

The Dynamics of Repression and Insurgent Practice in the Black Liberation Struggle¹

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When authorities repress insurgents, does it quell their rebellion? Classic approaches to this question yielded inconsistent results because they sought to generalize repressive effects without accounting for the practices repressed. This article proposes that insurgent practices that draw allied support in a given historic context evade the de-escalatory capacity of repression. The article assesses the effects of repression on odds of remobilization for various forms of Black insurgent practice in the post-war United States, comparing the impact on civil rights protests and urban uprisings between 1954 and 1992, before checking final models on out-of-sample data. Generalizing the dynamics of practice better accounts for the evidence, transcending invariant models to explain social process without devolving to ideographic analysis.

When people rebel against established institutions, authorities almost always take repressive action against them (Davenport 2007). But does repressive action succeed in quelling their rebellion? Scholars have long debated how repression affects mobilization (Gurr 1968, 1969; Jenkins and Perrow 1977; Tilly

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1978; McAdam 1982; Lichbach 1987; Earl 2003, 2006; Davenport, Johnston, and Mueller 2004; Davenport 2010, 2015). The results of empirical studies range widely and appear contradictory. Repression sometimes works and sometimes is escalatory—defying all prevailing theories (Davenport 2007; Earl 2011).

The influential “inverse-U” theory of repressive effects (Gurr 1968, 1969; Muller 1985) attempted to account for the range of repressive effects with reference to the intensity of repression. But inverse U assumes that the effects of categories of repression on mobilization are similar across historical situations. Thinking more processually, political process theorists sought to situate repressive effects as dependent on broader political dynamics. The core political opportunity thesis holds that the effect of repression on mobilization depends on structural openings that increase the political leverage of a social group, making repressive action against it less effective and more escalatory (McAdam 1982, esp. pp. 43, 174–79). But as a generation of nuanced empirical scholarship—largely inspired by political process theory—has shown, political process theory does not go far enough. Smaller-scale historical dynamics defy the explanatory capacity of the structural assumptions that undergird the classic opportunity thesis (for overviews, see Gamson and Meyer 1996; McAdam, Tarrow, and Tilly 2001; Goodwin and Jasper 2004, 2012; Armstrong and Bernstein 2008; Bloom 2015).

In recent years, qualitative case studies have pioneered explanations of mobilization with reference to the efficacy of specific *practices* in a given political context (e.g., Amenta 2006; Evans and Kay 2008; Taylor et al. 2009; Jansen 2011, 2016, 2017; Bloom 2015; Bloom and Martín 2016; Gastón 2017; Kay and Evans 2018). Other studies have sought to test divergent effects of tactical repertoires across situations (Bernstein 1997; A. Martin 2008; Walker, Martin, and McCarthy 2008; I. Martin 2010; McCammon 2012; Wang and Soule 2016). A few influential texts use practice theory to advance novel frameworks for theorizing social movements (Crossley 2002; Armstrong and Bernstein 2008; Fligstein and McAdam 2012). And a burgeoning literature has begun introducing considerations of practice into the study of repression (Earl, Soule, and McCarthy 2003; Carey 2006; Earl and Soule 2006; Chang and Kim 2007; Davenport, Armstrong, and Lichbach 2008; Ayoub 2010; Chang

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and Vitale 2013; Moss 2014, 2016; Davenport 2015; Honari 2018; Bloom and Frampton 2021).

I build on these insights, political process theory, and postclassical theory to advance a dynamic *practice* theory of repressive effects, situating the effects of repression as part of a larger struggle over the regulation of insurgent practice (Gramsci 1971; Bourdieu 1990; Mann 1993). Repression is fundamentally regulatory, sanctioning historically specific forms of social practice. *The effects of repressive action depend on the specific practices repressed* and are mediated by the political dynamics in which insurgent practices are enacted. Theorizing why specific forms of practice defy repression under some conditions makes it possible to meaningfully compare across cases without assuming that conditions are independently determinant. I propose that whether repression works to quell rebellion—or backfires and increases mobilization—depends on the historical breadth of opposition to repression of the specific *practices* repressed.

I systematically test two observable implications of this theory in the postwar Black Liberation Struggle. First, using hazard analysis and out-of-sample testing, I assess the differential effects of repression on two different forms of Black insurgent practice: civil rights action and violent urban rebellion. Given their relative public support, and contrary to both the inverse-U and classic political opportunity theses, I expect that civil rights practices were resilient, and violent urban rebellion susceptible, in the face of intense repression. Second, contrary to prevailing accounts, given lasting public support for civil rights practices, I expect this resilience persisted well after the decline of the movement. Repression did not quell the Civil Rights movement. To the contrary, I expect rates of repression declined preceding demobilization. Once they defeated Jim Crow, activists deserted civil rights practices because few vulnerable targets remained.

I begin by reviewing theories of repressive effects and proposing a practice theory of repressive effects to transcend their limitations. I then develop testable hypotheses concerning the Black Liberation Struggle, detail the methods of analysis, and present the study findings. I conclude by summarizing the findings and their implications for theories of repressive effects and the study of movements and politics. Generalizing the dynamics of practice offers a theoretical framework that can more fully account for variations in the historical evidence.

REPRESSIVE EFFECTS ON MOBILIZATION

Prevailing Theories

What are the effects of repression on mobilization? A common assumption is that repressive action by authorities does generally discourage participation

in subsequent insurgency. Previous studies have found empirical support for this position. Jenkins and Perrow (1977) argue that early farmworker unionization efforts failed in large part because of the repressive context. In an influential large-*N* cross-national comparative study, Hibbs found that “the lagged or long-run impact of elite repression is to deter” future insurgency (1973, p. 113).

Conversely, others maintain that repression breeds resistance. In a popular literary context, John Steinbeck famously wrote of “the little screaming fact that sounds through all history: repression works only to strengthen and knit the repressed” (1939, p. 137). Indeed, various empirical studies have found a positive effect of repressive action by authorities on insurgency. In a study of the *intifada*, Khawaja (1993) concludes that insurgents use repressive acts to frame authorities as bad, which advances micromobilization, leading to further insurgency. Drawing on biological models, Francisco (1996) develops a “predator-prey” analysis of quantitative data on mobilization and repression events from Germany and Northern Ireland. His findings support the “backlash” thesis that coercion increases protest (see also Francisco 2004). Using survey data from New York City and Hamburg on participation in rebellious political behavior, Muller and Opp (1986) find that high levels of repression generate a high valuation of the promise of challenges to the regime, which in turn increases participation in rebellion.

Inverse-U thesis.—One of the most influential social scientific theories of repressive effects attempts to account for the range of empirical variations with reference to the *intensity* of repression. The inverse-U thesis holds that repression works to quell dissent, but only intense repression. Scholars in this tradition propose that the relationship between repression and insurgency is shaped like an inverse U. In the classic formulation, “the threat and severity of coercive violence used by a regime increases the anger of dissidents, thereby intensifying their opposition, up to some high threshold of government violence beyond which anger gives way to fear” (Lichbach [1987, p. 270] paraphrasing Gurr [1970, pp. 238–39]).

A variety of empirical studies have found support for this position (Gurr 1968, 1969; Muller and Weede 1990; Gupta, Singh, and Sprague 1993; White 1993). Based on a large comparative cross-national study using time series data, Muller (1985, p. 60) concludes that “since countries with an intermediate level of regime repressiveness also may be especially likely to experience comparatively high levels of mass political violence, there seems to be truth in Machiavelli’s dictum that a leader should either embrace or crush his opposition.” This thesis underwrote Colin Powell’s influential doctrine of decisive force (Daalder and O’Hanlon 1999, 133). And a similar view is often advanced in domestic debates regarding the handling of urban uprisings (e.g., Miller 2015).

Political opportunity thesis.—The political opportunity thesis, the idea that structural political context in a given period and place fundamentally

enables or constrains mobilization by a group, revolutionized the study of social movements in the 1980s and 1990s (Bloom 2015; Buechler 2016; Staggenborg 2016). Social movement scholars developed the political opportunity thesis as the cornerstone of a broader political process theory. As part of a broader political process approach to analyzing mobilization, the political opportunity thesis has sensitized countless scholars to the importance of political context (see Meyer [2004] and Bloom [2015] for surveys).

A key facet of the political opportunity thesis concerns repressive effects on mobilization. Instead of the intensity of repression, the classical political opportunity thesis attributes variation in the effects of repressive action on insurgency to temporal variations in the political opportunity of the *social group* repressed.² McAdam explains that expanding political opportunity “for the aggrieved population raises significantly the costs of repressing insurgent action. Unlike before, when the powerless status of the excluded group meant that it could be repressed with relative impunity, now the increased political leverage exercised by the insurgent group renders it a more formidable opponent. Repression of the group involves a greater risk of political reprisals than before” and thus “serves to encourage collective action” (McAdam 1982, p. 43).³ McAdam’s foundational analysis of the role of repression in the Black insurgency is discussed below.

Unsettled findings.—The problem is that overall, as various reviewers have concluded, the results of empirical studies aimed at explaining the effects of repression on mobilization are “highly inconsistent” (Davenport 2007, p. 8). The inverse-U thesis, political opportunity thesis, and any other established theories cannot explain the range of available evidence. The long-standing puzzle of the effect of repression on mobilization has “fundamentally unsettled findings” (Earl 2011, p. 267).

Dynamics of Repression and Insurgent Practice

In order to better account for the range of repressive effects, I heed Tilly’s (2008) call for theory that can accurately generalize causal process across historical situations while transcending invariant models. I seek to develop

² My own recent extensions of opportunity theory have sought to transcend invariant models by centering practice (Bloom 2015).

³ While McAdam’s (1982) foundational formulation, as quoted here and illustrated in the context of the Black Liberation Struggle below, has been especially influential, it is worth noting that some scholars have used the political opportunity thesis differently, more like inverse U. In those applications, greater political opportunity implies lower levels of repression.

such theory by building on recent practice-centered scholarship on social movement dynamics, political process theory, and postclassical theory, to generalize the dynamics of repression and insurgent practice.

Here and throughout I use the term “insurgent practice” as “cultural routines: historically particular forms of action and rhetoric that promise transcendence of specified oppressive conditions by challenging an institutionalized authority” (Bloom 2015, p. 395).

Many movements develop in terms of the adoption of a coherent set of insurgent practices, and what I seek to explain in this study are systematic differences in repressive effects on different kinds of insurgent practice. Insurgent practices are broader than tactics because they bundle ideational claims and specific targets, rather than modular forms of action alone. For example, consider the wave of sit-ins that swept the South beginning in Greensboro, North Carolina, on February 1, 1960. These actions each involved a specific tactic—namely, nonviolent civil disobedience by occupying a space in violation of law and custom—rather than other tactics such as public marches or armed resistance. They also involved a specific target—segregated lunch counters and public places from which Black people were prohibited—rather than other targets such as workplaces, police stations, streets, or political offices. They also involved a specific claim—full participation in U.S. citizenship rights for Black people—rather than Black Power, affirmative action, or revolution. The rapid spread of the sit-ins over the next few months developed in terms of this coherent set of insurgent practices. Variation on any of these three dimensions—such as the call for Black Power, marches rather than sitting in, or targeting police stations—would have generated a very different political dynamic. While there was some experimentation at the margins, it was a set of insurgent practices consistent in tactic, kind of target, and claim that spread. It was not just a tactic of sitting in that proliferated. Sit-ins consistently targeted segregated public spaces and made claims for participation in citizenship rights.

In some historical circumstances, small differences between otherwise similar practices—such as a slight variation in framing, or target, or tactic—can make the difference between whether or not allied support is forthcoming. Similarly, a slight shift in the political context over time, or across geographic location, can also make a big difference in the reception of a specific practice. As Gramsci (1971, pp. 233–39) argued, how much a small difference in insurgent practice matters depends on the political situation. Where civil society is strong, small differences in the character of insurgent practices can affect the response of various third parties, whether broad opposition to repression is forthcoming, and thus whether repression of such practices leads to remobilization. For a fuller discussion on distinguishing between insurgent practices theoretically and in application, see the discussion and conclusions section.

The participation in insurgent practice I seek to explain here is also different than participation in “strategy” in the sense of a deliberate plan of action. “Strategy” implies an individual or a small group of people consciously constructing a plan of action for use in a specific situation. But I am concerned with how forms of action and rhetoric are repeated and emulated by different people over time and space. And I seek to explain the flesh-and-blood action itself rather than plans about the action. Insurgent practice can be studied in data on the occurrence of public events—what insurgents did and said in public. Conversely, studying strategy would require analyzing data on behind the scenes deliberations.

A burgeoning literature has introduced consideration of what insurgents do into the study of repression. Taking repression as the dependent variable, a variety of authors have argued that the character of insurgent action shapes repressive response by authorities. Earl and others (Earl, Soule, and McCarthy 2003; Earl and Soule 2006; Ayoub 2010; Chang and Vitale 2013) show that police are more likely to take repressive action against confrontational and threatening events. Chang and Vitale (2013) have shown that authoritarian states seek to repress not only situational threats but also challenges to their legitimacy.

Taking repression as the explanatory variable, a number of recent case studies sought to transcend invariant models accounting for historical processes in influencing repressive effects. In an interview-based study of Jordan, Moss (2014) seeks to disaggregate invariant models of repressive effects on mobilization showing how tactical innovations by insurgents can shape repressive effects. In the case of South Korea’s democracy movement, while repression led to a decrease in the number of protests it also led to an increase in alliance formation between social groups (Chang 2008) and facilitated the development of the movement’s organizational and discursive capacity (Chang and Kim 2007). Closely unpacking a wealth of internal archival data on the Republic of New Africa, Davenport (2015) illuminates the life course of a movement organization immersed in a sustained struggle with the state for survival. He shows that when authorities can convince publics in democracies that overwhelming force is warranted, repression works.

Various recent comparative studies have also challenged invariant models of repressive effects. In a review piece, Honari (2018) argues that better accounting for repressive effects will require taking movement agency seriously. Moss (2016) shows how historical differences between Syria and Libya shaped transnational repressive effects on the Arab Spring. In a nine-country comparative study, Carey (2006) shows that the character of past actions by both insurgents and authorities dynamically shapes repressive effects. In a comparative study of 149 countries, Davenport et al. (2008) show that no single invariant model can explain the escalation of insurgency to civil war and that historical differences in movement dynamics are important. Zhao’s (2000)

study comparing the surprisingly traditional rhetoric and rituals of the 1989 Beijing student movement to the less traditional earlier Chinese student mobilizations demonstrates the nexus of macrostructural context and insurgent practice powerfully shaping bystander response, and thus the effects of state repression.

As a whole, these studies recognize that what insurgents do matters and that their actions can have a tremendous influence on movement trajectories. Neither authority actions nor political conditions independently determine mobilization outcomes.

Generalizing the dynamics of practice makes it possible to account for the effects of repression obscured by the prevailing theories at two levels. First and most fundamentally, the consideration illuminated by generalizing the dynamics of practice is that repression is regulatory, sanctioning specific forms of insurgent practice. I conceptualize political repression as coercive action taken by authorities and their agents against participants in *a specific political practice*.⁴ Most obviously, this includes arrests and political violence directed by the state and its agents against such participants. The key distinction from some common conceptualizations is that here repression targets a specific practice and implicitly the *form* of that practice. In other words, repression targets people with regard to what they do, not solely who they are.⁵ Repression, understood in this way, is a practical deterrent. Authorities usually take repressive action with the intention of stopping participation in a specific insurgent practice and to deter other potential insurgents from engaging in that form of practice.

Since authorities target specific forms of insurgent practice for repressive action to discourage people from engaging in those forms of practice, accounting for repressive effects requires some historical knowledge about the relationship between the historical situation and the specific practices repressed. The effects of repression on mobilization depend on the specific practices repressed and the context in which they are repressed (Opp and Roehl 1990; Almeida

⁴ Scholars employ, and sometimes debate, the merits of widely divergent conceptualizations of repression. My aim here is not to resolve such debates. In my view, as Becker (1998) contends, concepts are empirical generalizations and as such are somewhat arbitrary. Many generalizations are possible, and no single conceptualization is inherently best (Becker 1998). The salience of any conceptualization inheres in descriptive coherence, and the explanatory power of the theoretical propositions in which it is embedded. Many of the conceptualizations of repression used in different contexts are inappropriate to my purposes here, and I would not expect my propositions to hold using such conceptualizations. For the sake of clarity, this conceptual discussion articulates my concept of political repression, that particular category of social life for which I expect my theoretical propositions hold.

⁵ Thus, in my conception, repression is different from oppression—which may include various forms of coercive violence or hardship imposed on members of a social group without regard to their participation in a specific practice.

2008; Bloom 2015; Bloom and Martin 2016). As Bourdieu argues, the organizing principles of social life are practical, played out differentially in different situations, not schematically mapable. Categorical effects diverge across time and place because the situations in which people apply relatively consistent principles of practice are different (Bourdieu 1990, esp. pp. 100–101). Thus in the face of repression, different forms of practice sustain mobilization in different contexts. Aldon Morris recognized this long ago, writing that movements “will not crystallize” if insurgents have “not developed tactics and strategies that can be effectively used in confrontations with a [specific] system of domination” (1984, p. 283).

