# The Coercive Weight of the Past: Temporal Dependence and the Conflict-Repression Nexus in the Northern Ireland "Troubles"

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# The Coercive Weight of the Past: Temporal Dependence and the Conflict-Repression Nexus in the Northern Ireland "Troubles"

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*After 40 years, we still know very little about how state repression influences political dissent. In fact, to date, every possible relationship, including no influence, has been found. We argue that part of the problem concerns the current practice of treating every repressive event as if it were substantively equivalent, differentiated only by scope (large/small) or type (violent/nonviolent). We advance existing work by arguing that the influence of repression is contingent on when it occurs within the temporal sequences of political conflict. Using new events data on the "Troubles" in Northern Ireland from 1968 to 1974, results show that when dissent has been decreasing in the recent past, repressive action inspires an increase in dissident action. When dissent has been increasing, however, repression has the opposite effect, decreasing challenging activity. These results provide important insights into resolving a recurrent puzzle within the conflict-repression nexus as well as understanding the interaction between government and dissident behavior.*

*KEYWORDS   civil war, conflict processes, human rights, Northern Ireland, repression*

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Researchers have been rigorously studying political conflict and violence for over four decades. The findings of this work have been significant as well as robust, setting the parameters of this research agenda. For example, we have come to understand the generally pacific nature of political democracy and economic development (for example, Davenport 1995; 2007; Hibbs 1973; Muller 1985; Poe and Tate 1994), the generally hostile nature of violent state-societal confrontations (for example, Gurr 1970; Moore 1998; 2000) and the escalatory influences of inequitable economic relations (for example, Aflatooni and Allen 1991; Carleton 1989; Robinson and London 1991) and restrictive economic trading practices (for example, Hafner-Burton 2005; Abouharb and Cingranelli 2007; Franklin 1997).

Despite the large amount of scholarship, however, numerous puzzles remain. For example, within an area referred to as the "conflict-repression nexus" (for example, Lichbach 1987) it has been found that repressive actions do not have consistent effects on political challenges across empirical examinations. Indeed, repressive events have been shown to increase dissent (for example, Francisco 1995; Kocher et al. 2011), decrease dissent (for example, Lyall 2009, White 1993), generate competing effects (for example, Moore 1998; Rasler 1996), or produce no effects whatsoever (for example, Gurr and Moore 1997). These findings are problematic for they suggest that we do not really know much about the effectiveness of government coercive behavior on influencing behavioral challenges—a mainstay of counterinsurgent, counterterrorist, and protest policing policy as well as diverse theories of the state.

We contend that the inconsistency in these results occurs, at least in part, because of an inattention to temporal sequencing in the analysis of political conflict (that is, the location of contentious events within a larger chronological series). From this perspective, prior trends in political conflict are likely to influence both how much dissent we observe and how repression influences ensuing dissident behavior afterwards. For example, we anticipate that repressive action will have a different impact on behavioral challenges if prior dissident behavior (before the government's action) was increasing or decreasing.

Using original events data on Northern Ireland's Troubles collected from media sources, NGOs, and human rights groups (the Northern Ireland Research Initiative [NIRI]), we empirically investigate the role of temporal sequencing in shaping the impact of repressive behavior on dissent. Results show that when dissidents are repressed following a period of decreasing dissent (that is, dissident activity was becoming less frequent), a "backlash" occurs where dissidents respond by increasing the number of subsequent acts of dissent. When repression is applied in response to increasing levels of dissent, dissidents respond in the opposite direction with "retreat." In this sequence, dissidents decrease the amount of subsequent dissent perpetrated in the aim of limiting their exposure to repressive actions.

In addition to demonstrating how scholars might pay greater attention to the temporal sequencing of repression and dissent, this study reveals

the potential contributions of temporal disaggregation within events data collection. There has been much discussion recently about the severity of data aggregation problems in the study of political repression and dissent (for example, Kalyvas 2006). Existing disaggregation efforts, however have focused more on analyzing political behavior across *space* than across *time* (for example, Cederman et al. 2009). As a result, scholars have done well to link spatially referenced variables on political conflict, but have been less prepared to examine the sequence of action-reaction choices made by governments and dissidents that generate various outcomes.

