

## DYNAMICS OF REPRESSION AND MOBILIZATION: THE GERMAN EXTREME RIGHT IN THE 1990S\*

Ruud Koopmans†

*In spite of the centrality of the relationship between repression and mobilization to our understanding of state-movement interactions, the literature has not even come close to providing conclusive answers. A variety of competing models exists, each of which can claim some theoretical plausibility and empirical support. This inconclusiveness seems to derive from the general level of analysis in many studies and from insufficient empirical acknowledgment of the interactive and dynamic nature of the repression-mobilization nexus. This paper aims to avoid these problems by presenting a detailed analysis of the interaction between the mobilization of the German extreme right and the different forms of repression that state authorities have reactively applied. Two types of repression – institutional and situational – and their impacts on two types of mobilization – violent and nonviolent – are distinguished and analyzed both cross-sectionally, by comparing the sixteen German federal states, and diachronically, through a time-series analysis for the period 1990-1994. The results consistently show that the two types of repression have very different impacts on mobilization. Whereas situational police repression as a direct reaction to mobilization events had an escalating effect, more indirect, institutional repression such as bans of organizations and demonstrations or trials and court rulings against activists had a clear negative impact on the extreme right's level of mobilization. The article discusses several reasons for this relative effectiveness of institutional repression, including its greater degree of consistency and legitimacy as well as its preventive focus on mobilizing structures.*

Although the question of how repression impacts mobilization is arguably at "the core of any theory of rebellion" (Francisco 1995: 263) – and, one may add, of any theory of social control – we are still a long way from a satisfactory answer. This is certainly not a result of a lack of theoretical and empirical studies on this issue. On the contrary, more than any other kind of state-movement interaction, the repression-mobilization nexus draws attention of scholars from a variety of fields, including social movements, political conflict (resolution), rational choice, and (violent) collective action.

The reason for this strong interest probably lies in the fact that the confrontation between protesters and repressive authorities is the most visible, concrete, and spectacular form of state-movement interaction. Whether seen from a rational choice, normative, or social psychological theoretical point of view, repression appears as a factor that should

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† Ruud Koopmans is a research fellow at the Department "The Public Sphere and Social Movements" of Wissenschaftszentrum Berlin für Sozialforschung (WZB). Please address correspondence to the author at WZB, Reichpietschufer 50, 10785 Berlin, Germany. E-mail: ruud@medea.wz-berlin.de

have a strong impact on the levels and forms of protest mobilization. The apparent empirical ease with which repression can be measured—compared for instance to measuring movement success, group solidarities, or individual motivations—and correlated with measures of protest has further contributed to the popularity of studies of repression. Finally, of course, the effectiveness of repression in combatting (extremist) protest is a hot political issue. It leads to a division between political "doves" and "hawks" in almost any case of large-scale confrontation between states and social movements.

Given the fact that generations of scholars have addressed this issue without coming up with a conclusive answer, my goals in this paper are relatively modest. To a considerable extent, too little modesty has been one of the causes for the deplorable state of our knowledge. Too often, scholars have tried to develop theories and empirical tests aiming at generalizations which encompass repression and mobilization *sui generis*—thus neglecting differences among *forms* of repression and mobilization—and which stretch across the globe and a variety of types of political regime. Usually, the quality of the empirical data used to test such theories has far from matched these theoretical ambitions (Zimmermann 1977: 131). In contrast to this "big questions, poor data" approach, I aim to start from the opposite direction by investigating the impact of repression on one specific category of protest—the mobilization of extreme right and xenophobic groups—in one democratic, Western country—Germany—during a relatively brief historical period—the first half of the 1990s. This modest focus allows the use of relatively detailed data which can be differentiated according to types of repression and mobilization. This enables the use of time-series designs to capture the interactive nature of the relation between coercion and protest. In the next section, I will first give a selective overview of the relevant theoretical and empirical literature, from which I derive a number of hypotheses. I proceed by first describing the data and operationalizations used. In the remaining part of the article I then present an empirical analysis of the relation between repression and extreme right mobilization, first through a static comparison among the sixteen states constituting the Federal Republic, and then through a time-series analysis for the period 1991-1994.