Second, in particular, it is the “politics” of the practice that matter. Repression is not simply a generic interaction between a monolithic state and an undifferentiated set of individuals. As Gramsci (1971) theorized, insurgencies disrupt unstable political equilibria, forcing influential political actors to take repressive action and/or make concessions in an effort to preserve their more fundamental interests. Repressive action is not the natural, or unilateral, expression of monolithic ruling interests. Rather it is part of an ongoing contest over the shape of the future between interests differentially institutionalized in past struggle. “The life of the State is conceived of as a continuous process of formation and superseding of unstable equilibria” (Gramsci 1971, p. 182). Weberian institutional statisticians have shown this broadly holds. While states participate in “rule making, backed up by some organized physical force” exercised over a territory, a state is not homogeneous, but rather a historically constituted and “differentiated set of institutions” (Mann 1993, 55). Rather than unitary and systemic, states are messy, and contradictory, embodying the outcomes of past struggles. Not only do states maintain and police historically specific social relations, but different sets of political actors support distinct standards of legitimacy (Gramsci 1971, pt. 2; Mann 1993, chap. 3).

Because the state is composed of and influenced by different and often competing constituencies (see Gramsci 1971, “war of position”), repressive action by authorities is contested. Constituencies whose interests are affected in different ways by the same repressive action will respond differently to that repressive action. Thus the cleavages between political constituencies shape the effects of repression. In some situations, repressive action by authorities will face resistance from broad constituencies, bolstering insurgents’ causes. Allied interventions can counteract authorities’ use of coercive power. As I found in an earlier case study (Bloom 2015, p. 396), “Allies drawn to support an insurgency [can] resist repression of the insurgent movement by the targeted authorities, making it easier for insurgents to sustain their challenge.” To encourage mobilization in the face of repression, the opposition must have bearing on how authorities wield coercive force or must bring other coercive or countervailing forces to bear on the situation.

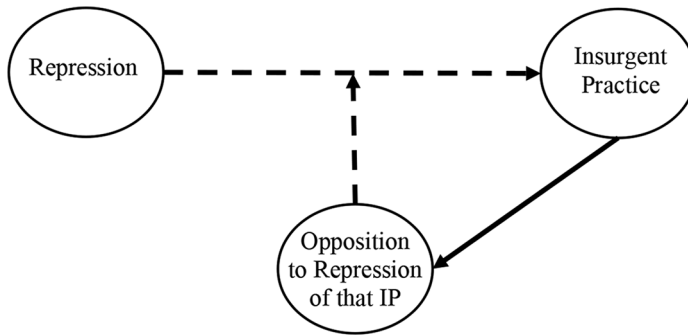


FIG. 1.—Repressive effects on participation in an insurgent practice

Repression of insurgent mobilization forces diverse political actors to take sides—either explicitly, or implicitly through their silence. Because states regulate *practices*, all political actors are forced to choose whether to accept repression of a specific insurgent practice, or whether to intervene, bolstering the insurgency. In a second case study (Bloom and Martin 2016, p. 397), I found that “the level of repression [does] not independently affect the level of mobilization in a consistent way. . . . Instead, the level of repression interact[s] with the political reception of insurgent practices to affect the level of mobilization.” The key question is not whether various political actors support the claims of insurgents in the abstract, but whether—given insurgents’ practical actions—these actors will oppose repression of a specific set of insurgent practices.

Thus the repressive effects on participation in an insurgent practice can be diagrammed as shown in figure 1. Insurgents are able to defy the de-escalatory capacity of repressive action—illustrated by the dashed arrow from repression to insurgent practice—by developing insurgent practices that draw allied support in the face of repression. My core proposition is that when insurgent practices draw broad opposition to repression, then repressive action by authorities increases mobilization. This proposition is illustrated by the interaction effect from opposition to repression, reversing repressive effects in figure 1.

HYPOTHETICAL EFFECTS OF REPRESSION ON BLACK INSURGENT PRACTICE

The Black insurgency in the United States that peaked in the 1960s was the forging ground for classical movement theory. It provides a strategic test for assessing the importance of practice in determining the effects of repression on mobilization.

Inverse U

The inverse-U thesis proposes that the effects of repression by authorities on subsequent mobilization depend on the *intensity* of repression. If this is true, the pattern should certainly hold for repression applied to members of the same social group under a given regime. Thus, there is reason to believe that in the restricted scope of Black insurgent mobilization in the post-war United States, similarly intense repression should have similar effects on subsequent remobilization regardless of the practice repressed. In particular, the inverse-U thesis suggests that while moderate levels of repression can be escalatory, high levels of repression deter mobilization, leading to the first hypothesis:

HYPOTHESIS 1.—*High levels of repression of Black mobilization decrease the likelihood of subsequent remobilization (implied by inverse-U thesis).*

Political Opportunity

Classic political process theory, developed largely in the study of Black insurgency, sought to go beyond simple reductionist models to account for historical context and political process. Its core political opportunity thesis proposed that the effects of repression on subsequent mobilization vary over time with the opportunity for the *social group* repressed. In his foundational book on the Civil Rights movement, Doug McAdam shows that the ability of civil rights activists to draw allied intervention against repression in the early 1960s was crucial to the development of the movement (1982, pp. 174–79). McAdam (1982, p. 174) describes what he calls the “critical dynamic” of the Civil Rights movement: “Lacking sufficient power to defeat the supremacists in a local confrontation, insurgents sought to broaden the conflict by inducing their opponents to [acts of repression] to the point where supportive federal intervention was required.” In the lunch counter sit-ins, the freedom rides, municipal integration campaigns, marches, and voter registration drives in the South during the early 1960s, insurgents peaceably violated segregationist law and de facto Black disenfranchisement and were brutally repressed by local White authorities and vigilantes. This repression threatened Black institutions such as the church and embarrassed the federal government as it attempted to assert moral leadership in a decolonizing world.

McAdam (1982, p. 73) explains the escalating effect of repressive action against Black mobilization with reference to a conducive structure of political opportunity for Black people at that time. From the classic political opportunity perspective, expanding opportunity for Black people in the early 1960s enabled widespread mobilization in the face of repression. Accounting for context effects in this classic manner leads to the second hypothesis:

HYPOTHESIS 2.—Repression of Black mobilization increases the likelihood of subsequent remobilization during the period of expanding political opportunity for Black people (implied by political opportunity thesis; contrary to inverse-U thesis).

Insurgent Practice

I believe McAdam's historical insights into these processes of civil rights mobilization were largely correct. And political process theory inspired a generation of nuanced empirical research on social movements. But as science progresses, it reveals the limitations of earlier models. It appears that full theorization of McAdam's historical insight was constrained by the invariant structural assumption of the classical political opportunity thesis. The assumption was that mobilization could be explained with reference to structural variables concerning Blacks as a group. Can the capacity to account for the range of empirical evidence concerning political process—and repressive effects in particular—be expanded by reconsidering these structuralist assumptions, and centering practice?

Twenty-first-century historians have recognized a variety of movements that make up the overall Black Liberation Struggle in the postwar years (Kelley 2002; Joseph 2006; Theoharis and Woodard 2016). While sharing the aspiration for freedom, in different waves of mobilization, different groups of Black insurgents made different claims, employed different tactics, and targeted different institutions. So if repressive effects on mobilization depend on the practices repressed, it would be reasonable to expect different repressive effects for different forms of Black insurgent practice, during the same period and under the same regime. Two of the most widely employed forms of insurgent practice in the Black Liberation Struggle were civil rights practice and violent urban rebellion. Did repression of these different forms of Black insurgent practice have different effects on subsequent remobilization?⁶

⁶ McAdam (1983) prefigures an argument something like this. But without the benefit of the subsequent generation of empirical work, and informed by structuralist assumptions about context effects, he proposed that it is the novelty of tactical innovations that generates new escalations of insurgency, rather than any specific match between the practice and the political context. This led to the assumption that such innovations become repressible as the novelty wears off (McAdam 1983). But from the insurgent practice view, so long as the political conditions under which specific practices draw broad support persist, I propose they will continue to generate mobilization in the face of insurgency. I argue that rather than the novelty of effective tactics wearing off, concessions outmoded particular forms of insurgent practice—such as the integration of lunch counters and interstate busing and Black access to the ballot—bringing the efficacy of specific forms of

Civil Rights movement.—Civil rights insurgent practices consisted of non-violent defiance of Jim Crow coupled with claims for Black participation in citizenship rights. Even where they entail similar tactics, civil rights practices can be distinguished from other kinds of nonviolent practices. For example, Black nonviolent workplace actions, such as strikes demanding collective representation of Black workers or improvement to wages and working conditions, are not civil rights actions as they entail different targets and claims.

The repression civil rights insurgents faced at the hands of local authorities and White mobs drew other actors into the fray—including more moderate Blacks, non-Black supporters, and the federal government. This dynamic was crucial to the success of the sit-ins, the Freedom Rides, Freedom Summer, the community campaigns, and the voting rights campaigns and was widely recognized by movement leaders. For example, Martin Luther King Jr. explicitly identified this dynamic in his discussion of the Selma campaign of 1965:

The goal of the demonstrations in Selma, as elsewhere, is to dramatize the existence of injustice and to bring about the presence of justice by methods of nonviolence. Long years of experience indicate to us that Negroes can achieve this goal when four things occur:

1. Nonviolent demonstrators go into the streets to exercise their constitutional rights.
2. Racists resist by unleashing violence against them.
3. Americans of conscience in the name of decency demand federal intervention and legislation.
4. The administration, under mass pressure, initiates measures of immediate intervention and remedial legislation.⁷ (King 1965a, p. 17)

This kind of resistance to Jim Crow had not always been viable. For example, in Elaine, Arkansas, in 1919 when, in response to a Black organizing drive, local authorities, with the support of federal troops, massacred hundreds of Black men, women, and children and arrested hundreds more to send a message that Black resistance to White authority would not be tolerated. But by 1960, the political dynamic had changed. Black churches, colleges, and other organizations had developed considerable social and political strength (Morris 1984). The decline of the cotton economy, and

civil rights practice to an end. Interestingly, much of the historical evidence provided in McAdam's original 1983 article appears to support my position. I systematically test hypotheses concerning the end of the Civil Rights movement drawn from these propositions below.

⁷ King expressed similar ideas on many occasions in many different ways (see, e.g., King 1963a; 1963b, p. 27; 1967, p. 185).

the broader agricultural and industrial transformation of the South, had removed much of the economic impetus for Jim Crow (Piven and Cloward 1979). The United States' entrance into the Cold War and competition with the Soviets for the allegiance of newly independent former colonies made the formal caste subordination of Black people in the United States a serious foreign policy problem (Dudziak 2011). Earlier Black movements had compelled the federal government to break the post-Reconstruction accord and advocate for civil rights (Bloom 2015). Increasingly, broad Black, liberal, and federal opposition could be expected to repression of civil rights practice.

In addition to anecdotal evidence, quotes from activists, and secondary analyses, it is worth considering public opinion data concerning repression of different kinds of Black insurgent practice to set up hypotheses for quantitative testing.⁸ By the 1960s, public opinion polls indeed revealed broad public opposition to repressive action against nonviolent civil rights activism. For example, in the voting rights conflict in Selma, Alabama, respondents in a representative national poll sided with protesters against local authorities who repressed them by a ratio of more than two to one. Black respondents almost universally sided with the protesters.⁹ Following the Birmingham campaign, President Kennedy was inundated with letters protesting repression, such as one from a Black mother with a news photo clipping of a police dog attacking a protester, and the handwritten notation "who gave this state policeman the right to turn a killer-trained dog on humans?"¹⁰ When the Kennedy administration sent U.S. marshals to intervene against repression of the Freedom Riders, national public opinion supported this action more than five to one.¹¹

⁸ Public opinion did not itself constitute the broad opposition that I theorize affects the outcome of repression. Instead it served as a proxy, indicative of whether the kind of opposition that could effectively challenge repressive action was likely forthcoming in response to various events on the ground. For a fuller discussion of this logic of inquiry and the application of theory to distinguish meaningfully between insurgent practices in different cases, see the discussion and conclusions section.

⁹ "In the recent showdown in Selma, Alabama, over Negro voting rights, have you tended to side more with the civil rights groups or more with the State of Alabama?" (May 17, 1965): 48% favored demonstrators compared to 21% who favored Alabama; 95% of Black respondents favored demonstrators vs. 0% who favored Alabama (Harris Survey, May 1965, via Pew).

¹⁰ JFK Library, document JFKMPFPOF-0201-003-p01. <https://jfklibrary.tumblr.com/post/164711822171/public-opinion-in-the-jfk-library-archives-civil>.

¹¹ "Do you think President Kennedy did the right thing or the wrong thing in sending U.S. marshals to Montgomery, Alabama [to protect Freedom Riders against repressive action by White mobs and local authorities]?" Seventy percent of people polled said Kennedy did the right thing compared to only 13% who thought he did the wrong thing (American Institute of Public Opinion [AIPO] June 21, 1961; Erskine 1962, p. 145). Broad opposition to repression of civil rights insurgent practices and support for federal intervention should not be confused with public support for the insurgent practices themselves, which is different. For example, a much smaller proportion supported the insurgent actions the

Given broad opposition, sometimes prompting federal intervention, from the vantage of insurgent practice theory, it is reasonable to expect that repressive action against civil rights practices in this period would fail to deter potential insurgents, contributing to escalating mobilization. McAdam (1982) considers many aspects of this historical dynamic, and my argument here builds upon and is similar to the political opportunity perspective with an important exception. McAdam and political opportunity theory suggest that these historical dynamics provided an opportunity for Black mobilization generally. I argue, instead, rather than being conducive to all forms of Black insurgent practice, that these historical developments specifically made civil rights insurgent practices difficult to repress.

For example, consider the Freedom Rides of 1961. Drawing lessons from the 1960 sit-ins, the Congress of Racial Equality (CORE) organized the Freedom Rides with the intention to provoke arrests by local authorities. The regulation of interstate busing was the purview of the federal government, and in 1960, following cumulative grassroots and legal challenges to Jim Crow, the Supreme Court had ruled against segregation in interstate bus terminals.¹² Yet throughout the Deep South, formal segregation remained both legal and customary. The Kennedy administration claimed to support civil rights but was reluctant to challenge the Jim Crow racial policies of Southern Democrats, who were an important part of the administration's national Democratic Party coalition.

CORE decided to force the issue by defying Jim Crow in interstate busing where the federal government had a clear legal mandate to intervene. Specifically, Freedom Riders bodily integrated interstate busses and facilities at the bus terminals serving them, calling for enforcement of the federal law. James Farmer, national director of CORE explained: "Our intention was to provoke the Southern authorities into arresting us and thereby prod the Justice Department into enforcing the law of the land. We started the Freedom Rides with thirteen people. But after one bus was burned in Anniston, Alabama, and the riders on another were beaten and abused, we were deluged with letters and telegrams from people all over the country, volunteering their bodies for the Freedom Rides" (Farmer 1965, p. 69). Defiance of Jim Crow led to repressive action by local authorities, which in turn drew allied support to the movement from not only local Black organizations, and liberals, but also forced the federal government to intervene. Seeing all this third-party support for the Freedom Riders in the face of repression encouraged potential

Freedom Riders were taking. "Do you approve or disapprove of what the 'Freedom Riders' are doing?" Sixty-four percent disapproved, while only 24% approved (AIPO June 21, 1961; Erskine 1962, p. 145).

¹² *Boynton v. Virginia*, 364 U.S. 454 (1960).

insurgents, and hundreds of people from all over the country joined the Freedom Rides, participating in this novel form of insurgent practice, often at great personal risk.

Following the Freedom Rides, some savvy authorities came to understand this dynamic and sought to avoid telegenic repression of protesters. Notably Sheriff Laurie Pritchett of Albany, Georgia, responded to the Albany desegregation campaign with cordial mass arrests. Pritchett imposed high costs on insurgents, jailing 500 by December of 1961, while avoiding imagery of brutal repression that might drive allied support. The Albany movement was never able to build momentum and soon unraveled (Carson 1995; Branch 2007). But ultimately polite repression could not stop the Civil Rights movement. Maintaining Jim Crow inherently required systematic repression. Movement leaders responded by crafting more targeted campaigns (King 1965*b*) and picking fights where the repressive regime institutionalized to maintain caste subordination was more flagrant, such as in Birmingham (King 1965*a* see above). Ultimately, to stop the movement, Jim Crow was dismantled.

Civil rights-type practices were not effective for challenging all forms of Black subordination in this period. Just Jim Crow. For example, during the height of the Civil Rights movement CORE invested considerable resources and placed some of their most effective organizers into a campaign to integrate employment at Woolworth stores in the North, using similar tactics to the sit-ins. But with no formal racial exclusion to defy, it was hard to get much leverage. Shutting down stores and demanding jobs, many people saw the activists as aggressors. After CORE invested more than a year and lots of scarce resources into the campaign, Woolworth's hired a single Black person, claiming they had integrated employment. CORE gave up the campaign (Meier and Rudwick 1973).