The rest of the article proceeds as follows: first, we review how temporal sequencing can influence both subsequent dissident behavior and the effects of political repression. We then review new events data generated by the Northern Ireland Research Initiative and discuss the identification strategy used in the study. Subsequently, we present the analysis and conclude with some general comments about the study of repression and dissent.

## CONTEXT VERSUS CONTEMPORANEOUS EFFECTS

The common practice among those analyzing the effects of political repression on behavioral challenges (in our case dissent) is to compare challenging behavior following government coercive action to base-level median rates of the number of challenging events. The argument guiding such a design and the expectations for observing the potential effect are relatively straightforward. Repression is believed to have led to political dissent when (ceteris paribus) observations of challenging behavior following repressive action are significantly higher than the sample average (presumably because of the anger and desire for revenge that the government' coercive action inspires among those targeted (for example, for theoretical explanation see Gurr 1970; for empirical example see Francisco 1995). In contrast, repression is believed to diminish dissent when observations of dissident behavior following repressive action are significantly lower than the sample average (presumably because of the fear and desire for survival/self-preservation that the government's behavior inspires (for example, for theoretical explanation see Tilly 1978; for empirical example see White 1993).

While useful in guiding research about the influence of repressive behavior on dissent for the last several decades, a principal limitation with this approach is that it does not address behavior that took place prior to the application of political repression (other than to use this behavior as a data point when calculating the sample median, or in some minimal control function, as discussed below). In this article, we argue that temporal sequencing of dissent and repression is significant for two reasons: one theoretical and one methodological.

Theoretically, changes in the rates of dissent occurring prior to the application of political repression may influence subsequent rates of dissent by

the repressed actor (that is, dissidents) directly through its impact on the emotional state of the target—the key mechanism within almost all studies of the topic. For example, the internal dynamics operating within a dissident group that inspired an increase in dissident activity prior to political repression may also lead to increases following the government's action. Attributing rates of dissident activity solely to the effects of repressive behavior following increased escalatory trends can inspire flawed conclusions because in all probability those rates would have been higher regardless of the government's activity. In this example, escalatory trends may lead subsequent dissent to increase with or without intervention by political authorities.

Methodologically, if we know that prior trends in dissent influence the application of repression (for example, Davenport 1995) as well as subsequent levels of dissent, then prior trends in dissent influence both the independent and dependent variables in existing work. Failing to account for prior trends in challenging behavior could result in omitted variable bias, which can produce spurious correlations and prejudice the results. These issues are rarely addressed except when individuals attempt to de-trend data through the incorporation of a lagged dependent variable or examination of an auto-regressive/duration analysis (for example, Davenport 1996; Derouen and Bercovich 2008). This is different from understanding the cause of individual temporal patterns, however, and does not address further complications arising from pre-intervention trends in dissent.

Taking temporal sequencing seriously requires both more nuanced theorizing about how repression influences dissent in light of prior behavior as well as more sophisticated modeling of the ways in which time trends impact both independent as well as dependent variables. Theoretically, the current research helps specify particular sequences or cycles in the interaction between dissidents and states as well as how behavioral trends in one particular direction at one particular time are likely to influence subsequent behavior. Our modeling strategy incorporates this contention directly into the analysis and allows temporal effects to fluctuate depending on the prior behaviors of dissidents.

## Rethinking Dissident Responses

From existing research, it is relatively clear that decisions to challenge political authority are not undertaken lightly or with frequent success. Indeed, there are a great many obstacles to collective action (for example, Lichbach 1998; Olson 1965). What is important about this work for understanding the impact of repressive behavior is that once a successful resolution to the collective action problem has been achieved and mobilization takes place (that is, it is underway) it is not likely to be easily overturned (for example, McAdam 1986). In short, mobilization occurs through cascades in which the

prior actions of individuals who contribute to or defect from social movements influence the subsequent willingness of others to take similar actions (that is, DeNardo 1985; Kuran 1989; Tarrow 1998). With this in mind, it becomes crucial to identify/monitor trends in dissident activity for current manifestations are likely to be contingent on the trajectory of recent behavior.