### THE EFFECTS OF COERCION ON PROTEST

From different theoretical perspectives, the effects of repression on protest mobilization seem obvious. For rational choice theorists, repression is a factor that increases the costs of collective action and therefore has a negative impact on the level of mobilization (Muller and Weede 1990: 635). Although authors working within this perspective often acknowledge that repression may also have backlash effects, these are usually treated as secondary to and weaker than the deterring effect (for instance, DeNardo 1985: 154). True to the axioms of rational choice theory, Opp and Roehl (1990) propose that repression has a direct negative effect on mobilization. However, their model moves beyond a narrow rational choice perspective by including the indirect, and possibly positive, effects of repression on social and moral "incentives" for mobilization, i.e. group solidarity and norms. Extended in this way, the rational choice model approaches models that stress the normative evaluation of repression, which I will discuss shortly.

A different strand of theory emphasizes the social psychological effects of repression. In Gurr's (1969) classical application of the frustration-aggression hypothesis to political violence, state repression is seen as further intensifying the frustrations underlying violent protest and therefore contributes to its escalation (see

also Feierabend and Feierabend 1966, Davies 1962, among many others). These classical approaches tend to see rebellion as irrational and spontaneous and have lost much of their popularity. However, Brockett (1995) and others have recently called for rehabilitating the role of emotions such as anger and revenge, which are seen as related to group solidarities and social norms of justice.

This latter position may be linked to a current emphasis in the social movement literature on group identities as constitutive elements of collective action. For countercultural movements that are based on a strong opposition between "us" (the movement) and "them" (its political opponents), violent conflict serves to reinforce the movement's collective identity and repression may be actively sought and provoked. Rather than being evaded as a cost, from these movements' perspective, repression embodies the very message that they seek to convey to their adherents and to the larger public, namely, that of a repressive political system that is in need of revolutionary change (Koopmans 1995: 32-35).

Because arguments can be derived for both the deterrent and escalating effects of repression, it is not surprising that several attempts have been made to integrate both of them into more complex models. The most influential of these is the so-called "inverted U-curve" (see DeNardo 1985; and Muller and Weede 1990, among others). This model is based on the assumption that protest mobilization first increases with increasing repression as a result of a combination of the above discussed normative and social-psychological mechanisms. Only beyond a certain— theoretically undetermined— level of repression does the deterrent effect of repression begin to get the upper hand, resulting in a virtual stifling of protest in totalitarian regimes at the far end of the repression spectrum. This reasoning seems quite plausible, but so too does its exact opposite, a (non-inverted) U-curve. This model assumes that while initial, limited repression may be effective, beyond some—again undetermined—threshold repression falls victim to the law of diminishing returns (Lichbach and Gurr 1981). High levels of repression strengthen solidarity among the protesters and increase support among the general populace by provoking moral outrage. In this way, too much repression becomes counterproductive.

In an attempt to integrate these contradictory attempts at integration, Neidhardt (1989) combines both models and produces a "lying S-curve." He proposes that repression first decreases protest according to a U-curve model. Then, an increase in mobilization occurs as the level of repression crosses what Neidhardt calls a "line of proportionality." This marks the normative boundary of what a society considers to be legitimate repression given the tactics and intensity of protest. Beyond this level protests increase because state coercion that is perceived to be "out of proportion" sets in motion a process of escalation. Finally, further along the range, increasing repression effectively succeeds in scaring people from the streets. The exact reverse of this model, again, is suggested by Francisco (1995). On the basis of empirical results indicating that very harsh repression accelerates protest, he hypothesizes that "the inverted-U curve might yield another rise in protest at the high end of coercion" (1995: 265).

This wild variety of theoretical models has led many authors to outcries of despair. Others have only added to the confusion by offering yet more subtle attempts at synthesis. In the words of Zimmermann (1980: 191), "there are theoretical arguments for all conceivable basic relationships between government coercion and group protest and rebellion, except for no relationship." In fact, this

must be considered an understatement, for there is also a strong argument to be made for no relationship at all. If we take both the deterrence and escalation hypotheses equally seriously, we are led to the plausible argument that both repression's costs and the moral outrage repression produces are linear functions of its intensity, in which case they neutralize each other and produce no effect at all.