As Morris (1993, p. 631) shows in the case of Birmingham, the key to the efficacy of civil rights practice to challenge Jim Crow was its capacity to create "social disorder, not violence." While the brutality of the repression against civil rights activists drew broad attention, Morris demonstrates that more fundamental was the capacity of the Birmingham campaign to disrupt business as usual. This was not a dynamic driven by third parties or violence. Civil rights activists drove the process by disrupting the Jim Crow status quo in a manner that forced third parties to take a stand. Third-party support allowed insurgents to continue disrupting the status quo in the face of repression. This dynamic depends on opposition to repression of the specific *practice* repressed.

Violent urban rebellions.—Contrary to the classical political opportunity thesis, from a practice-centered perspective, the civil rights dynamic was specific to the repression of civil rights practice and not applicable to other forms

of Black insurgency. According to the insurgent practice theory of repressive effects elaborated above, disruptive insurgent practices force social actors to take a position. The crux of the matter is opposition to repression of specific insurgent practices. If repressive actions by authorities are broadly viewed as justified, repressive action meets little resistance, and authorities succeed in de-escalating the insurgency.

Beyond civil rights action, another major category of Black insurgent practice prevalent during the Civil Rights era was violent urban rebellion—also referred to as riots. Violent urban rebellions targeted stores and sometimes infrastructure in Black neighborhoods for looting and property destruction. And they targeted police and other public officials with thrown objects, such as bottles and rocks, and occasionally sniping. Rather than sporadic instances of violence in otherwise nonviolent protests, violent urban rebellions consist of relatively continuous violent action. And in contrast to civil rights claims, the ideational claims articulated by Black violent urban rebels during this period usually responded to specific instances of racist action by authorities, especially police violence, and sometimes concerned Black Power.

In the 1960s United States, Black violent urban rebellions were widely unpopular among the general public. In a representative national poll following major violent rebellions in Newark and Detroit in 1967, two-thirds of respondents believed that people who threw firebombs during riots should be shot in the street. Eighty-six percent of respondents said they felt that the recent “race riots” had “hurt . . . the cause of civil rights for Negroes.” Only 4% saw the “riots” as helping the cause (Erskine 1967).

Thus, centering the question of the *form of practice* repressed, it is reasonable to expect that intense repression of Black violent urban rebellions would generally de-escalate the insurgency. This expectation was implied by the National Commission on the Causes of the Prevention of Violence, created by President Johnson in response to the urban rebellions following the assassination of Martin Luther King Jr. in 1968: “When group violence occurs, it must be put down by . . . the use of whatever force may be required.”¹³ The working assumption implied is that if sufficiently forceful, repression will succeed in quelling violent Black urban rebellion.

Anecdotaly, this assumption often appears correct. For example, following a rally in the Dixie Hill neighborhood of Atlanta, Georgia, on June 19, 1967, in which Stokely Carmichael called for “revolution,” a crowd of young Black people stormed a shopping center, smashing windows. Rebels threw rocks and Molotov cocktails at police, and police reported sniper fire as well.

¹³ National Commission on the Causes and Prevention of Violence, 1969, *To Establish Justice, to Insure Domestic Tranquility*, excerpted in Platt (1971, p. 433).

Local authorities responded with firm repression. The mayor imposed a 9 p.m. curfew enforced by a heavy police presence. Police arrested about a dozen rebels and shot two Black people. As expected for this type of insurgent action, most Atlantans, including many Black residents of Dixie Hill, supported authorities and opposed the rebellion. Seven hundred Black residents of Dixie Hill signed petitions, asking Carmichael to leave. The insurgency quickly abated.¹⁴

This is not to suggest that repression quells all forms of violent insurgent practice. As discussed above, in some times and places repression has failed to quell violent insurgency. This was also true for specific forms of violent Black insurgent practice in the United States such as revolutionary nationalist mobilization by the Black Panther Party in the late 1960s. During this period, repression did not work to quell the armed politics of the Black Panther Party. The more repressive action the state took against Black Panthers from 1967 to 1969, the more allied support the party garnered, and participation in Black Panther practices expanded (Bloom and Martin 2016; see app. E for preliminary quantitative assessment). Rather than suggesting a similar effect of repression on all forms of violent action, insurgent practice theory gives reason to expect that intense repression would work specifically to quell violent Black urban rebellions in the United States during this period.

Thus, by centering practice to explain political process, insurgent practice theory attempts to extend the insights of political process theory to more fully account for repressive effects. This leads to differential expectations regarding violent urban rebellion and the Civil Rights movement, and my third hypothesis:

HYPOTHESIS 3.—Intense repression decreases likelihood of remobilization for violent urban rebellions but increases likelihood of remobilization for civil rights insurgent practices (implied by insurgent practice theory; contrary to elements of both the inverse-U and political opportunity theses).

Decline of the Civil Rights movement.—Extending process theory in this way to account for practice also contradicts standard accounts of the role of repression in the decline of the Civil Rights movement. A long-standing puzzle in scholarship on the Civil Rights movement concerns explaining the movement's demise in the late 1960s. As Charles Payne argues, civil rights activists "still had a clear agenda of things they wanted changed" in

¹⁴ Walter Rugaber, "Atlanta Negroes Bid Outsiders Leave," *New York Times*, June 22, 1967, p. 24; "Chaos Follows Atlanta Shooting," *Chicago Daily Defender*, June 20, 1967; "Snipers Shoot at Police in Atlanta Riots," *Chicago Tribune*, June 20, 1967; Jack Nelson, "Curfew Eased in Negro Area of Atlanta," *Los Angeles Times*, June 22, 1967, p. 19; National Advisory Commission on Civil Disorders, *U.S. Riot Commission Report*, March 1968, pp. 52–56.

the late 1960s (1995, p. 361). Yet despite the unparalleled success of the Civil Rights movement at dismantling Jim Crow and transforming racial relations in the United States in the early 1960s, Black insurgents largely abandoned civil rights practices in the late 1960s. Why? In particular, how may repressive dynamics have contributed to the decline of Black nonviolence?

The political opportunity thesis suggests an obvious answer: political opportunities for Black people contracted, making the movement repressible. Structural changes during this period led to “weakening the movement by redefining the political significance of Blacks in such a way as to afford them less leverage with which to press their demands” (McAdam 1982, p. 192). Thus for the period of contracting opportunities from the late 1960s on, the expectations of inverse-U and political opportunity theories align—intense repression should reduce the likelihood of remobilization.

HYPOTHESIS 4a.—Intense repression of Black mobilization decreases the likelihood of subsequent remobilization from the late 1960s on (implied by both the political opportunity and inverse-U theses).

If hypothesis 4a is true, it raises serious problems for insurgent practice theory. As the Civil Rights movement won significant gains through the early 1960s, it also gained broader support. Studies show that public support for various forms of Black civil rights increased over these years (Schwartz 1967). From a practice-centered perspective, there is little reason to suspect that civil rights practices would have become more susceptible to repression in the late 1960s.

HYPOTHESIS 4b.—Intense repression of civil rights action continues to increase the likelihood of remobilization from the late 1960s on (implied by insurgent practice theory; contrary to both the political opportunity and inverse-U theses).

A different explanation of the role of repression in the decline of the Civil Rights movement in the late 1960s—compatible with insurgent practice theory—has been suggested (Robnett 2002; Bloom 2014; Bloom and Martin 2016) but never subjected to systematic tests. From this perspective, civil rights action was specifically effective at challenging Jim Crow, largely through defiance of formal segregation. In the post-1960s political context, repression of civil rights activists was powerfully resisted by Black organizations, liberals, and the federal government, leading to escalating mobilization. As the Civil Rights movement proceeded, White supremacist local authorities and their allies came to recognize that defending Jim Crow against civil rights insurgency was a losing battle, and they gradually stopped taking repressive action against civil rights practices. As White supremacists stopped defending Jim Crow, the rate of repression of civil rights practice declined. Over time, there were fewer formally segregated institutions for insurgents to defy. As formally segregated institutional targets became increasingly

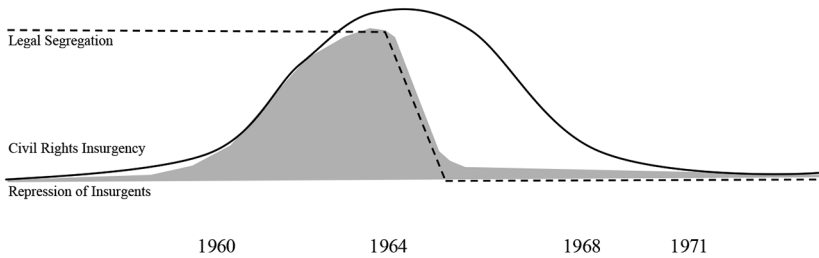


FIG. 2.—Hypothetical graph of mobilization versus level of repression in the Civil Rights movement.

scarce, in turn, civil rights practices lost their efficacy, and civil rights mobilization declined.¹⁵

While the Civil Rights movement won the battle against Jim Crow, the war against racism was far from over. White supremacists turned to largely informal means of defending and advancing White privilege. Antiracist activists sought to continue the movement. But civil rights practices never worked to challenge other forms of oppression such as poverty or police brutality. Civil rights—type tactics and claims, without formally segregated targets to defy, did not generate the same political dynamic.

Observable implications of this hypothetical practice-centered explanation of the role of repression in the arc of the Civil Rights movement are graphically depicted in figure 2. First, we should see a rapid rise in the level of civil rights mobilization tightly correlated with a rise in the level of repression of civil rights activists. Next, as legal segregation and de facto disenfranchisement are abolished, White supremacist local authorities and their allies stop defending Jim Crow, and we should see a quick falling off in the rate of repression of

¹⁵ This substantive argument about the decline of the Civil Rights movement is based on the insurgent practice theory about how repressive effects on mobilization are moderated by the match between practice repressed and historical context. From the insurgent practice theory perspective, “Insurgents garner influence by developing practices that leverage institutional cleavages. . . . When insurgents advance a practice that challenges the authority on one side of an institutionalized conflict while drawing allied support from the other side, they seize the opportunity provided by that cleavage to garner influence and following.” Such allied support can help insurgents resist repression, and thus contribute to mobilization (Bloom 2015, p. 396). In the Civil Rights movement, by nonviolently defying legal and customary Jim Crow and making claims for participation in citizenship rights, civil rights insurgents leveraged an institutionalized cleavage between local authorities intent on preserving Jim Crow on the one hand and, on the other hand, Black organizations, liberals, and the federal government who by 1960 had all vocally opposed Jim Crow. Bodily defying formal segregation, civil rights practices were thus uniquely suited to challenge Jim Crow from 1960 onward. Repression of civil rights activists forced purported allies to take a stand. Repression of other Black insurgent practices did not entail the same political dynamics.

civil rights insurgents. As the rate of repression of civil rights practice drops, the level of mobilization should at first persist as people hope to continue the earlier efficacy of the civil rights insurgency. But eventually, mobilization rates will fall off as Jim Crow targets become scarce and it becomes clear that civil rights practices have lost their leverage and efficacy generally.

These observable implications lead to my final hypothesis:

HYPOTHESIS 5.—Declining levels of repression of civil rights action precede the decline of civil rights insurgency (implied by insurgent practice theory; contrary to all theories that suggest repression caused the decline of the Civil Rights movement).

RESEARCH DESIGN

Training Data and Out-of-Sample Data

Data for my analysis are based on observations of Black insurgent events in the United States over a 40-year period. I test final models using the data set Dynamics of Collective Action, 1960–95, compiled under the direction of Doug McAdam, John McCarthy, Susan Olzak, and Sarah Soule. These data include a catalog of 23,616 protest events in the United States. For events to be included in the data set, they had to fit three basic criteria: (1) more than one person must have participated in the event; (2) participants must have articulated a claim, such as a grievance; (3) the event must have occurred in public. I included in the analysis the 4,717 protest events explicitly identified as initiated by Black people.

These data were collected in a National Science Foundation–funded two-stage process. The first stage consisted of identifying protest events. This was accomplished by a close reading of every article published in the *New York Times* over the period and including every protest event discussed. This method captured many small events and events embedded in articles primarily concerned with other issues, yielding a much more comprehensive set of events than index searches. The second stage consisted of project personnel coding events for variables of interest. Intercoder reliability measured above 90%. Analysis of event catalogs constructed from newspaper data is a staple of social movement analysis (Davenport, Soule, and Armstrong 2011, p. 157). This specific data set has been employed in a variety of influential studies (including McAdam and Su 2002; Earl et al. 2003; Van Dyke, Soule, and Taylor 2004; McCarthy and McPhail 2006; Walker et al. 2008; Davenport et al. 2011; Wang and Soule 2012).¹⁶

While newspaper-based data in general, and this data set in specific, have vastly expanded the range of social movement studies possible,

¹⁶ The data, and a fuller description of collection techniques, are available at <http://www.dynamicsofcollectiveaction.com>.

newspaper-based data are not without their limitations. First, news coverage can introduce description bias. Newspaper reporters and editors have their own social perspective that shapes how events are described. Despite description bias, newspaper data are generally consistent in their capture of “hard” facts (Earl et al. 2004; Davenport 2010). Second, newspaper coverage contains selection bias (McCarthy, McPhail, and Smith 1996; Barranco and Wisler 1999; Oliver and Myers 1999; Oliver and Maney 2000; Earl et al. 2004; Myers and Caniglia 2004). In particular, news has been shown to be biased toward the coverage of intense and violent events (Barranco and Wisler 1999; Myers and Caniglia 2004), police involvement (Oliver and Maney 2000), and events that address issues of current media interest (McCarthy et al. 1996). News selection bias makes it challenging to draw accurate inferences about patterns in the real world because patterns in the data may be artifacts of editorial selection (Barranco and Wisler 1999).

The current study controls for news selection bias by comparing the effects of repression on different kinds of movement events that have already been reported. Assuming newspaper selection bias affects which kind of events are included in the data set, it should do so regardless of the independent variable’s effect on subsequent events. For example, if news selection bias toward violent events leads to inclusion of a greater proportion of violent urban rebellion events in the data set, this should be true independent of repressive effects. The news selection bias literature provides no reason to expect that repressive action against violent events should decrease news coverage of subsequent violent events. In fact, the inverse may be true.

Despite these controls, newspapers do not cover all events, and editorial selection introduces systematic bias in ways that are impossible to account for. Readers should be aware that data on some coherent subset of movement events not included in this data set might display different tendencies than those found in the events covered here.

Before looking at the test data, I conducted the preliminary tests and trained my final models on a similar but distinct data set, Ethnic Collective Action in Contemporary Urban U.S. 1954–92. (See app. C on use of out-of-sample testing to rigorously assess the explanatory power of theory and prevent model overfitting.) The training data are also based on observations of collective action events as reported in the *New York Times*. It differs from the Dynamics of Collective Action data in a variety of ways, including the years covered, data collection based on indexes, the criteria for event inclusion and variable coding, the personnel who conducted the actual coding, and most importantly, the number of events included in the catalog.¹⁷

¹⁷ The test data are not purely out of sample in the sense that despite different coders and somewhat distinct procedures, there is undoubtedly some overlap in the events covered

I trained my models using a subset of the Ethnic Collective Action data set composed of the 1,572 events explicitly identified as initiated by Black people.¹⁸

Method of Analysis

The logic of inquiry is to interpret the historical situation I am analyzing using secondary sources and some polling data, to apply the theory, and then to assess observable implications of this application—specifically assessing the hypothesized effects of repression on subsequent remobilization.¹⁹ To assess the effects of repression on subsequent remobilization, and control for temporal clustering, I used a basic logistic hazard model. The unit of analysis is the city-day. My core empirical question is how likely is a mobilization event in a specific city given an earlier mobilization in that same city? My dependent variable thus measures whether or not there was a mobilization event in a given city on a given day following a previous mobilization event, coded 1 if there was a mobilization event on that given city-day and coded 0 if there was not.

I code civil rights-type protests to include all rallies, pickets, boycotts, and disruption of normal scheduled activities in protest that are initiated by Black people making racial justice claims and that involved no violence or threat of violence by protesters.²⁰ I code violent urban rebellion events

in the two data sets. That said, the test data set includes many more observations of repeat civil rights action in the South—688 observations vs. 150 observations. So even if every one of the training events were included in the test data, at the very minimum, 78% of the repeat events in the test data are not counted in the training data. The purpose of out-of-sample testing is to avoid model overfitting and to prevent modeling sample noise and ensure that the model actually reflects fundamental relationships in the data. See app. C. For these practical purposes, the extent of the test data relative to the training sample ensures that the test data function as out of sample. Any quirks in the training data that might have been accidentally seized upon through model overfitting would be greatly diluted once the models were applied to the much more extensive test data. No such dilution of findings occurs in this out-of-sample test. Instead, applying models calibrated on the training data, sight unseen, to the test data yielded stronger, more significant results. That tells us definitively that the findings are not the result of post hoc model calibration or model overfitting but instead reflect more fundamental relationships between variables in the data set.