For example, accounting for temporal sequencing can influence the *salience* of the estimated causal effects of external influences, such as state repression. If periods of significant dissident activity motivate the application of repressive action and simultaneously predict a higher level of action by dissidents subsequently, then failing to include prior dissent in the model can generate a substantially overinflated positive effect of repression on dissent. In this case, the external actions of the authorities are not what leads to dissent because the subsequent actions of the dissidents are not being driven by repressive behavior but instead by the internal dynamics of the movement. Traditionally, this has been when "conflict cycles" are expanding. Similarly, reduced effects would be observed when repression follows periods of lower dissident activity where this prior pattern of dissident behavior also predicts lower rates of subsequent dissent. In this case, when conflict cycles are contracting, failing to include prior trends in dissent would generate estimates of causal effects of repressive action that are substantially overinflated, but negative. In both scenarios, we can expect that including chronological sequencing into the model will reduce the size of the estimated effect of repressive action and that through this proper contextualization we can better ascertain the impact of repression on dissent. This leads to our first hypothesis:

H1: *The effects of repression on dissent will be significantly smaller when we control for recent trends in dissident behavior.*

In addition, recognizing that the trends occurring in the recent past are likely to influence how dissidents behave in the future allows for more nuanced theorizing about the effects of repression on ensuing dissent. Accounting for temporal sequencing can also influence the *direction* of the estimated causal effects. Here, we theoretically move away from the existing focus on the isolated effects of repressive action on either the challenger's anger or fear and instead attempt to situate these emotional states in the chronological context within which they take place. This is crucial because we would argue that repressive action is not something that simply happens to challengers, rather events like repression are interpreted by dissident groups as they happen in light of the sequences of activity that have recently occurred.

Because challengers are expected to react to repression in light of past behavior, then repressive action may have conflicting effects depending on how dissidents were acting in the recent past. In particular, the

path of recent trends in dissident mobilization is likely to play an important role in directing dissident responses to government coercion. In line with existing work, if repression is conceived of and interpreted as a "negative sanction," then the influences of repression on subsequent dissident behavior are likely contingent on the types of behavior movement leaders believe inspired the state to sanction the movement as well as how movement leaders believe they can successfully avoid future repressive actions (Lichbach 1987; Moore 1998). For example, if challengers have been recently failing to mobilize (that is, they had decreasing levels of dissent in the past), when repressive behavior takes place, then they would most likely frame repressive action as an illegitimate sanction of dissident behavior. Here, anger would be cultivated and a backlash of mobilization would ensue because movements would find new incentives to challenge the state that had sanctioned them (Francisco 2004). This yields the following hypothesis:

H2: *When past dissent has been decreasing, repression is likely to increase subsequent dissent.*

In contrast, when challengers have developed some successful resolution to the collective action problem and have been able to effectively mobilize in the recent past (that is, they had increasing levels of dissent in the previous period), it is possible that when government coercive action takes place it would reverse the mobilization trends and decrease subsequent dissent. Here, fear would be cultivated and mobilization would decrease because dissidents would be sensitive about losing what they had gained in terms of momentum. In this case,

H3: *When past dissent has been increasing, repression is likely to decrease subsequent dissent.*

## DATA AND METHODOLOGICAL APPROACH

For this analysis, we use data collected by the Northern Ireland Research Initiative (NIRI; Loyle, Sullivan, and Davenport 2011). Initiated in 2007, NIRI is a collection of data on the Troubles in Northern Ireland from 1968 through 1998 designed to capture the patterns of contention that took place at multiple levels. Using numerous data sources, the goal is to identify and catalogue all events from all actors that took place in Northern Ireland during the relevant temporal period. NIRI employs a number of sources: (1) records from human rights NGOs, (2) interviews with survivors, (3) files generated by the military as well as police, (4) media reports, and (5) existing conflict data archives.

