Political Repression: Iron Fists, Velvet Gloves, and Diffuse Control

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Abstract
This article reviews research on political repression by social movement scholars. Four topics are discussed: (a) debates over the conceptualization of repression, the breadth of the concept, whether distinctions within the concept are productive and/or forms of repression are directly comparable, and the relationship between repression and political opportunities; (b) recent research on different types of repression, particularly protest policing; (c) an evaluation of research on different explanations of repression; and (d) an evaluation of research on the consequences of repression. Attention is also paid to areas where future research effort might be most productively spent, including identifying substantial gaps where more research is needed, where important debates exist that need research to push toward their resolution, where robust results exist but could be furthered by refinements, and where a more inclusive conceptualization of repression may link the study of repression to other significant literatures.
INTRODUCTION

State and private elites have long tried to gain and retain political influence and control. Research on these efforts sprawls across a wide array of literatures in sociology and political science, from research on how racial, ethnic, and religious elites have sought to maintain and grow privilege; to research on constraints on institutional political involvement and civic culture; to research on criminal justice systems as means of social control. While all of these are important modes of control, political repression is distinct in that the purpose of the control is to prevent or diminish direct and noninstitutional challenges to social, cultural, and/or political power (i.e., protest, activism, and social movements). As such, repression is often much more targeted than these other diffuse systems of social or political control (except in authoritarian states). But, like other forms of control, political repression has complex effects, shaping noninstitutional political challenges across their entire life cycle from inception to success or death. Also like other forms of control, repression can be manifested in a range of ways. From tanks in Tiananmen, to South American death squads, to political trials, to KKK attacks on civil rights leaders, repressive toolkits are diverse.

I begin reviewing the literature on political repression by discussing its conceptualization and attempts to handle the complexity introduced by so many repressive forms. I then briefly summarize trends in research on different particular forms of repression, arguing that most research on repression has been descriptive or has tried to address two orienting questions in the literature: How can scholars explain the use of repression, and what are the impacts of repression? I review research on each of these explanatory questions in turn. I then discuss one form of repression—protest policing—that has developed substantial lines of inquiry outside of predictors and consequences of repression. I also discuss trends in the geography of research on repression. Finally, I return to the substantive discussion that begins this paper: I reconsider the meaning of political repression and its connection to, and sometimes disconnection from, other potentially relevant literatures, such as crime and incarceration, surveillance studies, and the sociology of human rights.

WHAT IS POLITICAL REPRESSION?

Although virtually all agree that political repression refers to repressive actions directed at individuals and groups based on their current or potential participation in noninstitutional efforts for social, cultural, or political change, scholars have disagreed about finer aspects of the definition. Some have defined repression relatively broadly as actions that increase the costs of protest (e.g., Tilly 1978), whereas others have defined it more narrowly as “harassment, surveillance/spying, bans, arrests, torture, and mass killing by government agents and/or affiliates within their territorial jurisdiction . . . [that] violate First Amendment-type rights, due process in the enforcement and adjudication of law, and personal integrity or security” (Davenport 2007a, pp. 1–2). And although most definitions either explicitly or implicitly focus on state action, a few scholars argue repression should not be limited to state action (e.g., Earl 2004, Ferree 2005).

Some of the divergence in definitions is due to the wide variety of disciplines (e.g., sociology, political science, and history) and subfields (the study of social movements, democracy, and human rights, as well as various area studies) that contribute to the study of political repression. For instance, political scientists have been focused on the state even more than sociologists; comparativists often examine forms of repression that are easier to compare cross-nationally (e.g., human rights violations), whereas researchers studying a single country, movement, or social movement organization (SMO) may focus on more fine-grained differences in repression (e.g., when do courts versus police repress; see Davenport 2010).

The challenge when one crosses so many academic boundaries is to find the inflection point between an under- and overbroad
concept. For instance, when seen as wholly about state violence, repression is easy to identify but woefully underinclusive (see Earl 2003, Oliver 2008 for more on underinclusiveness). However, finding a meaningful, alternative outer limit on conceptual expansion is not easy, either: What distinguishes political repression from other forms of control, such as general social control or crime control? Moreover, when is something actually repressive, versus just resistant to change—e.g., what is the difference between people who drag their feet against change versus those who actively suppress actors pushing for change? Must we even see noninstitutional and collective action to know that political repression is at work, given that quiescence represents the pinnacle of repressive success (Cable et al. 1999, Gaventa 1980, Oliver 2008)? I return to the discussion of overbroad versus anemic definitions of political repression in the last section of the paper, but for now I adopt a relatively broad (but still quite consensual) approach and define repression as state or private action meant to prevent, control, or constrain noninstitutional, collective action (e.g., protest), including its initiation.

**TAMING CONCEPTUAL COMPLEXITY: TO SUBSUME, AGGREGATE, OR DISTINGUISH?**

Unless a very narrow definition is used, a substantial amount of variation and complexity hides behind the term political repression. Testing and comparing explanations of court action, militias, militaries, and police action are not easy tasks. Scholars have addressed this complexity either by attempting to reduce it or by incorporating it into their work.

**Is Repression a Stand-Alone Concept or Embedded in Political Opportunities?**

One way to quickly reduce the complexity involved in studying political repression is to argue that it is not a stand-alone theoretical concept; instead, according to this argument, it can be subsumed under the study of political opportunities. In essence, repression is so strongly shaped by other kinds of political opportunities that it merely reflects trends in them and can itself serve as a measure of opportunities. This claim originated with the political process approach to social movements (McAdam 1982) and necessarily involves ignoring private repression (however, see Osa & Schock 2007 for an alternative statement of political opportunities that could address this).

Over time, political opportunities researchers have made distinctions between more stable political opportunity structures (POS) and more volatile political opportunities (PO) (Kriesi 1995) and have included repression in both types of discussions. In the case of stable POS, repression is viewed as a component of POS that can be used to explain varying levels of mobilization across nation-states, where more open POS facilitate protest, although a curvilinear relationship is also suggested.

This makes explaining both repression and its impact straightforward. For instance, according to this view, authoritarian states have closed POS, in part because of repression, which should lead to low or nonexistent protest levels. These propositions have been examined in relation to specific states and in research trying to explain differing levels of repression across a variety of nation-states (e.g., Brockett 1991). Alternatively, democratic states should have lower levels of repression and therefore higher levels of protest (but see King 2000 on whether democratic states are actually less repressive). Of course, even democracies can vary in terms of stable POS, so Davenport (2007b) compares countries with a wide variety of regimes, including very different democracies, using data on 135 countries from 1976 to 1996. He finds that competitive and participatory electoral systems are particularly important checks on repression, but political conflict can dull these democratic effects.