¹⁸ These data, and a fuller description of collection techniques, are available at http://data.stanford.edu/urban_ECA.

¹⁹ See the discussion and conclusions section for a discussion of this logic of inquiry and the application of theory to distinguish meaningfully between insurgent practices in different cases.

²⁰ `grp1==1 & thrt==0 & (evnform==2 | evnform==3 | evnform==4 | evnform==6 | evnform==9)`.

to include hostile crowd actions, spontaneous disruptions, and attacks on property initiated by Black people and involving the use of violence.²¹ Racial claims were usually implied, although not always explicitly articulated.

The variable I use to control for clustering is time from the earlier mobilization measured in days. That is, for a given city the day after an initial mobilization event, the time variable is coded 1. Two days after the initial mobilization event, the time variable is coded 2. Three days after the initial mobilization event, the time variable is coded 3, and so on. After too long a period, considering a subsequent event to be meaningfully related to a prior mobilization in the same city introduces noise and weakens all models. I consider movement memory to be five weeks, or 35 days.²²

The main measure of repression I used is arrests.²³ Working with the training data, I tested various ways of modeling arrest effects as discussed in detail below. Further, much of the repressive action against Black civil rights insurgents consisted of violent attacks by White mobs (McAdam 1982; Morris

²¹ `grp1==1 & thrt>0 & thrt!=. & (evnform==1 | evnform == 5 | evnform == 7 | evnform == 8).`

²² One question is how long the memory of past mobilization dynamics should be expected to significantly influence potential subsequent mobilization. If insurgents in a given city mobilize again in that same city the day after an initial event, it is reasonable to assume that they may know about and be influenced by memory of the earlier mobilization and any repressive action taken. But if the next mobilization in that city occurs more than 10 years later, it may not be reasonable to assume that insurgents are reacting to repression—or lack of repression—of the initial event. To handle this question, I sought to calibrate my time variable empirically. I ran my models using a wide variety of ranges for the time variable, from two days to 10 years (i.e., 3,650 days) to unlimited (i.e., as many days as it took to run through the data set). The results of the models were similar for all time period calibrations. But the very long memory models added noise, and the very short memory models cut off salient data, and so both yielded less statistically significant results. The most statistically significant calibrations used time period calibrations of about five weeks. So I used five weeks, or 35 days, for my models. These empirical indications are consistent with the findings of others. For example, in her influential study of the effects of repression on protest over time, Rasler (1996) found that repressive action by authorities had the greatest quelling effect on protest within the first 40 days, so she used a six-week period in her models, very similar to my empirically derived cut point.

²³ I used arrests to measure repression because arrests provided the cleanest, most parsimonious, measure available. Theoretically, arrests of protesters represent a systematic effort to regulate insurgent activity. Arrests not only interfere with an instance of insurgent action but act as a deterrent for future action and sometimes prevent insurgents from participating in future action (if they are in jail). Conversely, as Earl and Soule (2010) show, police presence is often ambiguous as different kinds of police action have different implications and, in some instances, police can even come to protect protesters. Further, with police violence it can be hard to distinguish between the rogue violence of individual officers vs. concerted regulatory violence—which hold different theoretical implications viz expected third-party response. Empirically, measures such as police use of force yielded similar effects as arrests but were not as statistically significant across categories of practice.

1984; Payne 1995). I capture this form of repression with a variable for White mob violence coded 1 if White mobs violently attacked Blacks in the city following the initial Black mobilization and 0 otherwise. To avoid overfitting, I favored parsimony rather than arbitrarily introducing supplemental variables.

To estimate the model coefficients, I used a binomial logistic regression model where a and b_k are coefficients analogous to ordinary least squares regression coefficients for explanatory variables X_k and the outcome variable is the natural log of the expected odds of a remobilization at time t and city i :

$$\ln\left(\frac{p_{it}}{1 - p_{it}}\right) = a + \sum_1^k b_k X_k. \quad (1)$$

For a robustness check, I also tried estimating model coefficients using King and Zeng's (2001) rare-events logistic regression. The results were almost identical in all cases with slightly improved statistical significance for several coefficients. While rare-event logistic regression is sometimes used for similar models (e.g., Andrews and Biggs 2006), I report the more conservative standard logistic regression consistent with common current practice (e.g., Biggs and Andrews 2015; Munsch 2015).²⁴ For ease of interpretation, I converted the resulting coefficients from log odds to standard odds for reporting in the tables below.

To test the hypotheses concerning various forms of insurgent mobilization, I begin by separating the data sets by form of practice and running similar models for each form. Then I turn to detailed modeling of arrest effects in the Civil Rights movement. Next, to assess hypotheses concerning the decline of the Civil Rights movement, I explore period effects and graphic analysis. Finally, I run final models on out-of-sample data.

FINDINGS

Preliminary Differences

To begin analyzing repressive effects on Black mobilization, I ran a basic model on three different sets of events: all Black mobilization; violent urban rebellions; and civil rights-type protest. The model assesses the hazard of

²⁴ Since I am using panel data with multiple observations per city, I also tried introducing a random disturbance term to the models (see Munsch 2015) to assess the importance of panel-level variance. I estimated these models with the xtlogit procedure in Stata, which uses adaptive Gauss-Hermite quadrature to calculate model coefficients. The resulting model estimations were not statistically distinguishable from the standard logistic regression models ($p \approx 0$).

subsequent remobilization in a given city-day following an earlier mobilization in that same city. For this preliminary test, to assess the effects of intense repression, I calculated the ratio of insurgents arrested and used “high arrests,” a dummy variable set to 1 for top quintile arrest ratios, as my main independent variable. Figure 3 depicts the results for the analysis of all Black mobilization events.

In figure 3, the y-axis represents the probability of remobilization in a city on a given day, ranging from 0 to just over .04. The x-axis represents days since the last mobilization in that city. Two lines are graphed. The first, indicated by a gray dashed line, represents the odds of remobilization if there were moderate or no arrests in the city previously. The second, indicated by a thick black line, shows a high ratio of previous arrests relative to the total number of insurgents mobilizing. What is striking about this graph is that the arc of the two lines appear basically indistinguishable. When considered as a whole, Black mobilization does not seem responsive to high levels of repression. The likelihood of remobilization appears similar whether or not previous insurgents are heavily repressed.

The numerical results are reported in table 1. Column 1 reports the results for the aggregate set of all Black mobilization events. This includes 36,137 panelized observations of city-days following a total of 1,572 aggregate Black mobilization events. The effect of a high level of arrests previously in a given city for all Black mobilization events when aggregated has an indeterminate effect. The model coefficient for the variable is close to 1, and the result

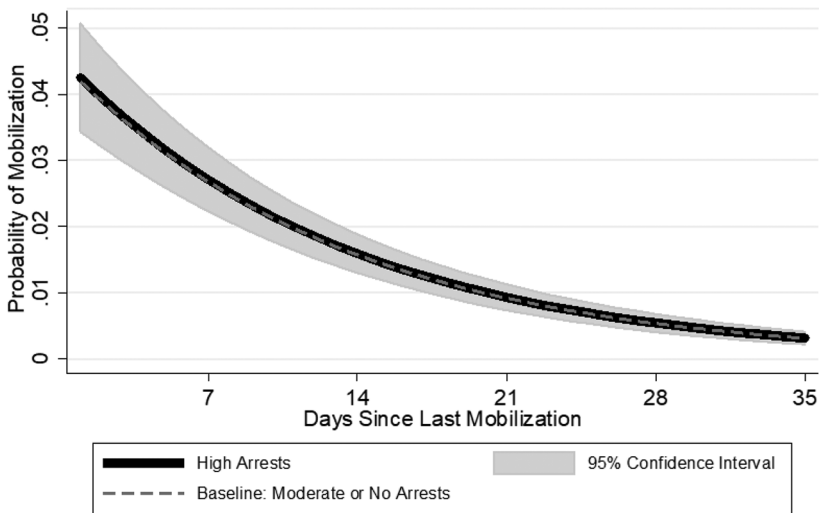


FIG. 3.—Probability of repeated black mobilization

Repression and Insurgent Practice

TABLE 1
ODDS OF REPEAT BLACK MOBILIZATION DISAGGREGATED

	ALL		VIOLENT URBAN REBELLION		CIVIL RIGHTS-TYPE PROTEST		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Time925***	.948***	.951***	.951***	.938***	.941***	.941***
High arrests	1.013	.507**	.500**	.495**	1.535***	1.652***	1.711***
Large?590*	.593*		1.202	1.186
Long?			1.083	1.079		1.066	1.054
Formal organization?			2.121***	2.237***		1.518***	1.471***
Police weapons attack?			1.419	1.468		1.938***	1.937***
Regional fixed effects?	No	No	No	Yes	No	No	Yes
Likelihood ratio χ^2	299	40	57	58	137	174	171
$P > \chi^2$0000	.0000	.0000	.0000	.0000	.0000	.0000
N (city-days at risk)	36,137	15,742	15,742	15,742	22,928	22,928	22,928

NOTE.—The N for all mobilization events is lower than the sum of the N 's for violent urban rebellion and civil rights-type protest even though violent urban rebellion and civil rights-type protest are mutually exclusive sets of events. This is because urban rebellion and protest occasionally occur in the same city at similar times, so some city-days are at risk for remobilization of both types.

* $P \leq .05$ (two-tailed).

** $P \leq .01$.

*** $P \leq .001$.

is not statistically significant ($P = .899$). I cannot reject the null hypothesis that high levels of repression have no effect on subsequent mobilization.

Next, I used the same model but applied it only to violent urban rebellions. Figure 4 depicts the results for the analysis of urban rebellion events. Here the overall rate of remobilization is lower. What is striking about this graph is that the arc of the two lines are easily distinguishable. Urban rebellions appear quite responsive to high levels of repression. The likelihood of remobilization appears much lower when insurgents are heavily repressed. Column 2 of table 1 reports results for urban rebellion events only. This includes 15,742 panelized observations of city-days following a total of 540 mobilization events. The analysis shows that, controlling for time since the last mobilization in that city, remobilization is about half as likely ($b = 0.507$) following high arrests, compared with periods following moderate or no arrests. The result is statistically significant ($P = .014$), so I reject the null hypothesis that high levels of repression have no effect on subsequent remobilization.

Now for preliminary comparison, I used the same model but applied it only to civil rights-type protest events. Figure 5 depicts the results. Here the overall rate of remobilization is a bit higher. Again, the arcs of the two lines are easily distinguishable, but the relationship is reversed. Civil

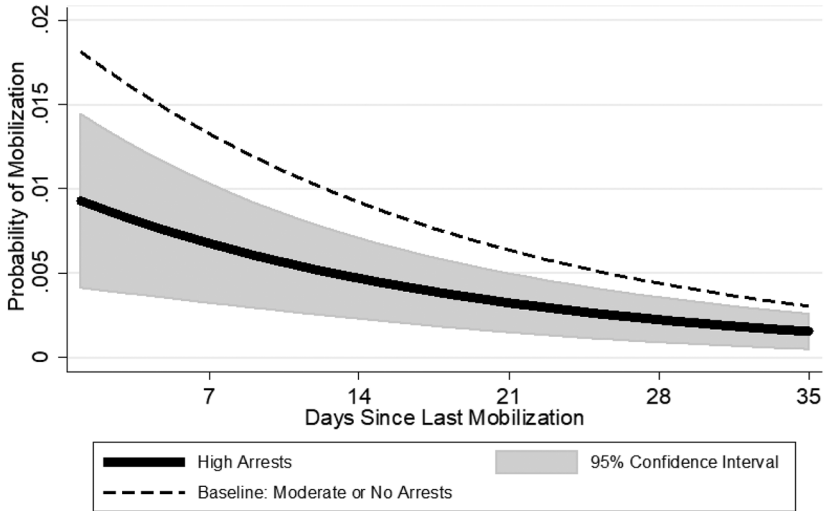


FIG. 4.—Probability of repeated violent urban rebellion

rights-type protest also appears quite responsive to high levels of repression, but the likelihood of remobilization is higher when civil rights-type protesters are heavily repressed. Column 5 of table 1, provides the statistics. This analysis includes 22,928 panelized observations of city-days following a total of 913 protest events. The analysis shows that, controlling for time since the last mobilization in that city, civil rights remobilization is about 50% more

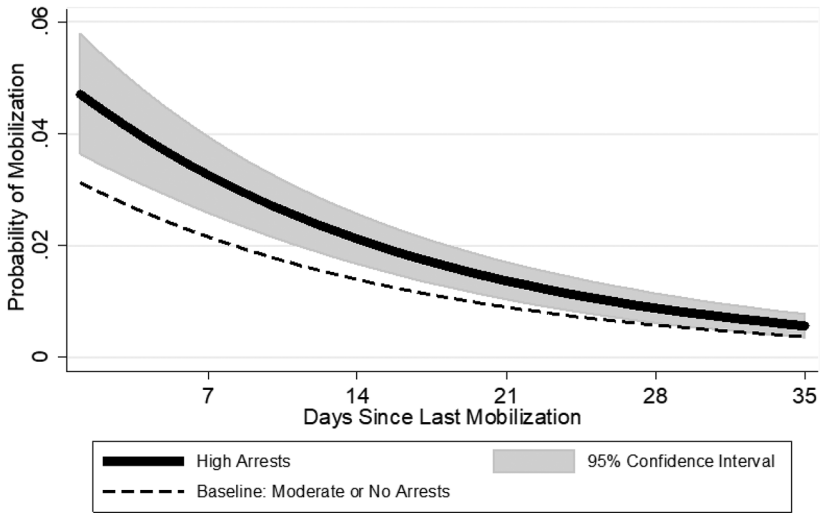


FIG. 5.—Probability of repeated civil rights-type protest

likely ($b = 1.535$) following high arrests, compared with periods following moderate or no arrests. The result is statistically significant ($P = .001$), so I reject the null hypothesis that high levels of repression have no effect on subsequent remobilization.²⁵

For all three sets of observations, there is strong, statistically significant clustering. The coefficients for the times variables in the range of 0.925 to 0.948 for the three models mean that remobilization is more likely soon after an earlier mobilization in a given city. For each subsequent day following a Black mobilization event in a given city, a remobilization becomes approximately 6% or 7% less likely. By the time five weeks (35 days) have passed, the odds of remobilization are approaching zero for all three sets of observations.²⁶

Before proceeding, I conducted a variety of robustness checks on these basic models, reported in table 1, columns 3 and 6. Control variables indicate whether preceding events are large (50 or more people attended) and longer than average, whether a formal organization helped initiate the event, and whether police used weapons against civilians. These controls do not substantively affect either the strength or the statistical significance of the effect of high levels of arrest on subsequent remobilization for either violent urban rebellion or civil rights–type protest.²⁷ Further, I fitted a regional fixed effects

²⁵ These differences are not attributable to differences between the cities in which the different types of practice occurred. There is too much geographic overlap. Of the 22,928 city-days at risk of remobilization of civil rights–type protest, a full 20,864—more than 90%—occur in cities that also experienced violent urban rebellion. Controlling for these categorical differences has little effect on the hazards. To assess, I generated a dummy variable from the violent urban rebellion data, coding the dummy variable 1 for every city in which there was a violent urban rebellion event and 0 otherwise. I then reran the original analysis of the civil rights–type protest hazards reported in table 1 but included this dummy. For the original analysis as reported in table 1 without the control, the coefficient for high arrests was 1.535, significant at the $P \leq .001$ level. Adding the control yielded a coefficient for high arrests of 1.549, also significant at the $P \leq .001$ level. Conversely, I generated a dummy variable from the civil rights–type protests data, coding the dummy variable as 1 for every city in which there was a civil rights–type protests event and 0 otherwise. I then reran the original analysis of the violent urban rebellion hazards reported in table 1 but included this dummy. For the original analysis as reported in table 1 without the control, the coefficient for high arrests was 0.507, significant at the $P \leq .01$ level. Adding the control yielded a coefficient for high arrests of 0.499, also significant at the $P \leq .01$ level.

²⁶ The mean ratio of arrests for civil rights–type protests, 0.11, is roughly comparable to that for violent urban rebellions, 0.13. Historically, this makes sense because nonviolent civil disobedience defying Jim Crow was illegal, and intentionally so. Think of the sit-ins. In many such events, nearly all the nonviolent protesters were arrested.

²⁷ While these controls do not alter the basic analysis, a couple of implications are interesting in their own right and worth mentioning. First, it is notable that when a formal organization initiated the preliminary event, remobilization was more likely, controlling for other factors. This finding held across *both* violent urban rebellion and civil rights–type protest and was somewhat stronger in the rare cases that formal organizations were involved in initiating violent urban rebellion. This finding suggests that the insurgent

model, reported in table 1, columns 4 and 7. For both violent urban rebellion and civil rights–type protest, the impact of the regional control was negligible as well.²⁸

Taken as a whole, these preliminary findings challenge hypotheses 1 and 2 and support hypothesis 3. While high levels of repression did decrease the likelihood of remobilization for violent urban rebellions, consistent with inverse-U thesis and hypothesis 1, the findings for Black insurgency overall were indeterminate. Contrary to inverse-U thesis and hypothesis 1, for civil rights–type protests, remobilization was significantly more likely following intense repression.