The Troubles in Northern Ireland were a multiparty conflict, involving pro-government, Protestant paramilitary organizations, the Irish Republican Army (IRA), and the military as well as the police of the United Kingdom. The analyses for this study focuses exclusively on the dyadic interactions of the British government and the Irish Republican Army.[1] Because the NIRI data collection is ongoing, the findings presented in this article represent data from only a subsection of the NIRI archive from the years 1968–1974.[2] By most accounts, this is the period of greatest contestation during the conflict (Sutton 1994). Additionally, we focus exclusively on violent action taken by the different actors involved.[3] For this subsection analysis, we use data from four sources: (1) a new coding of *Lost Lives* (McKittrick, Ketters, Feeney, and Thornton 1999) presenting an events-based description of all individuals killed over the course of the Troubles, (2) a record of human rights violations coded from witness statements collected by the Associates for Legal Justice, (3) coding of a community cataloging of conflict-related activities, *Ardoyne: The Untold Truth* (Ardoyne Commemoration Project 2002), and (4) a new coding of Deutsch and Magowan's (1975) media-based chronology of Northern Ireland events.[4] These sources were brought together, de-duplicated (that is, all redundant entries were eliminated) and arrayed into a unique time-series of political events by the region-month. Table 1 identifies the number of events by actor and data source.

**TABLE 1** Recorded Incidents by Source and Actor

| Source | Actor | | |
| --- | --- | --- | --- |
| | IRA | British State | Total |
| Lost Lives | 606 | 178 | 784 |
| Deutsch and Magowan | 292 | 422 | 714 |
| Associates for Legal Justice | 0 | 473 | 473 |
| Ardoyne Data Project | 13 | 25 | 39 |
| Total | 911 | 1,098 | 2,010 |

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[1]Dyadic analysis is common in the literature on political violence (for example, Cederman et al. 2009; Cunningham et al. 2009; Kalyvas 2006; Moore 1998). Within subsequent work, we will break these categories down further.

[2]Events omitted from this study are those that fall outside the temporal boundaries of our analysis, nonviolent events, events perpetrated by actors other than the British government or the IRA, and events for which either the location or perpetrator could not be identified. This leaves a subset of 2,010 events identified in Table 1.

[3]While it is possible with the NIRI data to conduct analysis on both violent and nonviolent events, for the purpose of this article we theorize the effects of violent repression on subsequent acts of violent dissent. Future extensions could use the NIRI data to test substitution arguments (for example, Lichbach 1987; Moore 1998, 2000).

[4]Additional information on the sources coded can be found at http://web.me.com/christiandavenport/Site_50/The_Northern_Ireland_Research_Initiative,_1968-1998.html
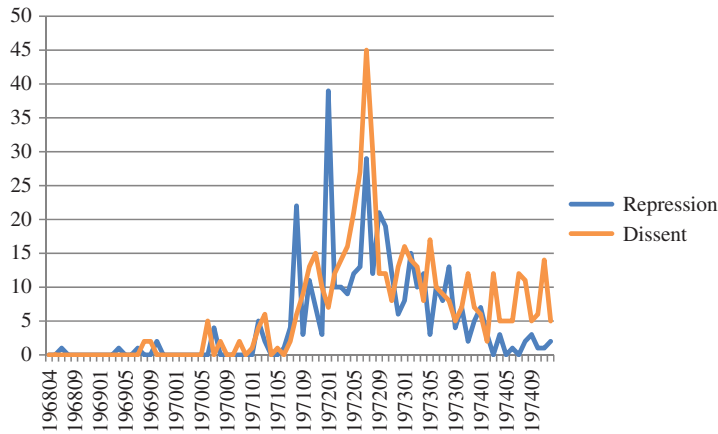
**FIGURE 1** Acts of repression and dissent by month, 1968–1975 (color figure available online).

To analyze this data, we compress each event to the region and month in which it occurred. Doing so creates units of analysis that may be compared to one another using cross-sectional time-series analysis.[5] In particular, five regions are analyzed—North Belfast, West Belfast, East Belfast, Londonderry, and Armagh. These units were selected because they have comparable populations and experienced comparable levels of violence during the Troubles (Sutton 1994).[6] Activities measured as state repression include event types such as beatings, torture during detention and targeted military attacks. Activities identified as dissent primarily consist of indiscriminate terrorist attacks, targeted military killings and shootouts. Each event is coded for the actor who perpetrated it as well as the type of activity, its intended target, and the number of resulting victims. As one can see in Figure 1, repression and dissent varied significantly over the period of interest.