POS/repression explanations are not without critics (e.g., Alimi 2009). And a large amount of research has examined how social movement mobilization still occurs in...
Channeling: indirect protest control using a reward and consequence structure that shapes, rather than directly controls, protest (e.g., nonprofit tax policy)


Researchers have also argued that more volatile PO greatly influence the level of repression. In this argument, changes in repressiveness are reflective of, or largely determined by, changes in the will of political elites or other aspects of volatile PO. Police and militaries are typically cast as agents operating at the behest of their political principals (della Porta 1995, 1996; della Porta & Reiter 1998a; Wisler & Kriesi 1998).

This variant is not without critics, either, particularly for explanations involving nation-states in which policing is decentralized and/or localized. For instance, in decentralized systems, there is a much larger principal-agent dilemma separating political elite will from police action on the street (e.g., Earl & Soule 2006), and the multiplex nature of “the” state (Cunningham 2009, Irons 2006) can make the realization of political elite will even more problematic.

Indexing Repression

Another way to reduce conceptual complexity is to collapse complexity through rankings or aggregation. Indexes and scales allow single measures of repressiveness and have been particularly popular in political science (Davenport 2007a). This is most evident in research that studies repression around the globe through large, quantitative, cross-national analyses (e.g., Davenport 2007b).

A very similar strategy for reducing complexity is to create a theoretical continuum on which specific forms of repression are plotted against one another; this renders forms of repression commensurable, undermining a focus on difference, but allowing easy comparisons between different situations. Such continua assume that there is some latent concept, “repressiveness,” on which real-world activities vary in value, allowing cases to be assigned specific values (e.g., McPhail & McCarthy 2005, pp. 3–4). Continua, in particular, require normative decisions by researchers: Researchers decide which actions are more or less repressive than others. There have been no empirical attempts to measure how subjects of repression would rank different forms of actions.

Distinctions Within Political Repression

A decidedly different response to complexity is to acknowledge it and then determine whether meaningful enumerations or distinctions can be made within the overall concept. Across time, scholars have amassed lists of repressive tactics in order to organize the field’s thinking (e.g., Carley 1997, Marx 1979) and have created ways of making more subtle distinctions within larger categories of repression (e.g., within protest policing, see della Porta & Reiter 1998a, Earl et al. 2003).

Alternatively, typologies organized around mechanisms or goals have been introduced. For instance, Boykoff (2007) outlines several Dynamics of Contention (McAdam et al. 2001) style mechanisms that result in the demobilization of social movements in the United States (e.g., resource depletion, stigmatization, intimidation). Oliver (2008) identifies three basic goals of repression—deterrence, incapacitation, and surveillance—that could be used to identify and categorize repressive actions.

Earl (2003) takes a different approach, cataloging repressive types and categorizing them based on three core distinctions: (a) whether the repressive actor is a state, private, or hybrid actor; (b) whether the repressive action is coercive or uses more carrot-based “channeling” (McCarthy et al. 1991, Oberschall 1973); and (c) whether the repressive action is observable/overt or unobserved/covert. Crossing these three distinctions produces 12 types, or categories, of repression, which can completely parse all known forms of repression. For instance, observable/overt coercion committed by national government actors includes
military-based repression, national guard deployments, and public protest policing. Unobserved/covert coercion by national government actors includes activities such as surveillance, monitoring, and the infiltration of groups.

**VARIATION IN RESEARCH ATTENTION BY FORM OF REPRESSION**

Earl’s (2003) typology was used to demonstrate that alternative forms of repression have been studied with varying intensity over time. Overt, coercive forms of repression are studied most often (e.g., violence, arrest). Within that larger category, protest policing is the most studied specific type of repression (and is discussed more below). Cross-national comparisons of overt coercion are also very common, although they diverge widely in the types of coercion examined (see Davenport 2007a for a review of quantitative, cross-national comparisons in political science research).

Covert repression, such as surveillance, has long been of interest to repression researchers, but it has received less overall attention. Marx (1974) was one of the first scholars to theorize about covert repression, particularly the use of informants and agent provocateurs. His work provided early empirical examples for a field starved for data on such covert actions and later provided a thorough review of covert surveillance tactics and repressive tactics (Marx 1979). However, nearly two decades passed after that, with very little work on the subject (save exceptions such as Gotham 1994, who argued that covert repression allowed democracies both to repress challengers and to appear nonetheless democratic).

Recently, though, scholars have been again attending to covert repression. Cunningham’s systematic analysis of multiple COINTELPRO programs (i.e., covert FBI counterintelligence programs that were exposed by activists) is the best known of this work (Cunningham 2003a–c, 2004, 2009). Cunningham documented the range of methods used by the FBI to suppress activism and destroy social movements, as well as how the organizational structure of the FBI influenced how COINTELPRO programs were developed and carried out. Irons’s (2006) examination of covert action against the civil rights movement in Mississippi suggests that covert surveillance may not be easily explained with unified elite will, a point that Cunningham (2009) has also made. Davenport (2005b) conducted a rare quantitative examination of covert actions by a black nationalist organization in the late 1960s and early 1970s. He finds that authorities react to threat and that the location of covert activities is related to the economic development and racial characteristics of neighborhoods. Instead of studying how covert repression is undertaken, as Cunningham, Davenport, Irons, and Marx do, Starr et al. (2008) interview activists to try to understand the effects of surveillance. They find that fear of surveillance takes a major toll on protesters and their organizations, leading to a range of behaviors (e.g., refusing to take meeting notes or keep organizational records) and fears (e.g., fear that informants are monitoring the organization) that limit activism. Varon’s (2004) largely historical account argues that covert repression can radicalize activists, a perspective that Zwerman & Steinhoff (2005) share. But Boykoff (2006) argues that repression does effectively demobilize. It is reasonable to expect that this resurgence of work on covert repression will continue as scholars worry that, since the terrorist attacks of September 11, 2001, increased surveillance is taking place in the name of fighting terrorism (e.g., Earl 2009; see also Dwyer 2007 for a report on a large protest surveillance operation).