Similarly, the findings show the limits of classic political opportunity thesis and hypothesis 2. Structural opportunities for Black people as a group over the same time period cannot account for the divergent repressive effects across different forms of insurgent practice. Overall, these preliminary findings are consistent with hypothesis 3. Insurgent practice theory is better able to account for the range of variation.

The Southern Civil Rights Movement

The preliminary findings above are consistent with the idea that the effects of repressive action on mobilization depend on the form of insurgent practice repressed. But a fuller test requires disaggregation. The remainder of this article will further unpack the dynamics of repression and insurgent practice in the Southern Civil Rights movement.²⁹ Different kinds of insurgent practice

mobilization effects of repression might be separate and distinguishable from the developmental mobilization effects of initiatives sustained by existing organizations. This possibility warrants rigorous investigation beyond the scope of the current article. Second, it is notable that controlling for high levels of arrest, police weapon attacks on civilians had no statistically significant effect on remobilization following violent urban rebellion but a strong positive effect on remobilization following civil rights–type protest. This finding is consistent with insurgent practice theory.

²⁸ The regional group dummy used for the fixed effects model indicated whether an event was held in the South, or not. Violent urban rebellion: $N = 15,742$; frequency yes = 4,903; frequency no = 10,839. Civil rights–type protest: $N = 22,928$; frequency yes = 10,888; frequency no = 12,040.

²⁹ This exploration required analyzing specifically civil rights actions. As no explicit coding for the Southern Civil Rights movement was provided in the data set, I employed the following criteria, available in the data set: (1) Was the event explicitly initiated by Black people or their allies in the interests of Black people? (2) Was it a protest event as opposed to an ethnic conflict, that is, was action taken “on behalf of [Black people], expressing grievances related to discrimination or racial policy” instead of constituting a confrontation “between two or more ethnic populations” (Olzak and West 2007)? I specifically exclude riots, ethnic vandalism, and nonprotest ethnic conflicts that occur in workplaces. (3) Was the event non-violent? (4) Did the event occur in the South? Like Olzak and West (2007), I follow the U.S. Census Bureau and include as the South the 123 Southern standard metropolitan statistical areas located in Alabama, Arkansas, Delaware, District of Columbia, Georgia, Florida,

are repressed differently in different places. To capture the best measure of repression specifically for civil rights mobilization in the South, I assess goodness of fit for models using four different measures of arrests: first, the dummy variable for high arrests used previously; second, a continuous variable for the ratio of arrests to participants in the previous mobilization; third, the squared polynomial for that ratio of arrests; and fourth, a dummy variable coded 1 if any of the recent participants were arrested and 0 if none were arrested. Because White mob violence constituted an important form of repression against civil rights insurgency (McAdam 1982; Morris 1984; Payne 1995), I add a dummy variable to the models indicating whether or not White violent mobs attacked Black people in the city at risk following the initial civil rights mobilization and prior to the day analyzed.

All four models assess the risk of one of the 150 civil rights remobilizations occurring in the 11,001 city-days at risk within 35 days following an earlier civil rights mobilization in a given city. The results are reported in table 2.

White mob violence is a strong positive predictor of remobilization with similar variable coefficients across the four models (ranging from 1.876 to 1.962), all statistically significant at the $P \leq .01$ level. With White mob violence included, the four measures for arrests also all positively predict civil rights remobilization, but at different strengths and levels of significance. Model 1 reports the results for the analysis using the same dummy variable for high arrests used in the preliminary analyses. The coefficient is similar to the coefficient for the same variable in the model analyzing all civil rights-type mobilization in table 1 ($b = 1.463$ compared to $b = 1.535$). This coefficient suggests that, controlling for temporal clustering and White mob violence, subsequent civil rights remobilization is almost half again as likely on a given city-day if there was a high level of arrests in the earlier mobilization. The coefficient is significant ($P = .05$).

Model 2 reports the results for the analysis using the continuous variable for arrest ratio with 1 meaning all civil rights insurgents in the early action were arrested, 0 indicating that none were, and numbers in between indicating the proportion of insurgents arrested. The variable coefficient here is similar, but a bit stronger: $b = 2.042$ ($P = .008$). This coefficient suggests that generally the higher portion of civil rights insurgents arrested in an earlier mobilization, the more likely a subsequent remobilization. Controlling for temporal clustering and White mob violence, subsequent civil rights remobilization is more than twice as likely on a given city-day if all insurgents were arrested in an earlier event compared with if none were.

Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Texas, Tennessee, Virginia, and West Virginia. While other definitions are possible, the merits of any potential revision are debatable.

TABLE 2
ODDS OF REPEAT CIVIL RIGHTS MOBILIZATION, TRAINING DATA

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	1965-92
High arrests	1.463*							
Arrest ratio		2.042**	1.764					
Arrest ratio ²		1.184						
Arrests?		1.851***		1.819***			1.650**	2.829**
1965-92?					2.033**	.510***	.628*	
White mob violence	1.876**	1.959**	1.962**	1.920**			1.650*	3.365
Large?					.870			
Long?					1.197			
Formal organization?					1.418			
Police weapons attack?					1.170			
Time	.922***	.922***	.922***	.924***	.925***	.922***	.924***	.938***
Likelihood ratio χ^2	91	95	94	101	108	96	107	27
$P > \chi^2$.0000	.0000	.0000	.0000	.0000	.0000	.0000	.0000
ROC	.72	.72	.72	.73	.73	.72	.74	.72
No. of repeat events	150	150	150	150	150	150	150	41
N (city-days at risk)	11,001	11,001	11,001	11,001	11,001	11,001	11,001	4,802

* $P \leq .05$ (two-tailed).

** $P \leq .01$.

*** $P \leq .001$.

Table 2, model 3 reports the results for the analysis again using the continuous variable for arrest ratio but adding a polynomial term for the arrest ratio squared. This allows testing of a potential nonmonotonic relationship between arrests and subsequent remobilization (as suggested by inverse-U thesis). The variable coefficient for arrest ratio here is similar, but not statistically significant ($b = 1.764, P = .577$). The variable coefficient for arrest ratio squared is still positive, but closer to identity, and not statistically significant ($b = 1.184, P = .881$). Since both coefficients are positive, and not statistically significant, I cannot reject the null hypothesis that the relationship between the intensity of arrests and subsequent remobilization is monotonic.³⁰

Model 4 reports the results for the analysis using the dummy variable recording whether or not any of the recent participants in civil rights insurgency in a given city were arrested. Again, the variable coefficient here is similar, but even stronger: $b = 1.851 (P = .000)$. This coefficient suggests that, controlling for temporal clustering and White mob violence, if civil rights insurgents were arrested in an earlier mobilization, the odds of a subsequent remobilization within 35 days increase more than 85%.

Taken as a whole, the models lead me to reject hypothesis 1 derived from inverse-U thesis: for nonviolent Black civil rights mobilization, high levels of repression do not decrease the likelihood of subsequent remobilization. This finding appears robust across measures of repression. The coefficients for the different measures of arrests, and for White mob violence, are all fairly similar and positive across models. Further, in all four models, higher levels of repression correspond to higher odds of subsequent remobilization in a given city. For all four models, when civil rights insurgents face both arrests and White mob violence, the likelihood of remobilization exceeds the likelihood of remobilization following one form of repression alone.

I select model 4 as my final model for further testing. While the four models are similar, model 4 appears to better capture repressive effects on subsequent civil rights mobilization. While the coefficients for time and White mob violence variables are significant at similar levels across the four models, the variable for arrests is most significant in model 4. Similarly, the likelihood ratio chi-square statistic is highest for model 4. For an additional assessment of goodness of fit across the four models, I calculated the area under the receiver operating characteristic (ROC) curve for each model. The ROC statistic measures each model's ability to predict whether or not a given city will

³⁰ The statistically significant coefficient for arrest ratio in model 2 is consistent with a potentially monotonic relationship. I also tried using an ordered categorical variable for arrest ratio quintile. Still controlling for White mob violence and temporal clustering as in the other models, this also showed a positive and statistically significant effect with $b = 1.129$ (per quintile) and $P = .009$. Those results are also consistent with a positive monotonic effect of the intensity of arrests on remobilization.

experience a subsequent remobilization on a specified day,³¹ and it ranges from .5 (no predictive power) to 1 (perfect prediction). Model 4's ROC statistics are slightly higher than the other three models'.

Before proceeding, I conducted a variety of robustness checks on the selected model. Model 4 is robust across controls. Model 5 in table 2 displays control variables indicating whether events are large (50 or more people attended), whether events are longer than average, whether a formal organization helped initiate the event, and whether police used weapons against civilians. The coefficients for these controls are not statistically significant and do not substantively alter the analysis.

To assess hypotheses 4a and 4b and whether repression continued to increase the odds of subsequent remobilization following the decline in the level of repression of civil rights insurgency from 1965 onward, I used a dummy variable for time period. This variable is coded 1 for events that occurred between 1965 and 1992 and 0 for earlier events. Table 2, model 6, reports the results of this period variable alone in predicting remobilization. The civil rights insurgency was more active in the earlier period, so it is unsurprising that the earlier period corresponds to a greater likelihood of remobilization. Specifically, remobilization was about half as likely in the later period: $b = 0.510$ ($P = .000$). Model 7 adds the period control to model 4. Coefficients for both arrests ($b = 1.650$) and White mob violence ($b = 1.165$) remain strongly positive and largely unchanged when controlling for period. In other words, controlling for White mob violence, civil rights remobilization is 65% more likely following arrests. And controlling for arrests, civil rights remobilization is 65% more likely following mob violence. Here the arrests variable coefficient is significant at the $P \leq .01$ level, and the coefficients for the mob and period variables are significant at the $P \leq .05$ level.

To specifically check repressive effects during the later period, I also applied model 4 to a subset of the data from 1965–92 as reported in table 2. The findings support the interpretation of results from model 7 that repression continues to increase the likelihood of remobilization with the qualification that the number of remobilizations in the training data for this later period is small—only 41 remobilizations, only two of which follow White mob violence. The coefficient for arrests is significant ($P = .002$), and the coefficient for White mob violence is not significant ($P = .105$).

Together, these findings provisionally suggest that—controlling for period effects—intense repression involving both arrests and White mob violence compounds the likelihood of remobilization. Contrary to hypothesis 4a and the expectations of both the inverse-U and political opportunity theses,

³¹ Or more accurately “retrodict,” since the events already occurred when the analysis was conducted and the models were built analyzing these data.

but consistent with hypothesis 4b and insurgent practice theory—the likelihood of civil rights remobilization increases in the face of intense repression *regardless of the period*.

Graphic Analysis

Before moving on to the out-of-sample tests, I conduct a graphic analysis of the hypothetical relationship between mobilization and repression presented in figure 2 above. To create an appropriate scale for graphing, I use effects proportional scaling (Treiman and Terrell 1975; Ross and Mirowsky 1979), regressing the total number of protestors per year on the number arrested, and the number of White violent events, for the key period of mobilization, 1954–64.³² I then graph the entire period 1954–92 using this scale. The results are reported in figure 6.

In various regards, the similarity between the hypothesized relationship in figure 2 and the results in figure 6 are striking. Levels of repression quickly increase through the late 1950s and early 1960s in tandem with the level of mobilization. Then, as legal segregation is dismantled nationally starting in earnest in 1964, the rate of repression quickly declines. For several years, through the mid-1960s, the level of civil rights mobilization persists as activists continue employing practices similar to those that had proven so effective in the early 1960s. But then, following the rate of repression with a lag, in the late 1960s and early 1970s, the rate of civil rights action declines precipitously.

Several unexpected differences are also worth noting. First, the rate of repression swells much more quickly in the late 1950s than the rate of mobilization, and it takes the movement until about 1959 to catch up. It turns out that most of the repression of civil rights activists in the late 1950s consisted of White mob violence, with relatively few arrests. It was only as the movement and its visibility grew, into the 1960s, that White mob violence was curbed, and arrests became the main means for repressing civil rights activists. This is an interesting preliminary historical finding that has not yet been discussed in the literature to my knowledge and is worth further investigation. Second, the rate of repression of civil rights activists does not decline as quickly as expected after 1964. Full decline takes about three years instead of one. And similarly, the decline of the movement itself takes longer than expected, settling down in about 1972 instead of 1968.³³

³² Specifically the coefficients are 10.6 for arrests, 486 for White violent mobs, and a constant term of $-3,462$. As negative estimates of the annual level of arrests and White mob violence are not meaningful, estimates are truncated at zero.

³³ It is also interesting that the graph shows a couple brief periods of remobilization clustered around 1980 and 1989, although these are not accompanied by significant levels of repression.

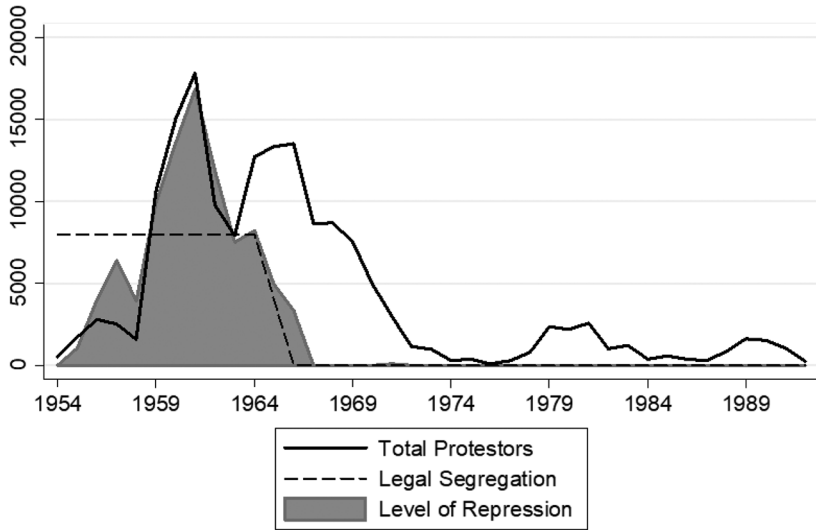


FIG. 6.—Mobilization vs. level of repression

A tabulation of the rate of arrests of participants in the Southern Civil Rights movement by period supports the assessment that the level of repression of civil rights practice declines. Over the period 1954–64, that is, roughly from the beginning of the Southern Civil Rights movement through the passage of the Civil Rights Act, civil rights insurgents face a high level of arrests, 61 per 1,000 participants. In the period of transition, covering the sweeping abolition of legal segregation and the decline of the Civil Rights movement, the rate of arrests decreases appreciably, to 34 arrests per 1,000 participants. In the period after, from 1972 to 1992 when the data set coverage ends, participants in civil rights–type practices face little repression with only one arrest per 1,000 participants.

Both the graphic analysis in figure 6 and the declining arrest ratios support hypothesis 5. Declining levels of repression preceded the decline of civil rights mobilization. Conversely, these findings appear to contradict all theories that suggest repression caused the decline of the Civil Rights movement, including the expectation, based on classical political opportunity thesis, that Black insurgency abated in the late 1960s because of the closure of political opportunity for Blacks and the associated increase in the intensity of repression (see, e.g., McAdam 1983, pp. 751–52). The findings here confirm empirical expectations of the practice-centered hypothetical explanation of the demise of the Civil Rights movement. As the Civil Rights movement succeeded, Jim Crow was dismantled, and local authorities gradually stopped trying to protect formal segregation. The rate of repression of civil rights insurgent practices declined, appropriate targets for civil rights action became

increasingly scarce, and civil rights practices no longer provided an effective means of challenging persistent forms of racism (Bloom 2014; Bloom and Martin 2016; see also Robnett 2002). If this hypothetical explanation of the decline of the Civil Rights movement is correct, it explains why the rate of repression of civil rights action first declined and then was followed by the declining rate of mobilization.

Out-of-Sample Test

Finally, I apply my final models, models 4 and 7, to the holdout data set without adjustment. The results are reported in table 3. The analysis for model 4 covers 23,176 city-days at risk for remobilization within 35 days after an earlier mobilization in a given city. Six hundred eighty-eight remobilization events occur within these city-days, and it is those remobilizations the model seeks to explain. The control variable shows significant clustering of subsequent remobilizations close to the earlier event, decreasing more than 17% for each day following an earlier mobilization ($P = .000$).

The odds of subsequent remobilization for civil rights action increase more than 85% when earlier civil rights insurgents are arrested ($P = .000$), controlling for White mob violence. The odds of subsequent remobilization also increase more than 85% following violent White mob attacks ($P = .000$), controlling for arrests. These findings support the core hypotheses derived from insurgent practice theory. The odds of subsequent remobilization of civil rights insurgent practice were higher following repression. It was especially likely following intense repression including both arrests and White mob violence. Results from the out-of-sample test provide assurance that the findings reflect real relationships in the data and are not an artifact of model overfitting (see app. C).