## Identification Strategy

To control for temporal sequencing in political conflict and to better identify the influence of political repression on dissent, we utilize three related approaches. Combining these approaches mitigates many of the problems resulting from inattention to temporal sequencing and helps generate estimates of causal effects that are less biased by the selection effects and omitted variable biases identified above.[7]

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[5]Compared to more aggregated studies, data disaggregated to the month allow for better modeling of the underlying temporal processes and reduce the risks of spurious correlations (Freeman 1989).

[6]By comparing units with similar experiences with violence and similar populations, we can be certain that conclusions based on comparisons between treated and untreated units are reasonably reliable.

[7]We note that we see these approaches not as *the* means for dealing with temporal sequencing in events models, but instead as *one* step along a research path that will lead to a better understanding of how

The first step taken in our analyses is to include a control variable in our model measuring the differential rate of change in the dependent variable (dissent) in a given region over the previous months. This measure (*Prior Trends in Dissent)* helps control for the internal mobilization dynamics occurring within dissident groups prior to the intervention by the state. The analyses are each replicated three times, using three different metrics for defining *Prior Trends in Dissent* (that is, one month earlier, two months earlier, and three months earlier). For each cut point, we measure the rate of change in dissident actions from the beginning of the time period to the end. These three time periods were chosen in order to test the sensitivity of the findings to the choice of cut point. These were also selected because they were near enough to the events of interest to produce reliable estimates of how prior trends influence subsequent dissent without generating too much of a time lag, which might allow for intervening variables to bias the results. See discussion of time lags in Lyall (2009).

The second step taken to address temporal dynamics, as well as control for any potential selection effects visible in the treatment, is to model the effects of repression and dissent through a series of difference-in-difference (hereafter DD) equations (Angrist and Pischkey 2009). DD equations control for a number of unobservable threats to causal inference. Three controls are noteworthy. The first is the *Prior Trends in Dissent* variable identified above. Second, the models include temporal fixed effects to control for temporal shocks common across both repressed and unrepressed units. This control helps guard against the possibility that temporal fluctuations in dissent that are disassociated from repression (for example, seasonal changes) might be incorrectly attributed to the independent variable. Third, the models include a control for whether a unit is treated (*Treated Unit Effect)*—a dichotomous control measuring whether a unit had either been treated in the previous X months or would be treated in the next X months. This is done to control for any potentially unobservable differences between treated units that might exist prior to treatment. With these controls in mind, the models estimate the effects of repression in the X months following repressive action within treated units, which is referred to as the average treatment effect (*ATE*).

Each of the DD models is replicated three times using the three different temporal reference points discussed above. For each temporal reference point, the X months used to define the *ATE* and the *Treated Unit Effect* are based on the relevant temporal metric for that equation (that is, for the "one month" models the *Treated Unit Effect* is one month before and after, for the "three months" models it is three months before and after).

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temporal sequencing influences the occurrence of repression and dissent as well as their interactive effects. Similarly, our approach should not be viewed as providing a definitive end to questions about how temporal sequencing influences the effects of repression, but instead as evidence that temporal sequencing needs to be taken seriously. Following this path, scholars may begin to further unpack the many complicated ways in which temporal sequencing can influence our understanding of the conflict-repression nexus.

Finally, our models take one additional step to control for chronological context. Specifically, we divide the sample and analyze separately those units where dissent was increasing or decreasing over the previous months. This step is taken to evaluate predictions regarding how the effects of repressive behavior on dissent diverge contingent on the prior behavior of the dissidents. Using the measure of prior trends in dissent identified earlier, the sample is divided into two. One of the subsamples includes all region-months when prior trends in dissent are positive. The other includes all region-months when prior trends in dissent are negative.[8] Both subsamples are analyzed using the DD approach, including the *Prior Trends in Dissent* control variable.