Still other types of repression have received very little attention. For instance, despite specific calls to study private repression, there is still very little of this kind of research. Earl (2004) has stressed the importance of studying private actors as divergent as the Pinkertons and the KKK, while Ferree (2005) has called on researchers to explore everyday forms of “soft repression” carried out in private life. Channeling of any form has received little attention (see Earl 2003 for a review of that
Almost no research exists that analyzes the relationship between different types of repression (i.e., does police violence drive vigilantism or countermovements?). Despite this variability in research attention to different forms, scholars have been very consistent about the purpose of repression research. With few exceptions, research has attempted to document or describe a repressive form, examine what produces variation in repression (i.e., what explains repression), and/or understand the impacts of repression. Given that unity of purpose, I simplify the following discussion by subjugating differences between repressive forms, time periods, and geography; instead, I focus on main findings when repression is treated as a dependent variable and then on findings when it is treated as an independent variable.

REPRESSION AS A DEPENDENT VARIABLE

A primary analytic question in the repression literature is how to explain varying levels of repression (whatever its form), including studying how repression varies over time, across countries or other spaces, and across movement actors such as SMOs. This has been examined in many ways: across nations around the world; across states within a region or continent; or within a country, state, city, or SMO located in a particular city (e.g., compare the very different approaches taken in Aditojondro et al. 2000, Davenport 1994, della Porta 1995, Gregory 1976). A wide range of types of repression, types of data, and data sources have been used, as well, ranging from large, multi-user, cross-national, comparative, quantitative data sets on human rights violations (see Davenport 2007a for a review of quantitative data sources), to quantitative events-based data sets on protest policing (e.g., McCarthy et al. 2007, Wisler & Giugni 1999), to more case-specific research on specific repressive agencies (e.g., Cunningham 2003a).

Instead of reviewing this work by type of repression, country, or data source/type, I highlight robust findings that have emerged from this diverse research enterprise. The most compelling finding has been that threat is critically important to explaining repression (Davenport 1995, 2000b, 2005a, 2007a; Earl 2003; Earl et al. 2003) across time, place, and form of analysis. Put simply, the more a movement or a protest threatens authorities, the more likely it is to face repression (again, see Davenport 2007a for a review). Although many threats are treated as objectively threatening to authorities, “threat perception” (Mahooney-Norris 2000) research has shown that states may perceive greater threats from challengers that have strength in areas in which the state is weaker (either geographically or demographically; e.g., Boudreau 2005), and dissidents themselves might foster increased assessments of threat (Davenport & Eads 2001).

More recently, and primarily owing to the protest policing literature, some have argued that researchers must attend to those being threatened—elites and/or control agents. In centralized policing systems common in Western Europe (e.g., della Porta 1995, Wisler & Kriesi 1998), threats to elites are clearly important (which many interpret as evidence for a PO view of repression, as discussed above, e.g., Tarrow 1989, 1994). However, others argue that in more decentralized policing structures (as is true in the United States, for instance), threats to the repressive actors (e.g., police) matter more than threats to political elites. Waddington (1998) made this point in distinguishing between different kinds of trouble (in the job versus on the job) that police can experience. Waddington et al. (1989) focused on the importance of “flashpoints” in police-protester interactions. Earl & Soule (2006) show that threats to police are more important than threats to elites in explaining police action at protest events.

Beyond threat (elite or police, objective or subjective), scholars have argued for the importance of organizational and institutional features of the police and military in explaining repression. For instance, Ron’s (2000) research shows how organizational concerns affect...
repressive activity by militaries. Cunningham’s (2003a–c, 2004) work on the FBI’s covert counterintelligence program, COINTELPRO, shows how the internal organizational structure of the FBI decisively shaped FBI action toward the New Left and toward white hate groups such as the KKK. Earl & Soule (2006) show that organizational characteristics of police departments and institutional characteristics of policing strongly influence protest policing.

Other trends in findings are harder to identify across the welter of work trying to explain repressive action. For instance, the effect of the relative weakness of challenges is debatable. Gamson (1990 [1975]) introduced weakness as a proposition, arguing that authorities are opportunists and will repress when they believe they can win. Duvall & Stohl (1983) echoed these themes in their study of state terrorism. White (1999) finds differences in repression between state-allied vigilantes and vigilantes opposed to the state, which could be read as supporting a weakness account. Stockdill (1996) and Wood (2007) argue for a particular version of weakness: that racial and ethnic minorities are opportunistically targeted for repression. Together, these questions are often referred to as the repression-dissent nexus (Lichbach 1987), dissent-repression nexus (Davenport 2007a), or repression-mobilization nexus (Cunningham 2009). The literature suggests several hypotheses: Repression can deter activism, can escalate activism, can have curvilinear effects on activism, can have no effect on activism but affect how people become involved and what tactics they use, or can have no net effect. Research also differs in whether effects are examined at the macro level (i.e., on overall protest rates), on the meso level (i.e., impacts on SMOs), or at the micro level (i.e., impacts on individual activists). I cross these two axes of variation in research (expected effect, on the one hand, by macro, meso, or micro level of effect on the other) to describe this subarea, showing that the area is uneven in focus and has fundamentally unsettled findings.

**Deterrence**

Scholars have long suspected that repression quells political, and specifically collective, action (e.g., Feierabend & Feierabend 1966). However, at the macro level, there is only mixed support for deterrence (Earl & Soule 2010). Some have argued that this is because repression affects individual social movements differently based on their leadership (Nepstad & Bob 2006) or the kind of repression (e.g., situational versus institutional, as in Koopmans 1997). At the meso level, numerous scholars have documented the negative effects of repression on particular SMOs (e.g., Beckles 1996, Davenport 2010, Jeffries 2002, Jones 1988, Williams 2005). However, other research suggests strong and weak SMOs are impacted differently by repression (Titarenko et al. 2001). There has been far less research at the micro level, the most definitive of which found that effects vary based on the kind of repression and its (il)legitimacy (Opp & Roehl 1990). More recent work suggests that recruitment pathways moderate repressive effects (Linden & Klandermans 2006). In sum, individual-level deterrence has found support contingent on a variety of factors.

**Weakness:** an approach arguing that weaker protesters, protests, SMOs, or movements are opportunistically targeted for repression

**Repression-dissent nexus:** the effect of repression on protest, including on mobilization levels or tactical selection

**Deterrence:** when repression effectively quells or constrains protest
Escalation: when repression actually produces higher levels of protest or more disruptive protest

Tactical substitution: when repression affects which tactics are used, instead of the overall level of mobilization

**Escalation**

Repression can backfire and actually stimulate protest, as macro-level research has shown (e.g., Almeida 2003, 2008; Jenkins & Schock 2004; Ondetti 2006; White 1989). Several scholars have found upticks in protest following particular repressive acts (Francisco 1995, 2004, 2005; Hess & Martin 2006). Ortiz’s (2007) quantitative cross-national analysis identifies conditions under which backlash is more and less likely. Francisco (1996) also finds evidence of backlash in his quantitative cross-national research, but finds even stronger evidence of tactical adaptation/substitution (discussed below).