TABLE 3
OUT-OF-SAMPLE TEST USING FINAL MODELS

	(4)	(7)	1965–92
Time826***	.826***	.830***
Arrests?	1.874***	1.668***	1.479**
White mob violence	1.873***	1.961***	3.721***
1965–92?661***	
Likelihood ratio $\chi^2(3)$	1,233	1,257	529
$P > \chi^2$0000	.0000	.0000
ROC85	.86	.86
No. of repeat events	688	688	264
N (city-days at risk)	23,176	23,176	12,580

* $P \leq .05$ (two-tailed).
 ** $P \leq .01$.
 *** $P \leq .001$.

Applying model 7 to the out-of-sample data provides a stronger assessment of hypotheses 4a and 4b. Here, with more than four times as many repeat events (688), all of the model coefficients are significant at the $P = .000$ level, and substantively, the results are much the same. Controlling for period effects (remobilization is only 66% as likely in the later period), both arrests ($b = 1.668$) and White mob violence ($b = 1.961$) strongly predict remobilization. Further, restricting analysis to data from the later period confirms that repression continues to strongly predict remobilization in the years after the civil rights heyday. Applying model 4 to the data from only the 1965–92 period supports the assessment that both arrests and White mob violence continue to predict mobilization in this later period. Controlling for temporal clustering and arrests, the effect of White mob violence in this period is similar to the effect for the overall period, but stronger: $b = 3.721$ ($P = .000$). Controlling for temporal clustering and White mob violence, the effect of arrests in this later period also appears similar to the effect for the overall period, but weaker: $b = 1.479$ ($P = .005$).

Taken together, these findings provide strong support for hypothesis 4b and lead me to reject hypothesis 4a. Demobilization of the Civil Rights movement cannot be explained by the renewed efficacy of repression. While overall levels of repression decline in the late 1960s and beyond, specific incidents of repression—and especially intense repression combining both arrests and White mob violence—continue to strongly correlate with remobilization. This finding supports the interpretation—drawn from insurgent practice theory—that Black insurgents turned away from civil rights practices not out of fear of repression but because Jim Crow targets became scarce and civil rights insurgent practices were no longer providing the leverage they once did.

These findings from analysis of the out-of-sample data are graphed in figure 7. Overall, because the out-of-sample data set is larger and contains many more events, the probability of remobilization is higher here than in the test data. The effects of repression on remobilization are quite stark here, with a low rate of remobilization for civil rights events that are not repressed and a much higher rate for events that are heavily repressed—with both arrests and White mob violence. The line for arrests and White mob violence in the later period—1965–92—is even higher, illustrating that intense repression continues to predict remobilization after the heyday of the Civil Rights movement.

Taken as a whole, this diagram illustrates the limits of both inverse-U thesis and political opportunity thesis and supports the expectations of practice-centered theory. Contrary to inverse U and hypothesis 1, for civil rights action, intense repression appears to increase the likelihood of remobilization. Contrary to political opportunity accounts and hypothesis 4a, this remobilization effect appears to continue well past the heyday of the Civil Rights movement and the purported closure of political opportunity for Black people. Conversely, the graph is consistent with insurgent practice theory and

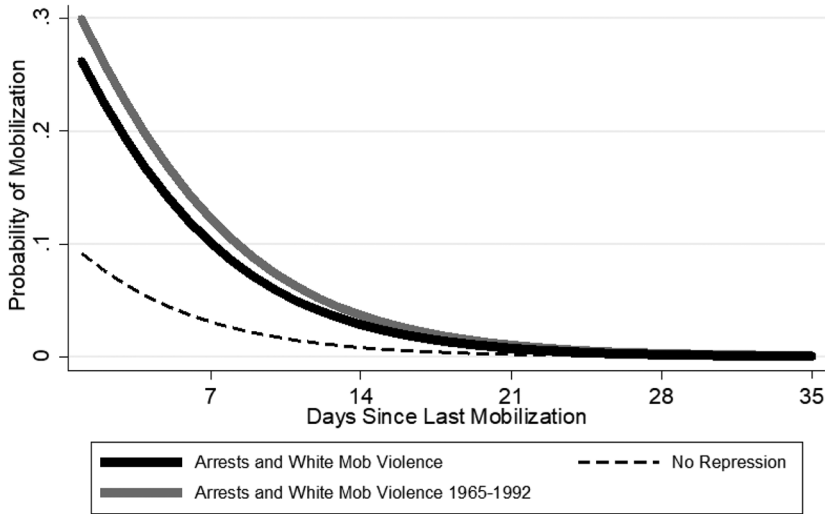


FIG. 7.—Out-of-sample test. All lines are based on out-of-sample data. The lines for “Arrests and White Mob Violence” and for “No Repression” are both calculated from model 4 as reported in table 3. The line for the period effect, “Arrests and White Mob Violence 1965–92,” is calculated from model 4 applied to the 1965–92 period as reported in table 3. For confidence intervals, see figs. C1 and C2.

hypotheses 3 and 4b. Intense repression of civil rights action corresponds with increased likelihood of remobilization, even after the heyday of the Civil Rights movement.

DISCUSSION AND CONCLUSIONS

Overall, the findings challenge the inverse-U thesis. And while in some circumstances the expectations of the political opportunity thesis hold, in others they don’t. The effort to reconsider repressive effects with reference to *practice* enable a better accounting for the range of empirical evidence. All the findings are consistent with the empirical implications of insurgent practice theory.

High levels of repression of Black urban rebellions *decreased* the likelihood of remobilization. But under the same postwar U.S. regime, intense repression of different forms of Black insurgent action had different effects. Contrary to the inverse-U thesis (Gurr 1968, 1969; Muller 1985) and hypothesis 1, hazard analysis including out-of-sample testing of final models showed that high levels of repression of civil rights practices *increased* the likelihood of remobilization. These findings support the generality of Morris’s (1993) conclusions beyond Birmingham. Sustained mobilization depended on the capacity of insurgents’ practices to generate a social crisis in a way that was

difficult to repress. Third-party support was contingent on what the insurgents did. And the violent brutality of repression was not independently determinant. While the analysis above shows that violent repression did have an escalatory effect on civil rights mobilization, even controlling for mob violence, arrests did as well. The crux of the escalation process was the form of insurgent practice, not the violence of repressive response.

The political opportunity thesis (Tilly 1978; McAdam 1982; Tarrow 1994) and hypothesis 2 correctly expected repression of civil rights practices to be escalatory. But from the classic political opportunity perspective, it is hard to explain why other forms of mobilization by Black insurgents were vulnerable to repression during the same period of “expanding political opportunity” for Blacks. Further challenging the expectations of the classic political opportunity thesis, period tests showed that the escalatory effects of repression on civil rights practice increased *after* the heyday of the Civil Rights movement following the purported contraction of political opportunity for Black people. Remobilization of civil rights action is even more likely following intense repression in the 1965–92 period. These findings—also confirmed by out-of-sample tests—are consistent with hypotheses 3 and 4b and the empirical expectations of insurgent practice theory.

If repression never worked to quell civil rights insurgent practice, this poses a puzzle about why Black insurgents largely abandoned civil rights practices and turned toward other forms of resistance following the great successes of the Civil Rights movement in the early 1960s. Consistent with insurgent practice theory, one explanation previously suggested (Robnett 2002; Bloom 2014; Bloom and Martin 2016) is that as Jim Crow was dismantled, there were fewer targets where civil rights practices could be effectively applied. Implications of this perspective are depicted in the hypothetical graph in figure 2 and articulated in hypothesis 5. Results of tabular and graphic analyses are consistent with these empirical expectations, with some important historical qualifications (and discoveries) regarding the exact timing of these changes and shifting balances in the character of repression.

Implications for Theories of Repressive Effects

The inverse-U thesis sought to generalize repressive effects on mobilization with regard to the intensity of repressive action (Gurr 1968, 1969; Muller 1985). But the effects of similar intensities of repression cannot be generalized. As shown above, the effects of repression on mobilization depend not only on the intensity of repression but can vary by the practice repressed given similar intensities of repression, even within the same historical context.

Political process theory took a more context-specific approach. The core political opportunity thesis sought to generalize the structural conditions favorable to members of a social group under which repressive action against

them would backfire and enable insurgent mobilization (McAdam 1982, esp. pp. 43, 174–79). But while this approach accounted for some effects, the effects of such conditions cannot be consistently generalized. As shown above, repressive effects can vary by the practice repressed even for members of the same group within the same structural situation. Structural conditions do not independently determine the effects of repression on mobilization by members of a social group.

Repression does not have similar effects in all situations—or even the same situation—because what insurgents do matters. Just because we call “repression” by the same name, there is no reason to assume similar effects on different practices. As a range of recent studies have shown, mobilization dynamics depend on the efficacy of specific practices in a given political context (e.g., Amenta 2006; Evans and Kay 2008; Taylor et al. 2009; Jansen 2011, 2016, 2017; Bloom 2015; Bloom and Martin 2016; Gastón 2017; Kay and Evans 2018), and similar mobilization repertoires yield divergent outcomes in different situations (Bernstein 1997; A. Martin 2008; Walker et al. 2008; I. Martin 2010; McCammon 2012; Wang and Soule 2016). A range of recent studies of repression have begun to account for movement practices (Earl, Soule, and McCarthy 2003; Carey 2006; Earl and Soule 2006; Chang and Kim 2007; Davenport et al. 2008; Ayoub 2010; Chang and Vitale 2013; Moss 2014, 2016; Davenport 2015; Honari 2018).

Building on this scholarship, this study finds that in the postwar Black Liberation Struggle, repressive effects varied significantly by practice repressed even for similar intensities of repression against members of the same social group under the same regime during the same historical period. Conceptualizing repression as targeting a specific form of political practice made it possible to account for these divergent effects.

First, the effects of repression on mobilization depend on the practice repressed and the specific context in which it is repressed, as found above and in several previous studies (Opp and Roehl 1990; Almeida 2008; Bloom 2015; Bloom and Martin 2016). Viewing repression as *regulatory* makes sense of why. Authorities take repressive action to sanction the use of specific practices in a given historical situation. Both the motivations and effects of social action follow a practical and situational logic rather than a categorical one (Bourdieu 1990). Contrary to any invariant or categorical model, explaining repressive effects requires historically specific knowledge about the practice repressed and the context in which it was repressed.

Second, beyond any ideographic analysis, it is the “politics” of the practice that matter, and the politics of the practice matter in generalizable ways. Competing constituencies comprising or influencing the state have distinct institutionalized interests in the practices sanctioned by regulatory repressive action (Gramsci 1971; Mann 1993). Repressive action forces competing constituencies to take a stand on the practices they sanction, either explicitly

or implicitly through their silence. It is this political reception of repressive action of a historically specific insurgent practice that determines the effects of that repression on subsequent mobilization. I believe that is generally true—across time and place.

The range of empirical findings here all support the proposition, developed in two earlier qualitative case studies (Bloom 2015; and Bloom and Martin 2016), that in a given historical context—when a specific form of insurgent practice garners broad opposition to repressive action—repression will increase the likelihood of mobilization. The current study is the first large-*N* test of this proposition.

It is important to note that while this study seeks to improve upon classical approaches, it owes much to the political process tradition. McAdam (1982) and Morris (1984) in particular advance complex and multicausal narratives of the Black insurgency in which repression is only one element; this study builds upon these narratives fundamentally and at times they anticipate my focus on practice.

Distinguishing between Insurgent Practices in Theory and Application

How much of a difference between insurgent practices makes a difference in repressive effects? Theoretically, the argument is that states regulate practice. Insurgent practices that draw broad allied support in opposition to their repression are hard to repress. Repressive action by authorities against such insurgent practices tends to lead to further participation. In some historical circumstances, small differences between otherwise similar practices—such as a slight variation in framing, or target, or tactic—can make the difference between whether or not such allied support is forthcoming. For example, in Birmingham, Alabama, in 1963, in the face of overwhelming repression, Martin Luther King Jr. and the Southern Christian Leadership Conference leaders were struggling to sustain active support for nonviolent protests calling for citizenship rights. But on May 2, in what has come to be known as the Children's Crusade, civil rights activists in Birmingham encouraged schoolchildren to walk out of school and join in nonviolent protest, marching to city hall to call for citizenship rights. Many did. Local police officers, under the direction of commissioner of public safety Bull Connor, attacked the children with dogs and high-pressure fire hoses. Twelve hundred children were arrested that day. Images of these events generated widespread outrage, drawing broad opposition, and proved a key turning point in the development of the movement.

Similarly, a slight shift in the political context over time, or across geographic location, can also make a big difference in the reception of a specific practice. For example, the first sit-ins of the Civil Rights movement were not

held on February 1, 1960, in Greensboro, North Carolina. One earlier sit-in, in nearby Durham, was led by Reverend Douglas Moore on June 23, 1957. The activists were arrested, and the case was tied up in court. At the time, North Carolina was not yet ready for such insurgent practices. There was little vocal opposition to the arrests, and no local remobilizations in the following weeks. It was not until February 1960, when active opposition to the enforcement of Jim Crow had grown, that the sit-ins became a powerful practice—hard to repress in many cities and thus widely emulated. It would still take time before the sit-ins would reach Mississippi, where in early 1960, activists could expect to be killed with impunity for sitting in.

As Gramsci (1971, pp. 233–39) argued, how much a small difference in insurgent practice matters depends on the political situation. At one extreme, in some times and places, such as Tsarist Russia in 1905, the state is rigid and isolated and challenges are widely seen as shared by the many opponents of the state. Such a large range of interests are aligned against state rule that any challenge garners broad support and the question is just whether or not the state will stand. But in most situations in the West, “a sturdy structure of civil society” protects the authority of the state. Insurgent actions that challenge political authorities are part of what Gramsci called a “war of position.” Where civil society is strong, small differences in the character of insurgent practices can affect the response of various third parties, whether broad opposition to repression is forthcoming, and thus whether repression of such practices leads to remobilization.

At the level of application, while small variations in a single dimension of insurgent practice or small differences in time or place can shape repressive effects, hazards of remobilization across broad aggregates of practice across many cities and many years can also be assessed as they are in this study. This is possible because—despite vast variations in repressive response to the range of specific variations in practice, time, and place—there is enough consistency across aggregates that notable categorical differences emerge. This is the case for civil rights practice and violent urban rebellion in this study.

It should also be noted that while quantitative methods can be used to analyze these theorized dynamics, the theory is fundamentally historical. It thereby requires fundamentally historical applications. There are no values of the variables claim, tactic, and target—or combinations thereof—that could be expected to respond similarly to repressive action by authorities across time and place. Effective insurgent practices cannot be defined, a priori, such that they could be recognized as such before they are enacted in a given situation.

Therefore the logic of inquiry in this article is not to establish which insurgent practices will resist repression across time and place. That is not possible. The logic of inquiry is to interpret the historical situation analyzed using secondary references and some polling data, to apply the theory, and then to

assess observable implications of this application. To usefully apply the theory in other situations requires a similarly historical logic of application. This means interpreting a historical situation, identifying an insurgent practice, and estimating the situational extent of opposition to repression. How can such an application be assessed? If the theoretical propositions advanced here hold, then in any given historical situation, where there is broad opposition to repression, the identified insurgent practice should be more widely adopted following intense repressive action taken against it. That is, broader opposition to the regulatory action of the state ought to encourage more participation in direct challenge in the form of the identified insurgent practice. If not, then there is something wrong with the theory or its application.

Limitations

The conclusions I've drawn in this article are limited by several aspects of the study and need to be qualified along three dimensions. First, the data used for these analyses are far from perfect. The study design sought to manage such limitations: assessing remobilization rates for actions already covered assured a fair comparison across types of practice, redressing systematic selection bias (McCarthy et al. 1996; Oliver and Maney 2000; Earl et al. 2004; Myers and Caniglia 2004) in the Dynamics of Collective Action or the training data set that could substantially shape the findings. Nevertheless, newspaper data do not provide comprehensive empirical coverage, so readers should keep in mind that it is possible that other kinds of data treating different kinds of events could reveal different tendencies. Also, while the data set allowed testing key empirical implications of the theories considered, additional variables—such as good measures of the Jim Crow character of targets, or the level of disruption generated by specific insurgent actions, or the character of allied response to specific repressive actions³⁴—would have allowed more extensive testing. It remains for other studies to collect data measuring such variables in the future.³⁵

³⁴ It would be illuminating to obtain more exact measures of the opposition to repression relevant to the remobilization events I analyze and to assess precisely additional hypotheses concerning the theorized role of this opposition in the mobilization process. To do so quantitatively on an event-wise basis across the scope of the Black Liberation Struggle would require detailed data on the responses of various constituencies—including Black churches and local political organizations and representatives of the federal government—to the specific insurgent events and repressive acts included in the event catalog data analyzed.