## EMPIRICAL ANALYSIS

Table 2 displays the results of the initial empirical analyses conducted for this study. The first model in Table 2 reproduces techniques common in the

**TABLE 2** The Influence of Political Repression on Subsequent Dissident Activity

| | Full Sample | | | |
| | | Set I | | |
| Temporal Trend | Model 1 OLS None | Model 2 DD 1 Month | Model 3 DD 2 Months | Model 4 DD 3 Months |
|---|---|---|---|---|
| ATE | | 0.845 | 0.670 | 0.220 |
| | | (0.337)* | (0.365) | (0.389) |
| Repression at time $t$ | 1.603 | | | |
| | (0.297)*** | | | |
| Dissent at time $t$ | 0.385 | 0.178 | 0.067 | 0.119 |
| | (0.046)*** | (0.042)*** | (0.057) | (0.073) |
| Treated unit effect | | 0.326 | 0.603 | 0.589 |
| | | (0.307) | (0.365) | (0.411) |
| Prior trends in dissent | | 0.497 | 0.168 | 0.045 |
| | | (0.032)*** | (0.024)*** | (0.016)** |
| Constant | 0.482 | −0.019 | 0.332 | −0.000 |
| | (0.118)*** | (0.631) | (0.753) | (0.816) |
| $N$ | 390 | 380 | 380 | 380 |
| $R^2$ | 0.33 | 0.74 | 0.59 | 0.56 |

*Note.* OLS Model includes spatial-fixed effects. DD Models include spatial and temporal fixed-effects. Fixed-effects omitted for presentation purposes. Huber-White Robust Standard Errors in parentheses.
*$p < .05$, **$p < .01$, ***$p < .001$ (two-tailed test).

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[8]Region-months where prior trends in dissent were flat are not included in the analysis.

study of repression and dissent.[9] Model 1 estimates the effects of repression (*Repression at Time t*) on subsequent dissent occurring the next month (*t+1*). Reproducing best practices in the literature, the model does takes one step to condition the estimates on how much dissent has occurred previously—including a measure of dissident behavior (*Dissent at Time t*) as a control variable. Beyond this limited control, however, the model estimates how political repression influences subsequent dissident activity without considering temporal trends in political conflict.

From Model 1 in Table 2 it appears as though repression positively influences subsequent dissident activity. Engaging in politically repressive action in a given month increases the number of acts of dissent subsequently committed in the same locality by between one and two events. This represents a substantial increase as the IRA committed an average of slightly more than one event per month in each of the localities reviewed over the period of analysis.

But as we noted above, there are reasons to question estimates that fail to take seriously the effects of recent trends in dissent on subsequent dissident behavior. Models 2 through 10 take these trends into account by estimating DD equations that are conditioned on prior activity. As discussed above, three sets of models are estimated. We begin by generating DD estimates across the full sample of region-months (Table 2). We then separate out locality months where dissent has been increasing over the recent past from those where it has been decreasing (Table 3).[10] As hypothesized, we anticipate political repression to have muted effects over the full sample of region-months, and differing effects depending on how dissidents were acting prior to repressive action. We turn to a discussion of our results below.

## FINDINGS 1: FULL SAMPLE ANALYSIS

In Set I of Table 2, we evaluate the effects of repression across the full sample. We control for *Prior Trends in Dissent*, but do not divide the sample based on how dissidents were behaving prior to repressive action. From the first set of models in Table 2 we see that prior trends in dissident activity significantly impact subsequent dissent. Across all three of the metrics used to

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[9]Two differences between this model and the equations most commonly used in the literature should be noted. First, this model estimates the effects of repression at time t on dissent at t+1, instead of repression at time t–1 on dissent at time t. Second, this model employs an OLS design that does not control for the count nature of the dependent variables. Both of these changes are done to make the model more comparable to the subsequently estimated DD models. Replication using a lagged repression variable and a negative-binomial framework revealed that neither significantly impacted the results.

[10]Again, the "recent past" varies between one month and three months. Each model defines it using the same time metric used to define the DD structure and the recent trend variable.