At the meso level, repression has been found to build solidarity within labor unions (Fantasia 1988), which could have implications for protest levels. Zwerman & Steinhoff (2005) argue that repression contributes to the splintering of organizations, sometimes causing radical factions to break off and move toward terrorism (see also Zwerman et al. 2000).

At the micro level, Hirsch (1990) argues for a radicalizing effect of repression. He claims that repression hardens commitment and makes individuals willing to take more radical action. Opp & Roehl (1990) find radicalization effects as well, but only when repression is viewed as illegitimate.

**Alternative Effects on Mobilization**

At the macro level, a U-curve between repression and subsequent mobilization has been suggested (Lichbach & Gurr 1981), as has an inverted U-curve (DeNardo 1985, Muller & Weede 1990), but more recent research calls both into question (e.g., see Francisco 1995 for a refutation of the inverted-U hypothesis). Reciprocal relationships between repression and protest have also been considered (e.g., Carey 2006). Others have suggested that timing influences the effect of repression on subsequent mobilization. For instance, Rasler (1996) argues that deterrent and radicalizing effects both occur but trade off over time. Brockett (1995) argues that the effect of repressive action varies based on when it occurs in the protest cycle.

Meso- and micro-level research has not suggested likely responses to repression beyond deterrence or escalation. One notable exception is Chang (2008), but he is focused less on mobilization than on the effect of repression on alliances among SMOs.

**Effects on Tactics**

An approach often referred to as a tactical substitution effect hypothesizes that states can influence which tactics are used by protesters through selective repression. Lichbach (1987) argues that the conflicting findings on the impact of repression on protest rates may be because the real changes are happening in tactical substitution. Tactical substitution has received empirical support at the macro level (Francisco 1996, Moore 1998). Other researchers have suggested that repression can limit the diffusion of tactics (Myers & Oliver 2008, Wood 2007). Meso- and micro-level research on these points has been rare.

**PROTEST POLICING**

As mentioned above, most research on repression of any form can be boiled down to descriptive research or explanatory research on the causes or consequences of repression. Research on protest policing has done those things, too. For instance, rich descriptions of protest policing have been authored (e.g., Waddington et al.’s 1991 account of the British policing of a mine strike). Researchers have also evaluated alternative explanations of police action, including threat (e.g., Earl et al. 2003, Earl & Soule 2006), weakness (Earl et al. 2003, Davenport et al. 2011), and political opportunities (e.g., della Porta 1996), among other explanations. The consequences of protest policing have also been analyzed to determine which police actions effectively suppress protest (e.g., Earl & Soule 2010). However, work on protest policing has also engaged several additional novel questions. I summarize these additional research issues here (see della Porta & Fillieule 2004 for an older review of this subarea).
The main subject of work in this area has concerned changes to overall protest policing strategies, particularly in Western democracies (e.g., della Porta & Reiter 1998b). McCarthy & McPhail (1998) were the first to identify and analyze major protest policing strategies. They argue that two primary models of protest policing have existed in the United States (and other Western nations) since the 1950s: escalated force and negotiated management. Escalated force is a primarily force-based and confrontational model of protest policing that was popular in the 1960s and early 1970s. The negotiated-management approach is a permit- and negotiation-based model that became popular, eventually eclipsing escalated management, in the mid-1970s (see also McCarthy et al. 1999, McPhail et al. 1998). Across a line of research, McCarthy & McPhail have attempted to document this shift in the United States and the exporting of these approaches to other nations.

Other researchers reach similar findings when studying Western democracies. For instance, Waddington (1994) makes compatible arguments about the rise of negotiated management in British protest policing, although Green (1990) disagrees (but see Waddington et al. 1991 on the same strikes). Della Porta et al. (1998) and other European researchers (e.g., Jaime-Jiménez & Reinares 1998) find a similar shift in European protest policing, albeit with some distinctions (e.g., Winter 1998). Interestingly, Fillieule & Jobard (1998) suggest tensions between supervisors and line officers, in which supervisors are more likely to embrace negotiated management—style policing and both supervisors and line officers are reactive to protester behavior (matching aggressive protesting with aggressive policing).

After initially wide-scale acceptance of these arguments, more recently the shift to negotiated management has been challenged on several fronts. First, some question whether the switch to negotiated management was ever that complete. Vitale (2007) finds that police use a negotiated management strategy at some protests but aggressive policing strategies at others, suggesting that a wholesale shift was never made. Soule & Davenport (2009) examine protest policing in the United States from 1960 to 1990 and find that, although protest policing was less aggressive after 1969, this shift could be explained by changes in characteristics of protest. When protest was threatening, police reacted aggressively across the entire period, indicating that the aggregate level of threat at a given time is more predictive of policing strategy than is the transition to negotiated management.

Second, others argue that the development and expansion of specialized units like SWAT for the control of protest is ignored in negotiated management research. Kraska & Kappeler (1997) show that paramilitary policing units developed in the 1960s, in part to handle crowd control and protest. Kraska & Paulsen (1997) push further, arguing that SWAT is the “iron fist” to negotiated management’s “velvet glove.” Instead of negotiated management replacing the iron fist, they synergistically coexist. Other militarized tactics have also been identified (e.g., Reiner 1998), leading Soule & Davenport (2009) to question whether protest policing has actually become more, not less, aggressive over time. These criticisms have not been uncontroversial themselves. For example, Waddington (1999) rebuts Kraska & Paulsen’s (1997) argument. But McPhail & McCarthy (2005), who originally argued for the switch to negotiated management, have acknowledged the concomitant rise of SWAT teams in protest control (but they date the rise of SWAT somewhat differently than others; see the Center for Research on Criminal Justice 1977 for an alternative dating).