Given the several plausible relations between repression and protest, the range of possibilities can only be narrowed down by empirical investigation. Unfortunately a substantial part of the literature is composed of highly abstract modelling of complex intertwined plausibilities which are untestable with the kinds of data available to us here on earth (see for instance Lichbach 1987; Hoover and Kowalewski 1992). There is also a large number of empirical studies that have tried to test some of the more down-to-earth models, but the empirical tests have been as inconclusive as the theories they have tried to test. Some studies, particularly comparisons of state populations, have found confirmation of the inverted U-curve model (Gurr 1969; Muller and Weede 1990). A larger number of studies have reported a positive linear effect of repression on protest.<sup>1</sup> Finally, the deterrence or backlash model has been confirmed to a lesser extent but still is supported by a substantial number of studies (e.g., Hibbs 1973; Francisco 1995).

The reasons behind this inconclusiveness seem to be both methodological and theoretical. Methodologically, the first problem concerns the quality of the data. Repression is often operationalized by general system characteristics such as the size of the armed forces or the extent of constitutional rights and civil liberties, and rarely includes direct measures of the actual deployment of coercion against protest (Zimmermann 1977: 123-133). At best these measures can be seen as more general indicators of political opportunity structures, but they certainly do not capture the specific impact of repression. The quality of the data used to measure protest or political violence is even more doubtful. Often these data are derived from the *World Handbook of Social and Political Indicators*, which, at least as far as protest is concerned, is extremely crude if not invalid (Rucht and Ohlemacher 1992: 79-81).

A second problem is that the overwhelming majority of studies (91% according to the review by Hoover and Kowalewski 1992: 156) use static, cross-sectional (usually cross-national) data. This is aggravating in the light of the fact that most authors acknowledge that the relationship between repression and protest is basically a *dynamic* process which evolves temporally. Moreover, this relationship is not only dynamic, but also *interactive*. Protest is a function of repression, but the reverse is obviously true as well. As a result, all correlations or regression "effects" drawn from cross-sectional analyses should be regarded with strong suspicion. They may reflect an effect of repression on protest; an effect of mobilization on coercion; or, most probably, some (undeterminable) combination of the two. This methodological artifact may go a long way in explaining why so many studies have found a positive linear relation between coercion and protest.

Theoretically, the main problem was already identified in my introduction: the level of theorizing and analysis has been much too general. Instead of continuing to try to generalize across the widest possible range of political regimes, it might be wiser to restrict theories and empirical analyses for the moment to specific regime types, for instance contemporary Western democracies. The same is true for generalizations across movements. Until we understand how repression affects specific types of movements it does not make sense to try to relate summary measures of repression to summary

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<sup>1</sup> No less than 70 percent of 101 studies between 1965 and 1990 reviewed by Hoover and Kowalewski (1992: 155) confirmed this hypothesis.

indicators of all kinds of protest. Why would we assume in advance that militant neonazis react in the same way to repression as, say, the peace movement? Of course, this does not imply that we should give up the goal of a more general theory of repression and rebellion altogether. We should acknowledge, however, that until we know more about the concrete interactions of repression with specific movement types in specific political contexts, the results of juggling data on too general analytical levels will remain frustrating and inconclusive.

Theoretical and empirical differentiation seems also necessary regarding different *forms* of repression and their relation to different forms of protest. Here, a number of interesting proposals have been made recently, which I will try to apply in the following analyses. Three analytical dimensions can be discerned. First, several authors (e.g., Zimmermann 1977; Lichbach 1987) have suggested that what matters most is not the level of repression as such, but its degree of *consistency*: "The rule for government is: Don't reward and punish the same tactic; reward one and punish the other" (Lichbach 1987: 287). In the light of the arguments discussed above for the deterrence and escalation hypotheses, it is easy to understand why this would be so. Whereas the moral outrage produced by repression, whether viewed from a relative deprivation or from a normative perspective, is likely to be higher if the repression is unexpected, the opposite is true for the effect of unpredictable repression on the evaluation of the costs of collective action. Repression that follows a clear pattern can be included in the cost/benefit calculations for future actions. However, if activists have reason to believe that a next time they might get away with their action without being punished, the deterrent effect is likely to be mitigated or neutralized.