³⁵ More extensive and refined data also would also allow more granular disaggregation of insurgent practices. While it would be valuable to assess variations of repressive effects across disaggregated forms of Black insurgent practice—such as the sit-ins, the freedom rides, the voting rights campaigns, the Black Panther Party defense of offices or breakfast programs, looting vs. burning, etc.—the Dynamics of Collective Action data were not comprehensive enough at a granular enough level to allow intelligible assessments. Such analysis remains for future study.

Second, the study assessed only a few theoretically important causal dynamics involved in mobilization and demobilization. Out-of-sample testing addressed concerns about model overfitting, increasing confidence in the analysis. But it also required parsimony, limiting the range of processes studied.

Third and most important, given the limited *empirical* scope of analysis, the theoretical implications of the study are necessarily limited. How would the theoretical conclusions hold up under very different conditions? For example, none of the events analyzed took place under authoritarian regimes. And while a few insurgents were killed, in the events studied here, intense repression did not include mass killings of insurgents. Hypothetically, I suspect that even mass-murdering authoritarian states are susceptible to the competing interests of varied constituent factions. Thus I would hope that a theory of insurgent practice—derived in part from Gramsci’s (1971) study of insurgent dynamics and the “war of position” in the rise of Fascism—can help illuminate repressive effects under a wide range of conditions. But that is purely speculative. This study does nothing to test repressive effects beyond the scope of postwar the Black Liberation Struggle in the United States. Future research is needed to see how well these theoretical conclusions hold up for different movements, in other times and places.

In short, for a variety of reasons, the analysis offered in this article should not be considered to provide “definitive and final proof” of my theory but more a “proof of concept” along the path to greater understanding. From my perspective, the ultimate arbiter of scientific rigor is the contribution to real-time predictive capacity that a theory and method of analysis provide. The historicist logic of inquiry presented here fundamentally acknowledges the impossibility of predicting repressive effects based on a priori categorization of types of practice or kinds of repression. My hope is that in the long run, this approach does not diminish, but instead promises to increase, the predictive capacity of the theory. If the argument advanced here is correct, then precise historical interpretation and instantiation of insurgent practice theory in a given historical situation should enable accurate short-term predictions about the effects of repression on a specific insurgent practice. Eventually, such prediction should be possible in real time. Still far shy of achieving that benchmark, all my limited analysis above can do is provide a plausible assessment of some of the empirical implications of the theory in the hope of moving closer.

Black Lives Matter

Following the May 25, 2020, killing of George Floyd by Officer Derek Chauvin in Minneapolis—and on the heels of the police killings of Breonna Taylor in Louisville and Tony McDade in Tallahassee—the Black Liberation Struggle mobilized on a scale unprecedented in the United States since the 1960s.

Black Lives Matter protests in late May and early June met with harsh repression. The repressive police actions, in turn, met with vast opposition and allied support for the protesters. Preliminary analysis suggests the dynamics of repression and insurgent practice in this recent wave align closely with the dynamics analyzed above. Widespread allied resistance to the repression of Black Lives Matter protesters encouraged unprecedented escalation of the insurgency. Authorities rapidly moderated their repression, which led to de-escalation. See appendix F for preliminary analysis and hypotheses based on insurgent practice theory and concerning movement trajectories up to the November 2020 elections.

Explaining Process by Generalizing the Dynamics of Practice

Since the fall of structuralism (Giddens 1984; Bourdieu 1990; Sewell 1992), the study of movements and politics has confronted a theoretical crossroads (Gamson and Meyer 1996; McAdam et al. 2001; Goodwin and Jasper 2004; Clemens 2005, 2007, p. 544; Armstrong and Bernstein 2008). On the one hand, empirical analysis has grown ever more sophisticated, revealing the limits of invariant theoretical models. On the other hand, ideographic—that is, historically unique—approaches cannot meaningfully compare and generalize across cases, losing capacity to cumulate theoretical knowledge (Tilly 1995, 2008; Clemens 2005, 2007). For a large catalog of events in the context of the postwar Black Liberation Struggle in the United States, the practice-centered approach described here transcended the limits of both invariant models and ideographic analysis for explaining effects of repression on mobilization.

As Tilly has argued, invariant models of categorical effects cannot effectively explain social processes like repressive effects because time, place, and what people do matter (Tilly 1995, 2000; see Abbott 2001). And attempts to generalize the effects of categories of social action independent of the practical dynamics of their historical contexts fall into the fallacy of misplaced concreteness (Tilly 2008; Whitehead [1925] 2011, [1929] 1957). For example, just because we label various actions “repression” across a range of situations does not mean that all actions so labeled will have similar outcomes.

Conversely, the turn away from structural master theories has often left scholars of movements and politics without meaningful ways to generalize and compare across historical cases (McAdam et al. 2001, p. 27; Clemens 2005; McAdam and Tarrow 2011). In his last decade, Tilly often called for theories that explain social processes as similar generalizable dynamics—broader and more variable than mechanisms—that could be meaningfully compared across time and space and thus progressively understood, while accounting

for historical differences (Tilly 2000, 2001, 2008; McAdam et al. 2001, p. 27; Tarrow 2018). But Tilly did not make clear how to construct such theories, and he noted regretfully that few scholars had done so (Tilly 2008, p. 9). As Clemens suggests, shifting the kind of comparison may be key: embracing the multiplicity of history and abandoning invariant models “may not require an abandonment of systematic comparison so much as shifts in how those comparisons are structured” (2005, p. 513).

In the analysis of repressive effects above, generalizing the dynamics of practice made historically nuanced systematic comparison possible. Historical inquiry—guided by a consistent practice theory of repressive effects—provided clear reasons to expect that intense repression of civil rights action in the postwar United States would increase the likelihood of remobilization and that intense repression of violent urban rebellion would decrease the likelihood. By testing these expectations, insurgent practice theory made it possible to account for historical variations in the repressive effects on subsequent mobilization in the postwar Black Liberation Struggle that were opaque to invariant and structuralist models.

Yet while historically nuanced, this was no ideographic analysis. The large-*N* study above tested for hypothesized similarities across many cities and events based on a general theory of the dynamics of repression and insurgent practice. Moreover, the same general theory explained both why repression of one kind of practice was effective and why repression of the other was escalatory.

While different practices lead to different outcomes in different situations, the outcomes of different kinds of practical dynamics are generalizable. Generalizing the practical dynamics of repression, I propose that mobilization outcomes vary by the extent of support for the practices repressed, rather than varying by intensity of repression or structural context alone. This theory is not invariant because the extent of support for a practice is historically specific. This argument is not ideographic because it generalizes what mobilization outcomes depend on across time and place.

In recent years, countless fine-grained analyses of movements and politics have illuminated a myriad of detailed and historically specific processes that defy all invariant models. But the metatheoretical dilemma pitting invariant models against ideographic analysis has limited our ability to pursue these insights without forgoing the fundamental project of cumulative theory building. Much movement scholarship has fallen to the production of unique case studies and the elaboration of untestable mechanisms (Tilly 2000, 2001, 2008; McAdam et al. 2001, p. 27; McAdam and Tarrow 2011). The results of this study suggest that one way that scholars may be able to cumulate theory while accounting for historical nuance—transcending invariant models to explain social process without devolving to ideographic analysis—is by generalizing the dynamics of practice.

APPENDIX A

Descriptive Statistics

TABLE A1
DESCRIPTIVE STATISTICS: CONTINUOUS VARIABLES

	N	Mean	SD	Min	Max
Time (all)	36,137	16.56	10.23	1	35
Time (violent urban rebellion)	15,742	17.31	10.15	1	35
Time (civil rights–type)	22,928	16.56	10.20	1	35
Time (civil rights)	11,001	16.79	10.24	1	35
Time (late period, training)	4,802	17.24	10.18	1	35
Time (out of sample)	23,176	16.28	10.46	1	35
Time (late period, out of sample)	12,850	16.72	10.38	1	35
Arrest ratio (civil rights)	11,001	0.11	0.25	0	1
Arrest ratio ² (civil rights)	11,001	0.08	0.22	0	1

TABLE A2
DESCRIPTIVE STATISTICS: DICHOTOMOUS VARIABLES

	N	Frequency Yes	Frequency No
High arrests (all)	36,137	7,031	29,106
High arrests (violent urban rebellion)	15,742	3,366	12,376
High arrests (civil rights–type)	22,928	3,954	18,974
High arrests (civil rights)	11,001	1,948	9,053
Arrests? (civil rights)	11,001	3,473	7,528
Arrests? (late period, training)	4,802	871	3,931
Arrests? (out of sample)	23,176	8,536	14,640
Arrests? (late period, out of sample)	12,580	3,108	9,472
White mob violence (civil rights)	11,001	1,061	9,940
White mob violence (late, training)	4,802	83	4,719
White mob violence (Out-of-Sample)	23,176	1,319	21,857
White mob violence (late, out of sample)	12,580	587	11,993
Large? (violent urban rebellion)	15,742	11,791	3,951
Large? (civil rights–type)	22,928	15,536	7,392
Large? (civil rights)	11,001	6,912	4,089
Long? (violent urban rebellion)	15,742	8,229	7,513
Long? (civil rights–type)	22,928	12,395	10,533
Long? (civil rights)	11,001	5,853	5,148
Formal organization? (violent urban rebellion)	15,742	1,558	14,184
Formal organization? (civil rights–type)	22,928	10,166	12,762
Formal organization? (civil rights)	11,001	3,730	7,271
Police weapon attack? (violent urban rebellion)	15,742	4,181	11,561
Police weapon attack? (civil rights–type)	22,928	1,252	21,676
Police weapon attack? (civil rights)	11,001	553	10,448
1965–92? (civil rights)	11,001	4,802	6,199
1965–92? (out of sample)	23,176	12,580	10,596

APPENDIX B

Conceptualizing Repression

In her landmark conceptual work on repression, Earl (2003; see also 2011) distinguishes three dimensions of repression: repressive agents—either tightly or loosely connected with national political elite; observable vs. unobserved; and coercion vs. channeling. I include only a subset of the varied forms of repression Earl considers in my concept because I only expect a subset to adhere to the dynamics of repression and insurgent practice I theorize and observe. I view my narrower conceptualization as complementary to Earl's broader conceptualization. Consistent with Earl, I conceptualize repression as including actions by informal agents of authorities, as well as authorities themselves—e.g., Mussolini's black shirts (Bosworth 2006) or the White mob violence of the Jim Crow South (McAdam 1982; and see discussion in text). But while I share Earl's capacious definition of the agents of repression, my concept is restricted to observable coercion in Earl's (2003) typology. People have to observe repressive action by authorities in order to oppose it. And while covert and indirect actions by authorities may influence the dynamics of repression, direct and explicit coercive action like arrests and political violence play a meaningfully distinct role.

My conception of repression is similar to that advanced by Tilly, who also sees repression as regulatory but with key differences. In Tilly's conception, "repression is any action by another group which raises the contender's cost of collective action" (1978, p. 100). I diverge from Tilly's concept in three key ways: (1) In my conception, repression does not generically target "contention by a group" but pointedly targets participation in specific forms of practice (Bourdieu 1990, and see theory section). (2) While Tilly emphasizes cost, I emphasize the Weberian concern with organized "use of physical force in the enforcement of its order" within a territory as the crux of political power (Weber 1978, p. 54). Like Earl (2003), I use the slightly broader term "coercion" rather than Weber's "physical force" because the dynamics I theorize often concern censorship and other forms of coercion besides physical force, although such coercion is always backed by organized physical force. (3) In contrast to both Tilly and Earl, I make a point to distinguish repression from both concessions and vilification. Both concessions by authorities aimed to coopt support for an insurgency and vilification of insurgents through public campaigns to malign insurgents can raise the costs of contention, and thus are included in Tilly's (1978) definition. But I exclude both concessions and vilification because they don't involve overt coercive action.

In my view, the role of concessions and vilification are intelligible in terms of the ways they shape public reception of overt and coercive repressive action by authorities. Authorities often turn to concessions to draw support away from insurgents precisely when insurgent practices enjoy broad support and

authorities hope to avoid repressive actions that might escalate the insurgency. Similarly, authorities may covertly attempt to vilify insurgents with the intention to make them more repressible. For example, Federal COINTELPRO misinformation initiatives sought to frame Black Panther Party leaders as criminals. Internal documents later revealed by the U.S. Senate investigation showed that Hoover and the FBI intended these actions to turn supporters against party leaders and make them repressible (Bloom and Martin 2016). While such actions raised the cost of insurgency, and so would be classified by Tilly as repression, they were neither overt nor directly coercive. That said, like concessions, vilification is an important dimension of the repressive dynamic. Vilification can effectively undercut public support, making an insurgency vulnerable to direct repressive actions like arrests, raids, and political violence. As such, vilification efforts play a role in the dynamic of repression and insurgent practice that is meaningfully distinct from the role played by overt coercive repressive action itself.

APPENDIX C

Out-of-Sample Testing

A pervasive challenge in quantitative social analysis is that scholars customarily estimate a wide variety of models but report only a few preferred estimates (Young 2009, p. 380). As every quantitative researcher knows, it is unrealistic to expect that the first models tried will effectively use the data to precisely capture the social dynamics of interest. In successive stages of analysis, the researcher arrives at a deeper understanding of the relationship between the variables as captured in the data. It is only after many stages of analysis—when the researcher arrives at tests the results of which are both interesting and important—that the results are reported. The challenge is that this exploratory process, while uncovering the interesting and important relationships, simultaneously violates the foundational premises of statistical inference. As Leamer noted decades ago, social science “as it is practiced at the computer terminal involves fitting many, perhaps thousands, of statistical models. One or several that the researcher finds pleasing are selected for reporting purposes. This searching for a model is often well intentioned, but there can be no doubt that such a specification search invalidates the traditional theories of inference. . . . Traditional theor[ies] utterly lose their meaning by the time an applied researcher pulls from the bramble of computer output the one thorn of a model he likes best, the one he chooses to portray as a rose” (Leamer 1983, p. 37). In the poignant words of R. H. Coase, “If you torture the data enough, nature will always confess” ([1960] 2012, p. 27).

A range of meta-analytic, Bayesian model averaging, and econometric empirical studies have demonstrated the ways in which reporting a few

models selected from many can present an arbitrary assessment of social dynamics (Western 1996; Sala-i-Martin 1997; Hoeting et al. 1999; Magnus and Morgan 1999; Pinello 1999; Young 2009). Selecting among a large number of models can overfit the data much like building models with a large number of explanatory variables relative to the number of observations. The problem is not just that different controls, different equations, and different model calibrations yield different and often opposing assessments of a variable's influence. Perhaps even more troubling, overfitting models risks yielding results that explain "noise in the sample" rather than capturing a robust social relationship (Sarle 1995; Watts 2014, p. 339). As Babyak warns, "If you use a sample to construct a model, or to choose a hypothesis to test, you cannot make a rigorous scientific test of the model using that same sample data. This, by the way, is the real statistical meaning of the term post-hoc—it does not refer to afterward in terms of time. Rather, it refers to looking at the data to decide which tests or parameters will be included in the analysis and interpretation" (2004, p. 414).

One method of redressing this challenge is out-of-sample testing. In out-of-sample testing, "the model is estimated or 'fit' on one set of data—usually called the 'training' data—and then is evaluated exclusively on a distinct 'test' or 'holdout' set" (Watts 2014, p. 338). Long employed in a variety of fields, from sociology to econometrics, and computer science to medicine (Meese and Rogoff 1983; Kohavi 1995; Raftery 1995; Clark and McCracken 2001; Furlong et al. 2001), out-of-sample testing can allow the researcher to explore, fit, and calibrate models using real data, without violating the foundational premises of inference. While no quantitative method of analysis can account for the influence of all possible calibrations or controls on the relationship of interest, testing models out of sample has the advantage of assuring scholars that results reflect relationships more fundamental than sampling noise.