Third, researchers have questioned whether the so-called “Battle in Seattle” of 1999 and/or 9/11 have shifted police protocols back toward escalated force or led to the development of new aggressive policing styles that are replacing negotiated management. For instance, Noakes & Gillham (2007, p. 335) argue that the anti–World Trade Organization protests in Seattle, Washington, revitalized protesters and was a “Pearl Harbor” from the perspective of some in the police establishment. This led to
the development of an alternative to negotiated management that they refer to as “strategic incapacitation”:

This new approach is characterized by a range of tactical innovations aimed at temporarily incapacitating transgressive protesters, including the establishment of extensive no-protest zones, the increased use of less-lethal weapons, the strategic use of arrests, and a reinvigoration of surveillance and infiltration of movement organizations. This shift in police tactics during protests is consistent with broader changes in the ideological underpinnings of crime control, including an emphasis on risk management and the prevention of (rather than reaction to) crime and disorder. (Gillham & Noakes 2007, p. 343)

Della Porta & Tarrow (2001) argue for a general swing toward aggressive policing, as exemplified by Seattle and by the Group of Eight Summit protests in Genoa, Italy, in 2001, but argue that this shift was under way well before 9/11. Further confounding a specific link to changes in policing strategy and 9/11, Zwerman (1989) found links between repression and counterterrorism as early as the Reagan era. That said, the USA PATRIOT Act of 2001 allows many of the practices that had been outlawed in the wake of the COINTELPRO scandal of the 1970s, (re)opening opportunities for increased covert, domestic surveillance (Cunningham 2004), and it has required the development of new frames to oppose domestic repression (Maney et al. 2005).

Beyond debates about overall protest policing strategies, this area has produced numerous other novel theoretical claims and concerns. For instance, in her study of European protest policing, della Porta (1998) argues for the importance of “police knowledge” (i.e., the beliefs that police hold about their roles and their knowledge of protesters). Although most research on police knowledge supports its importance to protest policing (e.g., Wahlström 2007), de Fazio (2007) has argued that ongoing interactions between police and protesters can lead police to act outside of negative beliefs about protesters on occasion. Research on protest policing has also raised the importance of laws governing police behavior before and during protests (McCarthy & McPhail 2006).

Other scholars have examined finer distinctions within protest policing, effectively decomposing protest policing. For instance, Earl et al. (2003) show that the deployment of five types of police tactics (e.g., presence with no action, minimal action, arrests, etc.) is explained by a different set of factors for each police tactic (see also Davenport et al. 2011). Put differently, the causes of very specific police actions differ, suggesting that the standard approach in the literature of modeling many divergent police tactics together as undifferentiated police action might yield misleading results.

Other research focuses on particular kinds of police action, arguing, for instance, for a deeper investigation of surveillance (e.g., Starr et al. 2008). Similarly, researchers are examining the consequences of arrests and examining their legacy through political trials. Despite classic work showing the sharp teeth of arrests (Balbus 1973; Barkan 1984, 1985; Scheingold 1974), arrests have been regarded by many as the lesser of evils of social control mechanisms or are even seen as preferred substitutes for police violence. For instance, McPhail & McCarthy (2005) view arrests as less repressive than even the use of barricades to constrain protester movement by police. But when a researcher empirically examines arrests—as Earl (2005) does with arrests across several decades—arrests seem far more aggressive, consequential, and repressive; arrests can also co-occur with, versus serve as alternatives to, violence. Thompson (2007) also recognizes the enormous repressive capacity of arrests in her examination of jail solidarity actions.

Other researchers follow arrests into trials (e.g., Steinhoff 2007), thereby studying the legacy of police action instead of police action directly. Early work on trials establishes the costs, consequences, and sometimes opportunities that trials create, as well as examining defense decision making (Barkan 1977,
In more recent work, Barkan (2006) tries to reinvigorate research on political trials by mapping an agenda for research.

REPRESSION ACROSS THE GLOBE

Thus far, I have not stressed the geography of cases. Instead, I have focused on the similarity of topics (e.g., research on protest policing in countries ranging from the United States to the Middle East), questions (e.g., researchers examining both democratic and nondemocratic states are interested in whether repression works), and findings (e.g., threat matters to predicting repression in a wide array of states). But it is worth noting the geography of cases because research has not been spread evenly around the world.

Research on repression has favored North American (particularly the United States) and European cases. My review has reflected this trend with a plurality (but not a majority) of the literature reviewed covering such cases. Much of the work focused on the United States and Europe examines protest policing and political trials (as discussed above, and acknowledging that work outside of U.S. and European cases is referenced on both topics). A substantial portion of work is focused on major social movements, such as American civil rights movements and/or black nationalist movements (e.g., Beckles 1996, Cunningham 2009).

English, Irish, and European cases also capture a good deal of scholastic attention. Here, research may focus on a particular nation (e.g., Ireland, Germany, Switzerland, Italy, the Netherlands) or may compare European nations to one another (e.g., della Porta 1996). Again, a substantial amount of this work examines protest policing, but there is also a notable contingent of work on the effects of repression and its predictors.

Cross-national, comparative (often quantitative) research comparing cases from different regions of the world also makes up a large share of the literature. Indeed, cross-national research is competitive in output with research on European cases. Most of the large-N work in this area comes from political science (e.g., Feierabend & Feierabend 1966 as an early leader in this area). A subset of comparative research focuses on groups of countries identified through shared theoretical characteristics. For instance, research examines repression in developing nations across the globe (e.g., Davenport 1994) and in nondemocratic states (Osa & Schock 2007).

Central and South America is also featured in a notable amount of work (e.g., Brockett 1995, Nepstad & Bob 2006; for examples of work from specific countries, see Almeida 2008 on El Salvador and Ondetti 2006 on Brazil; and see Loveman 1998 for a comparisons within South America). Much of this work has examined more authoritarian regimes.

The Middle East has also received moderate attention. Much of this attention is due to research on the Palestinian/Israeli conflict (e.g., Alimi 2009, Ron 2000), although other notable work has been completed that is not concerned with this conflict (e.g., Rasler 1996 on Iran).

Eastern European nations, particularly former Soviet states, have also been examined, but with less frequency (e.g., Johnston 2005, 2006), as have southeast Asian countries (e.g., Chang 2008) and the Pacific Rim (e.g., Zwerman & Steinhoff 2005). The smallest amount of work has been done on African nations.

BROADENING THE CONCEPT OF REPRESSION?

I opened this review by arguing that the key challenge in defining repression is finding the inflection point between an overly narrow definition and an overbroad one. A very narrow definition is like a single purpose tool: It is wonderful for what it does but limited by its specialization. For instance, the study of political repression would be a deficient area if protest policing was the sole focus of inquiry.

On the other hand, an overbroad definition allows for so much complexity and so little specificity that a concept risks becoming a
theoretical umbrella, not an analytical guide. For instance, if one made no distinction whatsoever between generalized social control and targeted political repression, repression research would suddenly include any study of how state and private actors steer or control the behavior of individuals or groups, including research on state-supported racial inequalities, war, genocide, and crime and punishment, among many others. The resulting lack of precision would encourage the unwitting comparison of theoretical and empirical apples to oranges and spread the literature so thin that it could not provide much value-added on any given topic. This approach would also ignore the generally targeted nature of political repression and dynamics tied to noninstitutionalized politics or political participation.