Many students of the German extreme right have pointed to inconsistent repression as one of the causes of increased its mobilization in the 1990s. Expanded mobilization often occurred after riots, such as those in Hoyerswerda in 1991 and Rostock in 1992, in which authorities intervened in highly inconsistent (and largely unsuccessful) ways against violent right-wing mobs attacking foreigners. Actions that were tacitly tolerated one day were countered with police violence the next day, and although the riots were strongly condemned by politicians and many of their instigators were arrested, authorities simultaneously rewarded rioters by removing the foreigners who were attacked. In the context of this paper, it will not be possible to directly analyze the role of inconsistent repression. Inconsistency is hard to quantify, and requires a level of analysis (day-to-day events in specific localities) for which the data used here are too crude.

The discussion of repression inconsistency indicates that it is important to link repression to specific tactics used by protesters. The second dimension builds on the same idea and focuses on the legitimacy of repression in relation to the tactics used by protesters. Neidhardt (1989) formulates this idea in a general way by introducing his "line of proportionality." DeNardo (1985: 197-199) distinguishes "legalist" regimes, whose repression is a linear function of the radicalness of protesters' tactics, from "tyrannical" regimes, which repress challengers regardless of their tactics and punish them as much for what they think as for what they do. In a similar vein, Opp and Roehl (1990) differentiate between "legitimate" (directed against illegal protest) and "illegitimate" (directed against legal protest) repression. The idea behind these distinctions is that, in contrast to "deserved" repression against protesters who use illegal or violent tactics, repression against nonviolent protesters that uses an "illegitimate" amount of force provokes moral outrage, and is likely to have an escalating effect. Thus, police violence as a reaction to movement violence is much less likely to cause feelings of solidarity with the protesters than repression of deliberately peaceful demonstrations, such as those of the black civil rights movement in the Southern U.S. in the 1960s (see McAdam 1982). In the following

analyses, I will try to test this idea by distinguishing between repressive police reactions against both violent and nonviolent actions of the extreme right.<sup>2</sup>

A third, more structural dimension embraces what has been alternatively called "systemic" and "event" coercion (Zimmermann 1977: 129); "formal" and "informal" repression (White and White 1995); or "structural" and "behavioral" repression (Muller and Weede 1990). This dimension refers to the distinction between the *institutional*, formal, more general, less direct, and usually legally sanctioned repressive measures taken by higher-level state authorities, such as governments or the judiciary, and the *situational*, informal actions of lower-level state agents, most importantly the police, who in direct contact with protesters apply repression in a relatively spontaneous, ad-hoc manner. The link between this and the two former dimensions is obvious: institutional repression is likely to be more predictable and consistent, and can claim more legitimacy than the direct interventions of the police. Thus, institutional repression may be expected to be more effective in deterring protest than situational forms of coercion. Even more important than their consistency and legitimacy may be that institutional and situational repression may have different targets. Whereas the latter represses collective action directly, the former often aims at the underlying mobilizing structures and at preventing the (re-) occurrence of protest altogether. As Tilly (1978: 100-101) has argued: "From a government's point of view, raising the costs of mobilization is a more reliable repressive strategy than raising the costs of collective action alone. The antimobilization strategy neutralizes the actor as well as the action, and makes it less likely that the actor will be able to act rapidly when the government suddenly becomes vulnerable. . . ." The German extreme right has been confronted with this type of institutional repression to a considerable extent, especially since the death of three Turkish women in a right-wing arson attack in Mölln in November 1992. Since then, the most important extraparliamentary organizations of the extreme right (such as the *Nationale Offensive* and the *Deutsche Alternative*) have been banned. Others have been placed under the observation of the internal security office (such as the *Republikaner*), and right-wing rallies, meetings, and demonstrations have been increasingly prohibited. The following analyses will distinguish such forms of institutionalized repression from police repression that occurred as a direct reaction to extreme right protests.

### DATA AND OPERATIONALIZATIONS

The data are derived from a content analysis for the period 1991-1994<sup>3</sup> of every second issue (Monday, Wednesday, Friday) of the German national daily newspaper *Frankfurter Rundschau*. This newspaper was chosen because, compared to other German newspapers, it gives a relatively broad coverage to the issues relevant for the

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<sup>2</sup> The alternative distinction between legal and illegal protests could not be taken into account here since the number of legal actions by the extreme right was too low for reliable analysis (11.5% of all protests). This is a result of the German extreme right's action repertoire which is heavily dominated by violence (75.7%). To some extent it is also a result of the extreme right's demonstrations often being banned by the authorities, which considerably narrows its legal maneuvering space.