APPENDIX D

Additional Confidence Intervals

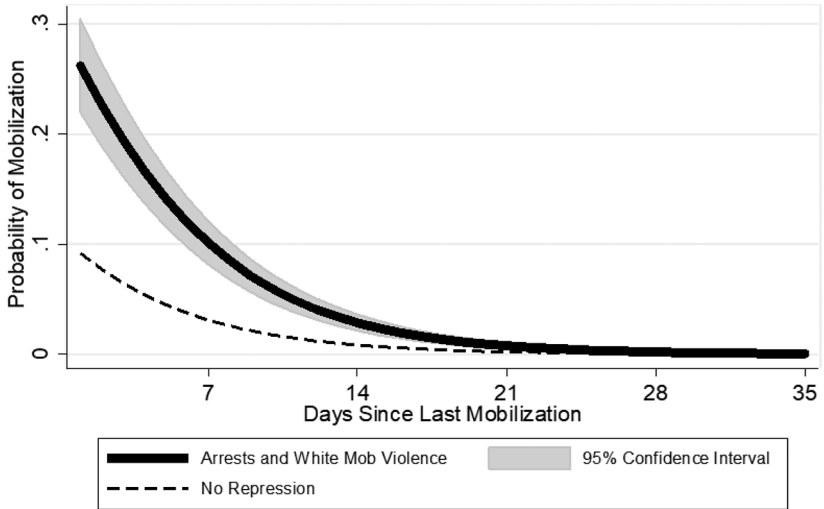


FIG. D1.—Confidence interval for out-of-sample test

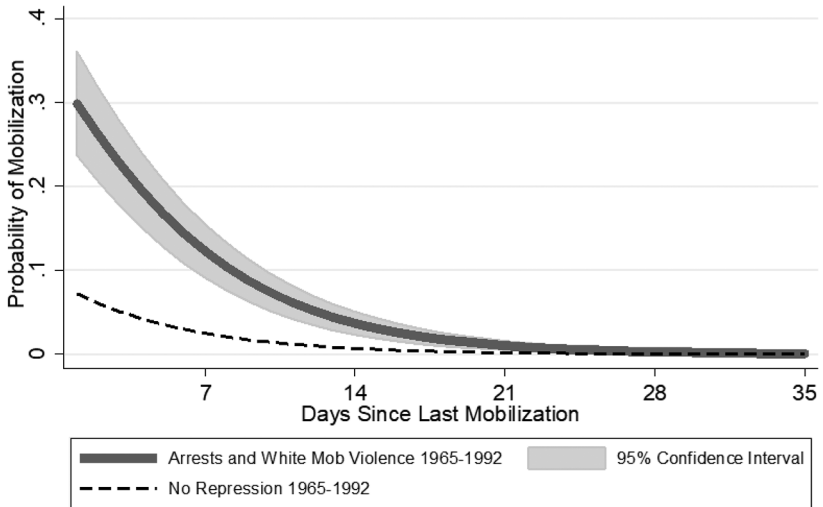


FIG. D2.—Confidence interval for late period out-of-sample test

APPENDIX E

Preliminary Quantitative Check regarding Black Panther Mobilization

Based on a qualitative analysis, Bloom and Martin (2016, p. 396) write that in “1969, brutal state repression . . . fostered increased mobilization.” To begin to check this claim quantitatively, I gathered preliminary data from the *New York Times*. To collect these data, I first conducted a keyword search in the historical newspapers database, available through ProQuest, for any articles published in the *New York Times* in 1969 containing the exact phrase “Black Panther.” I excluded all nonarticles, such as advertisements and opinion columns. This yielded 343 candidate articles.

I then went through each of these articles word for word and excluded any articles that didn’t explicitly discuss recent (usually the previous day) news of on-the-ground activities by the Black Panther Party. A few of the articles were not about the Black Panther Party, for example articles about similarly named sports teams. And many of the articles only referred to the Black Panther Party in passing, or retrospectively, rather than reporting on recent news. This culling process yielded coverage of news regarding 103 on-the-ground events of Black Panther mobilization.

During this period, the Black Panther Party promulgated a fairly consistent set of movement claims, largely presenting itself as a vanguard party representing the true interests of Black Americans and part of a global revolutionary movement, advocating violent confrontation with the state (Bloom and Martin 2016, p. 391). This claim is consistent with the practices represented in the *New York Times* coverage. Events covered in these *New York Times* articles involved some variety of tactics, but two kinds of tactics comprised the largest concentration of events: first, events in which members of the Black Panther Party engaged in armed resistance—usually including some exchange of fire—against police raiding local party chapter headquarters around the country; second, events of courtroom resistance where members of the Black Panther Party—usually under state custody and participating in high-profile legal proceedings—actively defied judges’ or marshals’ orders regarding courtroom conduct.

I coded these articles for three basic variables, namely, date, number of participants in the event, and number arrested.³⁶ I then analyzed these data applying the preliminary the basic hazard model presented in this article (see table E1).

This analysis includes 1,705 panelized observations of city-days following the Black Panther mobilization events. The analysis shows that, controlling

³⁶ Arrests include both street arrests and courtroom arrests. When courtroom arrests, i.e., imposition of bodily physical constraint by marshals, led to conviction on charges of contempt of court, street-type arrests were not employed because the Panthers found in contempt were already prisoners in state custody.

for time since the last mobilization in a given city, Black Panther mobilization is about twice as likely ($b = 2.156$) following high arrests, compared with periods following moderate or no arrests. The result is statistically significant ($P = .003$), so I reject the null hypothesis that high levels of repression have no effect on subsequent mobilization.³⁷

This analysis provides preliminary quantitative support for the claim that intense repression of the Black Panther Party led to increased mobilization. These findings should be taken as preliminary and suggestive, rather than definitive or final, for several reasons. First, I coded the data myself without the benefit of double-blind coding by a staff and measurable reliability as employed in construction of the event catalogs in the main analysis and customary for event catalog construction generally. Second, Bloom and Martin (2016) show that different Panther practices were effective in different periods over the course of the party’s history as the political conditions changed and that from the spring of 1971 on, the party became much more repressible generally. It would be good to test these conclusions quantitatively, but such an analysis was well beyond the scope of what I could rigorously accomplish here. Third, the two specific Black Panther tactical forms that appear most important in the 1969 *New York Times* data—namely, armed resistance to police raids and high-profile public defiance of judges’ orders regarding courtroom behavior during trials—are both excluded from the Ethnic Collective Action data, which focus more pointedly on riots and more traditional protest actions. Thus, any comparison with analyses to data from the Ethnic Collective Action data set should be drawn cautiously.³⁸ A fuller and more reliable quantitative analysis of repressive effects on the Black Panther Party over the course of its history is warranted.

TABLE E1
ODDS OF REPEAT BLACK PANTHER PARTY MOBILIZATION

	Party Mobilization
Time907***
High arrests	2.156**
Likelihood ratio $\chi^2(2)$	54
$P > \chi^2$0000
N	1,705

* $P \leq .05$ (two-tailed).

** $P \leq .01$.

*** $P \leq .001$.

³⁷ Descriptive statistics for the analysis are Time (Black Panther Party): $N = 1,705$; mean = 15.19; SD = 10.32; Min = 1; Max = 35. High arrests (Black Panther Party): $N = 1,705$; frequency yes = 454; frequency no = 1,251.

³⁸ See Davenport (2010) for discussion of some further challenges in rigorously producing an analysis of repressive effects on the Black Panther Party.

APPENDIX F

Afterword on the Current Moment

Beginning about five weeks ago at the time of this writing, following the killing of George Floyd by officer Derek Chauvin in Minneapolis—as well as the killings of Breonna Taylor by police in Louisville and Tony McDade in Tallahassee—the Black Liberation Struggle mobilized on a scale unprecedented in the United States since the 1960s. Nonviolent antiracist protesters were often met with harsh repression. For example, in one widely reported incident on June 1, police and the National Guard used tear gas and flash grenades and beat protesters with batons at Lafayette Park in Washington, D.C., to clear a path for a presidential photo op at St. John’s Church.³⁹

Highly repressive police response to Black Lives Matter protesters met with vast and unprecedented outpourings of allied support. In a very brief recapitulation of the trajectory of the Civil Rights movement, as depicted in figure F1, the Black Liberation Struggle quickly regained the right to protest peacefully, and the rate of repression plummeted. The rate of mobilization then declined. Some aspects of this wave of mobilization are quite familiar. Others stand out in contrast to the recent history of the Black Liberation Struggle and call for explanation.

The claims, targets, and tactics of this wave of mobilization are consistent with other Black insurgent mobilizations since the Ferguson uprising in 2014. In recent years insurgents have often demanded police accountability and the transformation of policing ranging from reform to abolition. Supported by some purely symbolic protest, and sometimes accompanied by property destruction, insurgents have usually protested nonviolently in streets and sometimes highways, blocking traffic in defiance of police orders.

One defining aspect of the Black Lives Matter mobilizations has been their punctuated rhythm. Unlike the Civil Rights movement where activists chose their battles and drove the process of insurgent mobilization by bodily violating Jim Crow institutions, Black Lives Matter mobilizations have largely responded to police killings of Black people.⁴⁰ For example, think of the mobilizations for Eric Garner in New York, Tamir Rice in Cleveland, Freddie Gray in Baltimore, Walter Scott in North Charleston, Alton Sterling in Baton Rouge, or Korryn Gaines near Baltimore. These, and most other highly publicized recent Black Lives Matter mobilizations, all emerged in response to police actions and then, after a period of heightened mobilization, de-escalated. These mobilizations do make business as usual impossible for a time. But with a few exceptions—like Ferguson—the movement has not figured out how to sustain disruption.

³⁹ <https://www.nytimes.com/2020/06/01/us/politics/trump-st-johns-church-bible.html>.

⁴⁰ Secondary waves of mobilization have often responded to the failure to indict.

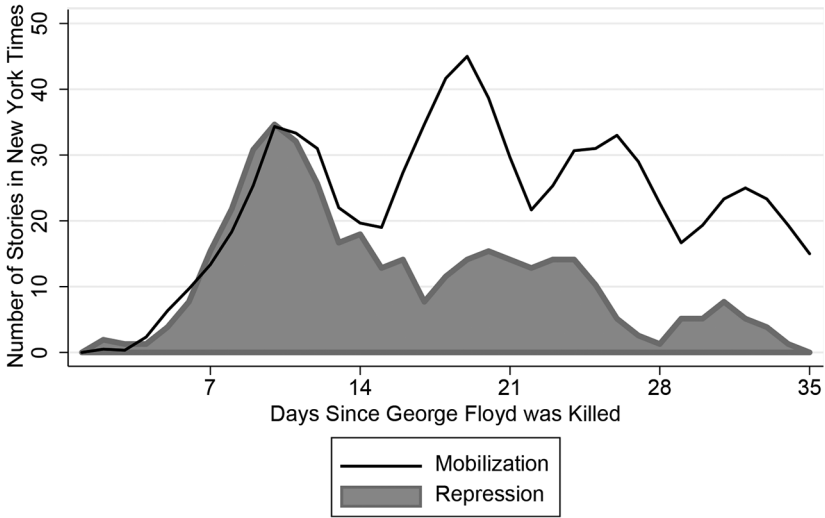


FIG. F1.—Mobilization versus repression in Black Lives Matter. Number of stories published in the *New York Times* per day mentioning “Black Lives Matter” is used as a proxy for mobilization. The number of these stories mentioning “tear gas” is used as a proxy for repression. The repression variable is scaled for legibility. Three-day rolling averages are graphed.

In a series of conversations in 2016 and 2017, I spoke about this issue with Alicia Garza—who helped coin the phrase “Black Lives Matter” and is one of the founders of the Black Lives Matter Network. Garza told me she recognized this punctuated rhythm of mobilization in response to police killings and sought to build the Black Lives Matter Network as a kind of “container” that would be available to support such mobilizations when they arose. Her idea was to use the momentum and resources forthcoming in those moments to build organizational strength for the long term. This approach has proven highly effective for organization building. Garza said that in her view, “I’m not sure that this is yet a movement. . . . The question of being able to drive insurgency is a question of coordination that I don’t think exists in the way it did during the last period of Civil Rights.”⁴¹ It appears that, at least for now, movement leaders have accepted the punctuated cycle as a given. For example, near the height of the recent wave of protests in mid-June, Garza told the *New York Times* that the protests would “start to die down, which they always do.”⁴² In this sense, the recent punctuated wave of Black Lives

⁴¹ Email from Alicia Garza, February 8, 2017.
⁴² <https://www.nytimes.com/interactive/2020/06/13/magazine/police-reform.html>.

Matter mobilization in response to police killing was quite similar to other mobilizations since 2014 but different than the activist-initiated waves in the Civil Rights movement.

But two aspects of this recent wave of mobilizations appear different from earlier Black Lives Matter mobilizations and could provide clues about what kind of insurgent practices might allow activists to sustain and direct the movement. First, the character of repressive action by the police seemed notably different. In some instances in recent years—such as in Ferguson in 2014—police responded to nonviolent protesters with harsh repressive actions, for example, using dogs, tear gas, and military style guns. But in recent years in many cities—perhaps learning from the escalations in Ferguson—police have sought to avoid the appearance of aggression toward nonviolent protesters. In the two weeks after George Floyd was killed, many police departments dispensed with such restraint. Video footage of police violence against nonviolent protesters flooded the internet. As implied by figure E1, such police violence appears to have contributed to the extreme escalation of mobilization. This observation suggests several questions for analysis: Why did police respond differently to this current wave of protests?⁴³ To what extent did harsh police repression drive the escalation of insurgency in the two weeks following the killing of George Floyd? Why did police nationally moderate the character of repressive action against nonviolent protesters after the first couple weeks? Was the shift coordinated? What role did the moderation of repression play in de-escalation?

Second, allied support is more extensive than in previous waves. Mobilization of symbolic support in more than 2,500 towns and cities throughout the country is striking⁴⁴—even predominantly White and Republican-leaning towns have mobilized. There has been an outpouring of financial support for insurgents. Mainstream politicians and businesses have taken unprecedented stands in support of the movement. And mainstream liberal newspapers such as the *New York Times* and the *Washington Post* have featured extensive op-eds in favor of the movement by movement leaders and allies. These observations suggest further questions for analysis: Why is allied support different now—what motivated it? What role did allied resistance to repression play in the escalation of this wave of insurgency?

⁴³ One hypothesis is that many police officers were emboldened by Trump's racist and authoritarian rhetoric. Police officers do the difficult and dangerous work of maintaining dominant social institutions, including structural racism. With statements like "when the looting starts, the shooting starts" (<https://twitter.com/realDonaldTrump/status/1266231100780744704>), the president declared support for extreme repressive violence against challengers.

⁴⁴ <https://www.nytimes.com/interactive/2020/07/03/us/george-floyd-protests-crowd-size.html>.

The structural racism that undergirds policing of Black communities is constitutive of U.S. society and is not going away easily. The Trump administration has responded to the antiracist protests by playing to its base with an exaggerated Nixonian “law and order” posture, augmented by more explicit White supremacist rhetoric. From my perspective it is likely some level of mobilization will continue at least until the November elections, but it is far from certain whether any significant leverage will be maintained. Based on insurgent practice theory and my own interpretations of recent events, I offer the following expectations and hypothetical explanations:

1. Movement allies in this wave have been motivated by a variety of grievances and political cleavages in addition to opposition to racist policing, not least, life-threatening mismanagement of the coronavirus pandemic; vastly expanding long-term inequality in income and wealth punctuated by the policy response to the 2008 Great Recession, and now again with the economic policy response to the pandemic; and the polarizing racist authoritarianism of the Trump administration. For the Democratic Party leadership and the liberal establishment specifically, recognition that the Black Lives Matter mobilizations and polarization with Trump concerning race is good for polling for the November 2020 elections has motivated a facilitative stance *viz* the movement.
2. Given the widespread allied support, a high level of national attention to the Black Lives Matter insurgency will persist until the November elections. One measure of the level of national attention to the Black Lives Matter insurgency is the number of articles mentioning “Black Lives Matter” published in the *New York Times*. In the year before George Floyd was killed, from May 25, 2019, through May 24, 2020, the *New York Times* published 204 articles including the phrase “Black Lives Matter.” Conversely, in the month following the killing of George Floyd, from May 25, 2020, through June 24, 2020, the *New York Times* published 729 articles including the phrase “Black Lives Matter.” I predict that in each calendar month between now and the November elections—namely, July, August, September, and October—the *New York Times* will publish no less than 50 articles that include the phrase “Black Lives Matter.” This is a rate about three times the monthly rate in the year before George Floyd was killed.
3. Extensive efforts will be made to vilify the more disruptive elements of the insurgency and make them repressible. Such efforts will increase after the November elections regardless of who wins. One of the more potentially disruptive practices antiracist activists have engaged since the George Floyd’s death is the establishment of encampments. Authorities will label any encampments as fostering violence and posing

threats to public health and will attempt to forcibly remove them. Authorities will also seek to vilify and repress insurgent practices involving property destruction, such as burning or looting.

4. Short of intense and sustained campaigns that seriously disrupt business as usual in specific locales, most concessions to the movement will be piecemeal and will not fundamentally redress structural racism. A main demand of the insurgents has been to “defund the police,” redirecting resources to better address social needs. The vast majority of municipalities will make no cuts to their police personnel budgets (as a percentage of overall budget). Very few municipalities, if any, will cut more than 10% of the police personnel budgets. Only those municipalities that experience the most large-scale sustained disruptions—as measured over time by value of property damage, numbers of protesters arrested, and street closure hours—will make any reduction to police personnel budgets.
5. Intense campaigns seriously disrupting business as usual will only be sustained if-when-where insurgents develop practices that are not only inherently disruptive but also hard to repress because they draw strong allied support. What is not yet clear is what forms of insurgent practice—if any—will be developed that can sustain large-scale disruption in this moment. Lots of allies are supporting the movement in spirit. But thus far—unlike the sit-ins or the Freedom Rides or the Birmingham campaign, or the Black Panther Party’s coupling of Free Breakfast Programs with armed self-defense of party territory—no seriously disruptive insurgent practices have emerged that appear to draw strong allied support in resistance to repression in this moment.⁴⁵

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⁴⁵ One partial exception seems to be the destruction of Confederate monuments, which seems likely to continue, but not to challenge customary policing.

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