After hinting at this dilemma early in the article, I sidestepped it and proceeded to review the literature on its own terms using a consensual, if broad, definition. I proceed quite differently here: I ask what different literatures might offer to students of repression if the concept of political repression were more inclusive. Instead of trying to review each area below in any detail, which is far beyond the scope of this article, I point out areas of potential connection between repression research and other areas and the opportunities that could arise from such connections, and I briefly speculate on what has limited previous engagements between these literatures.

Control in Democratic States

Repression researchers have traditionally thought of repression as a scarce commodity that is selectively deployed (especially in democratic states), even when countries invest substantially in other forms of social control (e.g., as the United States does in crime control). This, in turn, implies that the boundaries between social and political control (which affect most of the population) and political repression (which is targeted at individuals and groups participating in noninstitutional, collective action) are bright-line distinctions, at least in less authoritarian states. However, at least three research areas—research on mass incarceration, surveillance studies, and terrorism—challenge the targeted nature of control in democratic states, pointing to pervasive control systems that might have immediate or downstream impacts on political engagement.

Mass incarceration. Although connections between mass incarceration and institutionalized political engagement have been powerfully made (e.g., Manza & Uggen 2006, Wakefield & Uggen 2010), research on criminal justice has been slow to track connections between mass incarceration and noninstitutional political engagement such as protest. Similarly, social movement scholars have not examined this connection extensively (save Oliver 2008, discussed below) because it is not clear that the policies creating mass incarceration, or mass incarceration itself, were ever intended to control protest.

Yet, recent research calls into question the wisdom of this disconnection. On a global scale, Ruddell & Urbina (2007) show that there is a relationship between overall imprisonment rates and repression in the top 100 richest countries, which is notable given the wide variety of political regimes represented in that set. Oliver (2008), though, has been the most forceful advocate of the connection between incarceration and protest control. She argues that the mass imprisonment of black men in the United States is integral to repression following the 1960s and 1970s race riots in America and that crime control and “dissent control” are inextricable (p. 13). Oliver deepens this criticism by arguing that repression can work through deterrence (which is what most scholars immediately think of), incapacitation (in Oliver’s argument, through prior arrest), and coercive surveillance (through the surveillance of black men and communities, facilitated by the wide reach of imprisonment, parole, and probation). Repression researchers are practiced at studying the first mechanism (i.e., deterrence; see, for example, Davenport et al. 2011 on “protesting
while black”) but tend to miss the other two by forgoing research on the connection between mass incarceration and protest.

In a more subtle manner, Oliver forces scholars to consider how important intention is conceptually to repression. She acknowledges the significant controversy within criminology and law and society scholarship on whether the war on drugs and mass incarceration were designed to allow control over the African American community, but her arguments suggest that scholars need to consider effect, not just intention, in identifying repressive practices.

According to Oliver, without considering these broader connections between crime control, inequality, and social movements, we lose powerful leverage in understanding the trajectory of major social movements, such as the civil rights movement (and, according to Davenport et al. 2011, even miss important dynamics within protest policing). Oliver also implies that repression researchers lose the ability to see connections between what is happening in the world of protest and the rest of the social landscape.

**Surveillance studies.** Surveillance has been an important topic in research on political repression. For instance, classic work by Marx (1974, 1979, 1988) demonstrated the importance of covert surveillance to political repression. Cunningham’s (2003a-c, 2004) work on the FBI’s COINTELPRO program has updated these concerns, and surveillance continues to be the subject of work today. For instance, Gibson (2008) shows that Americans reported feeling less politically free than they did in the McCarthy era, which he attributes to a generalized versus targeted perception of repression and political intolerance today. Starr et al. (2008) focus on the impacts of surveillance on individual protesters and SMOs in contemporary America, documenting the resulting development of a “security culture.”

However, there is an interdisciplinary and autonomous literature on surveillance—surveillance studies—that has been spurred on by changes in technology that make it affordable and practical to collect and analyze massive amounts of data on individuals. Drawing on socio-legal research, science and technology studies, political sociology, and political science, scholars examine how surveillance of all types operates and what the effects of surveillance are.

Despite the obvious overlapping concerns of surveillance studies and research on political repression, these fields have remained notably distinct. There are several likely reasons. First, whereas social movement researchers have viewed repression as limited and targeted (particularly in democratic societies), surveillance studies scholars (e.g., Lyon 2007; see also Marx 2007) argue that surveillance can be found in a wide variety of modern spaces, including corporate record-keeping about employees and consumers and government census-taking, as well as in more classic arenas such as military surveillance and police surveillance. Clearly, forms of surveillance as divergent as the census, the mining of consumer databases by marketers, and closed-circuit television do not uniquely affect political expression. And, whereas for some forms of surveillance the potential impacts of expanded surveillance on protest may be more obvious (as with closed-circuit television monitoring of protests), other forms of surveillance, such as the use of RFID cards, have far less immediate connections to protest control (at least for now).

Second, the theoretical heritage and traditions of each area also differ. Research on political repression has stressed the ebb and flow of repression across time, whereas surveillance studies sees surveillance as an increasingly pervasive element of society and, for many authors, of modernity (Lyon 2007). Consequently, theorists such as Bentham, Foucault, and Giddens play major roles in surveillance studies but lesser roles in the study of political repression. For instance, because Foucault sees possibilities for control in most spaces and contexts, his work is a touchstone for surveillance studies scholars who see control everywhere, too, but it is less used by repression researchers, who tend to see repression as scarce and targeted. Other
surveillance studies scholars take a Weberian approach, casting surveillance as the result of widespread bureaucratization (e.g., Dandeker 2007). The theoretical disconnect is similar: Surveillance studies sees surveillance as increasingly ubiquitous, whereas political repression researchers tend to focus on targeted forms of control.

Third, surveillance studies increasingly argues against a distinction between the observer and the subject of surveillance, suggesting that people participate in their own surveillance through their adoption of new technologies and conveniences (e.g., Lyon 2007, Marx 2007). But students of political repression are not apt to see politically repressed individuals as inviting or participating in their own repression.