**TABLE 3** The Influence of Political Repression on Subsequent Dissident Activity in Varying Chronological Contexts

| Temporal Trend | When dissent is decreasing | | | When dissent is increasing | | |
|---|---|---|---|---|---|---|
| | Set II | | | Set III | | |
| | Model 5 DD 1 Month | Model 6 DD 2 Months | Model 7 DD 3 Months | Model 8 DD 1 Month | Model 9 DD 2 Months | Model 10 DD 3 Months |
| ATE | 0.834 | 3.066 | 0.006 | 0.747 | 0.426 | −0.997 |
| | (0.269)** | (0.241)*** | (0.009) | (0.415) | (0.388) | (0.492)* |
| Dissent at time $t$ | 0.339 | 0.086 | −1.162 | 0.598 | 0.416 | 0.564 |
| | (0.035)*** | (0.063) | (0.042) | (0.059)*** | (0.125)** | (0.093)*** |
| Treated unit effect | 0.119 | −2.983 | 0.020 | −0.015 | 1.329 | 1.777 |
| | (0.239) | (0.245)*** | (0.086) | (0.375) | (0.366)*** | (456)*** |
| Prior trends in dissent | 0.856 | 0.666 | 0.925 | 0.360 | 0.190 | 0.184 |
| | (0.033)*** | (0.101) | (0.027)*** | (0.041)*** | (0.030)*** | (0.029)*** |
| Constant | −0.844 | −0.086 | 7.643 | 0.309 | −0.309 | 0.142 |
| | (0.673) | (0.114) | (0.209) | (0.866) | (0.987) | (0.963) |
| $N$ | 246 | 137 | 114 | 269 | 239 | 185 |
| $R^2$ | 0.95 | 0.96 | 0.97 | 0.75 | 0.64 | 0.71 |

*Note.* OLS Model includes spatial-fixed effects. DD Models include spatial and temporal fixed-effects. Fixed-effects omitted for presentation purposes. Huber-White Robust Standard Errors in parentheses.
$^*p < .05$, $^{**}p < .01$, $^{***}p < .001$ (two-tailed test).

define *Prior Trends in Dissent* (1 month, 2 months, and 3 months), this variable is positive and statistically significantly related to subsequent dissident activity. This result demonstrates that prior trends are significant in shaping subsequent dissident activity. Looking at the results of Model 2, if dissent has increased by two acts over the previous month, then dissidents are predicted to perpetrate one additional act of dissent in the next month. Similarly, if dissent has been decreasing over the previous month, Model 2 predicts lower rates of dissent the following month. The predicted increases or decreases in subsequent dissident activity have nothing to do with the external effects of repressive action. Rather, they are determined by the internal dynamics of mobilization that occur as dissidents respond to monthly fluctuations in their own behavior.

The first set of models further show how controlling for trends in dissent substantially alter the results derived from existing analyses. Across the first set of models, the previously identified substantive effects of political repression on dissent are dramatically reduced. The effects of repressive action on subsequent dissent (identified as the *ATE* in Models 2 through 4 and *Repression at Time t* in Model 1) are significantly smaller than previously identified. This is consistent with Hypothesis 1. Where previously government coercion was estimated to increase subsequent dissent by more than

1.5 events in the next month, the effect is now reduced to about half of that amount (Model 2).[11]

Addressing the statistical significance of our estimates, the estimate of the effects of political repression on subsequent dissent are not quite indistinguishable from zero in Model 2, though it does approach the .05 $p$ value conventionally used to identify such a null effect. When 2 or 3 months are used to identify the prior trends in political conflict in Models 3 and 4, we observe statistically insignificant results. This too is consistent with the muted effects of repressive action on subsequent dissent that were predicted above. As we suggested, once we integrate temporal sequencing into our models many of the previously observed effects are reduced or become insignificant, increasingly so as one considers longer periods of time.

## FINDINGS 2: SUBSAMPLE ANALYSES

Having addressed the effects of prior trends and repressive action on subsequent dissent across the full sample, we now turn to the subsamples estimating levels of dissent when dissident behavior has been increasing or decreasing in recent months. This empirical analysis aims to evaluate our directional hypotheses (H2 and H3) as well as the general prediction that the direction of prior trends in dissent will significantly impact how repression influences challengers' subsequent behavior (H1).

