—Live Rounds**
46- **Other forms of state repression** (*coding to be used sparingly*)

Paramilitary violence

47- **Intra-communal violence/social control**: Paramilitary groups imposing sanctions on their own communities for behavior deemed

anti-social. The difference between internal policing and social control is that internal policing has to do with the politics internal to an organization, while social control has to do with sanctioning an individual for allegedly violating social norms

48- **Turf war/Territorial dispute**
49- **Gang violence**
50- **Ethnic derogation/Ethnic violence**
51- **Intra-organizational violence/internal policing**

Other Forms of Violence

52- **Torture, mental or physical**
53- **Beating** (seen as 1 guy being attacked by multiple assailants; either civilian or police)
54- **Suicide/Attempted Suicide**
55- **Hijacking/Kidnapping**
56- **Rape/Sexual Assault/Sexual Harassment**
57- **Raid/Siege**: can be carried out by police, but does not have to be (raid and siege are distinguished by differing levels of violence)
58- **Robbery**
59- **Provocation by Victim**
60- **Provocation by Perpetrator**
61- **Arms Purchased by Insurgent, or Insurgent Friends/Kin**
62- **Arms Purchase by State, or State Friends/Kin**
63- **Victim Taken to Hospital/Doctor**
64- **Public Sympathy for Victim**
65- **Ethnic Migration**
66- **Public Sympathy of Perpetrator**
67- **Forcibly Evicted**

Miscellaneous

68- **Non-Violent Trauma**
69- I**nitiate an investigation**
70- **secure a perimeter**
71- **combat training**
72- **inform superiors**
73- **legal procedures/trial**
74- **solicited international actions**
75- **"non-violent mobilization" activities designed to encourage people to participate in political protest**
76- **Violent mobilization**
77- **Formation of an armed group**
78- **Terrorism**

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