Despite these rather large differences in orientation and tradition, researchers interested in political repression could still usefully engage research on surveillance studies in a range of ways. Perhaps most important, surveillance studies has examined how the cost of surveillance has declined over time (particularly due to technological advances), which makes it easier to raise overall surveillance levels. Repression researchers have always seen political repression as scarce, in large part because control is expensive, but surveillance studies has much to say about changing cost and supply curves for surveillance. Perhaps even in more democratic societies, surveillance has become both unobtrusive and inexpensive enough that it can be less targeted. Without a deeper engagement with this literature, it will be hard for repression researchers to identify and explain those changes. In more technical ways, surveillance studies can also offer a large and growing repository of research on specific surveillance practices, including practices such as record-keeping, which repression researchers have not written much about. Surveillance studies can also help repression researchers to understand how digital technologies affect political repression. While digital technologies are bread-and-butter issues in surveillance studies, there are very few studies of digitally enabled political repression.

**Terrorism studies.** Whereas for the literatures discussed so far I have staked my case for engagement on what repression research can gain, repression research has much to offer to research on terrorism. On the ground, there is a large amount of anecdotal evidence to suggest that protesters and Muslims have been heavily monitored since 9/11 because of crude conflations between terrorism and protest and/or terrorism and Islam (Dwyer 2007; see Sullivan & Hendricks 2009 on civic conflations). Yet social movement research suggests that conflating terrorism with protest in general and/or conflating terrorism with Islam is unfounded and counterproductive. First, research on repression has examined subjective threats and shown how wasteful (in terms of efficiency of control) and harmful (to human rights and civil liberties) states can be when they miscalculate who or what is threatening. Moreover, other work on social movements suggests that viewing protesters or Muslims in general as threatening to the state is an empirically poor judgment. Such a subjective threat assessment not only ignores distinctions between peaceful and violent protesters and peaceful and violent Muslims, but it also ignores an axiom of social movement studies: Only a very small proportion of people who hold a belief will ever take political action on that belief (but see LaFree & Ackerman 2009 for acknowledgment of this point within some sectors of terrorism studies). Finally, repression researchers recognize that radical beliefs can be nonviolent but that repression can move peaceful but radical groups toward terrorism, even when that shift would have otherwise been unlikely (e.g., Zwartman & Steinhoff 2005, in terrorism studies; see Sánchez-Cuenca & de la Calle 2009 for some recognition of this point). This means that the conflation of radicalism and terrorism may actually create terrorism, not help control it.

Despite the obvious importance of what repression researchers could bring to an academic conversation on terrorism, they have been slow to make connections to terrorism studies. I speculate that some of this has to do with a
fear of seeming to consort with the enemy by providing advice to state actors about repression. Certainly some of the same agencies that repress protest also substantially fund antiterrorism research and efforts. But this disconnection is shortsighted because it invariably ensures that information that could both protect protest and limit terrorism is harder to find and use.

### Controlling Specific Populations

In both free and authoritarian states, social stratification powerfully shapes social and political life. Social movement scholars have come to understand much more about how race, ethnicity, gender, and religion (among other stratification systems) affect social movement formation, individual participation, and repression. However, social movement scholars, as Oliver (2008) points out, have been slower to consider pervasive stratification as a form of repression in and of itself. Henderson (2004) makes a similar argument, noting that repression researchers have not analyzed the subjugation of women around the globe as a form of political repression, despite its undeniable effects on women’s political participation (both institutional and noninstitutional).

This is likely due in large part to the massive expansion that labeling stratification as repression would involve for research on repression. It would be the conceptual equivalent of the fish swallowing the whale. Many repression researchers would find this conceptualization so broad as to fail to offer any analytic utility. Moreover, it is not clear that stratification researchers would benefit from such a change in nomenclature, either, given that research on stratification already works to understand the elaborate and varied social and political stratification systems that nation-states and their citizens have created and maintained (e.g., see Winant 2000 on racial stratification).

However, there are still points of connection that could be further leveraged. For instance, researchers interested in race and ethnicity have worked to understand how states create and maintain racial hierarchies, often in nonobvious ways. This kind of work could be blended with work on political repression to understand how racial state practices operate in tandem with repression to police larger racialized systems. Irons (2006, 2009), for instance, examines how states maintain the social meaningfulness of racial boundaries by literally policing both racial boundaries and social change efforts. The marriage of work on racialized states and how that racialization affects repression would move far beyond work that examines, for instance, how racial bias affects protest policing (which has been more commonly studied). The same could be said for gender as a stratification system.

### Generalized Control and Studying Authoritarian States

Although repression is typically thought of as a scarce resource, authoritarian or totalitarian countries are unique in their massive investments in, and consequent large capacities for, control. Instead of selective repression, a large share of the population is subjected to substantial control across much of social and political life, including political engagement. The boundaries dividing social control, political control, and what has been classically considered political repression collapse in such a situation. One cannot even distinguish between repression and other forms of control based on impact because limited noninstitutional political participation is overdetermined in authoritarian states. I argue that it would be useful to examine how multiple control systems, including repression, reinforce one another in authoritarian states.

**Human rights.** As Somers & Roberts (2008) point out, although a large amount of work speaks to human rights across an array of disciplines and subfields, only recently has a recognizable sociological subfield on human rights emerged. Complete with a new American Sociological Association section, this subfield has immediate overlaps with repression researchers, who have used human rights...
violations to measure repressiveness in comparative work for decades (e.g., Davenport 2007b). However, a deeper engagement between these two areas could accomplish much more.

For instance, human rights researchers are interested in violations of rights, as well as in the development and change in the concept of rights, the codification of rights in legal instruments, and the development and diffusion of human rights regimes globally. These are certainly issues that could deeply inform cross-national repression research and research on repression in specific countries, but they have not powerfully done so yet. Moreover, repression researchers are often looking at the dark side of repression: what causes it and its consequences. But an engagement with human rights research suggests that repression researchers could also look for potential solutions to political repression, such as the development and adoption of human rights regimes. By allowing repression researchers to study what may prevent, preclude, or limit repression, human rights scholarship has much to offer.

Although I am suggesting this connection in relationship to authoritarian states, Somers & Roberts (2008) make a case for human rights scholarship encompassing work on stratification and legal support for stratification. That line of work would obviously also be relevant to repression researchers studying democratic nations (see section on stratification above).

Genocide. Genocide involves much more than suppression of noninstitutional political engagement, but researchers interested in repression have much to gain and to contribute to this area of research. Indeed, much like the study of revolutions, the study of genocide often is a boundary area for social movement scholars. Contention is usually heavily implicated in both genocide and revolution, but both go much further than the less apocalyptic contention that social movement scholars typically study. In the case of genocide, research on repression is the most proximate social movement research area because genocides tend to be committed by autocratic governments against minority communities (Harff 2003).