Across the models in Table 3 there is evidence to confirm the contention that engaging in political repression will reverse the trends in dissident behavior. Where dissent had been decreasing, engaging in repression is shown to lead to an increase in subsequent dissent. Engaging in political repression where dissent has been de-escalating in recent months is predicted to inspire up to six acts of dissent over the next 2 months (Model 6). Where dissent had been increasing, repression is shown to reduce subsequent dissident activity. Engaging in political repression when dissent has been decreasing over the previous 3 months leads to one fewer act of dissent in that locality in each of the next 3 months (Model 10).

These results indicate that political repression functions as a sanction and reverses trends in dissident activity. We see that when dissent has been decreasing, engaging in repression escalates subsequent dissident activity. When dissent has been increasing, engaging in repression diminishes subsequent dissent. This result should be taken as only tentative evidence, however. The significance of the result does not hold consistently across all models, and the time periods vary. The positive effect of repression

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[11]The models were also replicated including a control for the number of violent repressive actions committed at time t. This did not substantively alter the findings.

on dissent when dissent had been decreasing appears only when we measure subsequent dissent over the most immediate time period (1 month and 2 months). The negative effects of repression when dissent had been increasing are only apparent when we measure subsequent dissent over a longer time frame (3 months). This suggests that future research is necessary to further investigate the interactive effects of prior dissident activity and political repression. Returning to theory, these results also suggest that it might be lucrative to explore how challenger emotions might be experienced across distinct time horizons; anger might be an emotion that is more likely manifest in the short-term whereas fear might be an emotion that is more relevant to long-term dynamics.

## CONCLUSION

Our research set out to shed some light on perhaps one of the most enduring puzzles within the research on repression and dissent: how does the former influence the latter as well as why have researchers found almost every type of impact. Within this article, we argued that the key to understanding these issues lay in including a more rigorous consideration of temporal dynamics and using new data on conflict as well as repression during the Troubles of Northern Ireland from 1968–1974 (from the Northern Ireland Research Initiative), we set out to examine diverse influences. The analysis proved to be informative and directs scholarship in a new direction.

Cumulatively, the analyses show that repression leads to increased dissent but only in very specific circumstances. The effects of government coercion are shown to be highly contingent on how dissidents were acting prior to the intervention of repressive action. When dissent had been decreasing, engaging in repression is found to increase subsequent dissident activity (backlash). Conversely, when dissent has been increasing in the recent past, we see either no effect or a slight negative effect of repression on challenging behavior (retreat). In both cases, repression leads to a reversal in the trend of dissident activity.

Taking these results seriously, future research needs to pay greater attention to the role of prior dissident actions and its influence on the relationship between state repression and subsequent dissent. As conceived of in this research, conflict emerges out of the dyadic interaction between governments and challengers. Understanding when, where and how each of these two actors responds to one another requires delving deeper into their interaction to understand how actions taken at one stage in the sequence shape subsequent actions by both sides. Of course, this having been said, future studies of the conflict-repression nexus should move beyond the case of Northern Ireland and test the efficacy of our findings across other conflicts.

There are further implications of our work for both dissidents and governments. On the one hand, challengers seeking to inspire greater mobilization should recognize that this process takes time and build on their own mobilization. Actions taken today play a dramatic role in shaping tomorrow's actions. More attention should be directed inwards if dissidents are to understand how mobilization (or demobilization) unfolds. On the other hand, political authorities should understand that despite their intentions to wield repression to suppress dissent, it does not always have this effect. The use of repressive action in settings where dissent has been decreasing can inspire backlash that increases the amount of dissent directed at the state. Things could also go in the opposite direction when repressive action is applied amidst increasing dissident behavior but this is more variable.

Finally, our findings point to the necessity of including more disaggregated events data in the study of the conflict-repression nexus. Data which includes the timing and temporal sequence of conflict events is essential for understanding state and dissident interactions. As such more data collection efforts should focus on compiling information to be used towards this end. Indeed, it is our intent with this study to inspire more consistent consideration of temporal dynamics along with the growing interest in space.

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