A principal question facing genocide researchers and repression researchers involves how central political contention is to the creation of genocide. Certainly civil war is often a prerequisite for modern genocides (e.g., Krain 1997, Harff 2003), but is genocide usually directed primarily at political challengers and/or political opponents as Valentino et al. (2004) claim, or does genocide often involve mixed motives (e.g., Hagan et al. 2006), or does countering political contention serve as a cover for more far-reaching and opportunistic violence (e.g., Davenport & Stam 2009)? Harff (2003) goes so far as to differentiate between genocide and politicide to make this point. Additional research, including research that makes distinctions between different kinds of genocide across time (e.g., Chalk & Jonassohn’s 1990 four types of genocide), is needed to address this question.

A related question facing genocide and repression researchers involves whether any of the same explanations that have been used to explain other forms of repression help to explain genocide. Political threat seems a likely element of any politicide, but research has also found that suddenly opened political opportunities can facilitate genocide (Krain 1997). From a repression perspective, this is surprising because opening political opportunities are usually associated with declining repression. However, it also appears that democracy buffers both repression (Davenport 2007b) and genocide (Rummel 1995).

Of course, students of repression study significant political violence, including murders, that are not genocides. In these instances, there is also much that repression researchers can learn from genocide research. For example, genocide researchers have studied the wider complex of problems that occur during humanitarian crises aside from direct government repression, including refugee, medical, housing, and food crises (Hagan et al. 2006). Trying to measure and understand how humanitarian and social welfare crises interact
with repression is a critical task for repression researchers. For instance, humanitarian or social welfare crises may make dissent quite difficult and thus make directed governmental political suppression less necessary from the perspective of rulers; alternatively, such crises might make suppression even more potent when dissent does occur. Recent genocides have also involved a range of other crimes, including wide-scale rapes (Hagan et al. 2006), that repression researchers need to study.

Repression researchers have much to contribute to the study of genocide, as well. For instance, Davenport & Stam’s (2009) work on Rwanda demonstrates how important it can be to apply classic repression research tools to research on genocide and civil war. Their research findings suggest a major revision is needed to accounts of Rwandan violence and genocide; their findings are partially based on having created an event-based timeline of violence that would be a familiar data collection strategy to repression researchers and social movement scholars. As such, they provide a model for how repression researchers could work on civil war and genocide going forward.

Censorship. Repression researchers have primarily been concerned with censorship when it is targeted at protest, social movements, or non-governmental organizations (e.g., understanding the processes behind, and consequences of, Chinese censorship of Falun Gong is of concern to political repression researchers, but the censorship of other nonmovement-relevant information in China has not been). For instance, social movement researchers have not been very engaged in debates over the censorship of pornography in America, but are instead concerned with censorship of war-related reporting.

The most prolific discussion of censorship in repression research casts it as a component of cross-national measures of government openness versus totalitarianism or as a component of scales of repressiveness (e.g., Davenport 2005b). The assumption is that censorship is a marker of a more totalitarian state rather than a process in which democracies may also engage, at varying levels. These measures have been much more popular in political science than in sociology, in large part because of their usefulness in large, cross-national, quantitative research. Interested readers should consult Davenport’s (2007a) review of this work within political science.

Various scholars have also taken up the issue of censorship more indirectly through a discussion of data and methodology. Researchers have questioned the use of newspaper data (a) in authoritarian states because of substantial in-country repression (e.g., Ball 2005) and (b) during armed conflicts (e.g., Alimi 2007 on Israeli censorship). These researchers have identified alternative data sources, such as interviews with victims of repression. However, the scale of these projects can be massive (Ball 2005 draws on interviews with 20,000 individuals to estimate political murders, using a capture-recapture method for indentifying murders), which may substantially limit their use.

However, understanding information access and censorship in more nuanced ways (including how censorship operates in democracies) could help repression researchers understand how censorship might contribute to quiescence, how censorship limits the options for redress that citizens can even identify when they have grievances, and how censorship shapes in-country versus out-of-country views of major social and political issues. Work on censorship might also help scholars unpack how international pressure influences in-country repression, as Peksen (2010) does in his study of media openness when international economic sanctions are being applied (finding that sanctions worsen censorship). Moreover, as Deibert et al. (2008) document, online censorship is increasingly widespread and often targeted at political opponents and insurgents. Understanding how online censorship works and how groups attempt to circumvent it will be increasingly important to understanding social movements over time.
CONCLUSION

The literature on political repression, focused particularly on the repression of social movements and social movement actors, is broad and varied. This review has identified and examined debates over the conceptualization of repression, research on different forms of repression, and research that alternatively casts repression as an independent or dependent variable. Clearly, major progress has been made over the past 30 years.

Nonetheless, substantial gaps still exist. There is a paucity of research on private forms of repression and still far too little research on covert forms of repression and channeling. There is virtually no research examining potential trade-offs or synergistic relationships among different types of repression. There is also far less research on the meso- and micro-level repercussions of repression than on the macro impacts. Vigorous research on any of these topics could lead to substantial advances in the field.

Areas where more robust findings are present—such as threat as a predictor of repression—can still benefit from research that adds additional nuance. For instance, research examining the difference between threats to political elites and threats to police, and research examining objective threats versus threat perception, are still important research edges in this otherwise more resolved subarea of the literature.

And, despite substantial research efforts, very unsettled, and yet quite important, debates in the field persist. For instance, and of both substantive and theoretical importance, the macro impact of repression on mobilization is empirically very unsettled.

Finally, I introduced a number of literatures that could enrich the study of repression (and/or be enriched by it) if the conceptualization of repression were expanded. My hope is not to unbound repression conceptually, but to encourage new research horizons, or at least connections to other research areas.

In addition to these frontiers, certainly others will emerge over time. For instance, with more and more protest-related activity happening online, it will be increasingly important for scholars to consider how repression may work online. Earl & Schussman (2004) have taken an initial step in this direction by examining the actions of state governments against strategic voting Web sites. Before that, Peckham (1998) examined how Scientology attempted to control online critics, hoping to squash the anti-Scientology movement. Uncovering new questions and frontiers, particularly when provoked by empirical changes in the protest field, is critical to the continued health of the literature on repression. In turn, continuing to move our collective understanding of repression forward is critical to improving sociology’s understanding of political and cultural change.

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Argues that repression research should expand to include broad social controls such as mass incarceration.

Argues that threat, not a transition to negotiated management, explains varying protest policing across time.


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