<sup>3</sup> Although my broader data base includes the year 1990, I have chosen 1991 as a starting date because throughout most of 1990 East and West Germany were still formally separate states with their own police and legal systems. This may have resulted in different dynamics of repression and mobilization.

project. The representativeness of the source was confirmed by comparing the resulting time series with those derived from other newspapers.<sup>4</sup> These data were gathered as part of a larger project on immigration and citizenship politics and their relation to the mobilization of extreme right and xenophobic groups in Germany<sup>5</sup>. For the purpose of this article two types of events that were coded: first, collective action events by extreme right and xenophobic groups, ranging from meetings to violent attacks; and second, acts of repression by state actors directed against the extreme right, including arrests, police violence, court cases, or bans<sup>6</sup>.

Extreme right collective action (n=785) was differentiated into violent (77.3%) and nonviolent events (22.7%). Events were classified as extreme right if (1) the stated goal of the action could be classified as extreme right (e.g., denial of the Holocaust or the slogan "Germany for the Germans, foreigners out!"); (2) the actor could be identified as belonging to the extreme right (e.g., organizations such as the *Nationale Offensive* or the *Republikaner*, or collective actors such as skinheads); (3) extreme right symbols such as the Swastika or the Hitler salute were displayed; or (4) the action was targeted against foreign, ethnic minority, or Jewish persons or objects (e.g., homes, graveyards)<sup>7</sup>. An event was classified as violent if there was evidence of actual and substantial use of physical violence against objects or persons (threats of violence by themselves were not). The nonviolent category included such actions as collective gatherings, marches, and writing slogans on walls. The large majority of violence took two forms: physical assaults on (mostly foreign) people and arson attacks against foreigners' homes and centers for asylum seekers. Although violent protests were much more numerous they also tended to involve a relatively low number of people. The large majority of participants in extreme right events were mobilized in nonviolent collective action (75.5%)<sup>8</sup>. To some extent, the two types of mobilization also involved other types of actors. While the violence was predominantly carried out by youth and skinhead groups or by unidentified perpetrators, nonviolent mobilization was dominated by explicitly extreme right

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<sup>4</sup> Taking the series for extreme right violence as an example, the monthly counts for the *Rundschau* correlated .91 with those derived from Germany's largest tabloid, the *Bild-Zeitung* (coded for the period June 1991 through June 1993) and (on a weekly basis) .89 with data derived from three local newspapers (for the period June through October 1991). Also, the *Rundschau's* coverage of extreme right mobilization closely mirrors the development of extreme right violence as measured by the statistics of the *Bundesamt für Verfassungsschutz*, Germany's federal internal security agency: the correlation among the two sources regarding the monthly counts of extreme right violent events is .89.

<sup>5</sup> The use of newspapers as a source of event data on protest has become widely accepted in recent years. For a discussion of the methodological issues involved, see, for instance, Snyder and Kelly 1977; Rucht and Ohlemacher 1992; Kriesi, Koopmans, Duyvendak, and Giugni 1995; McCarthy, McPhail and Smith 1996.

<sup>6</sup> The boundaries of events were drawn using three criteria: time, place and acting in concert. If two acts were separated in time or space or if the actors involved in the two acts were obviously not acting in concert, they were treated as separate events. Following the latter criterion, extreme right collective actions and police interventions (e.g., arrests) against such actions were coded as separate events.

<sup>7</sup> Of course, actions against such targets that could be attributed to other actors (e.g., attacks against Turkish objects by Kurdish separatists) were not included in the category of extreme right events.

<sup>8</sup> The German extreme right is not, to be sure, a mass movement. Across the four-year period the participation in all extreme right events totals only 42,000. The actual number of extreme right activists is lower because this figure includes cases of multiple participation by the same persons.

organizations and groups. In order to see whether the effects of repression differed between mass and small-group mobilization, the analysis will include both numbers of events and numbers of participants (natural logs) as dependent variables.

Following Tilly (1978: 100), repression was defined as any action by state authorities that raises the costs of extreme right collective action. State repression (n=677) was divided into two main categories corresponding to the two poles of the institutionalization dimension discussed above. Formal, institutional repression (65.3%) included acts instigated by governmental authorities (such as bans), the judiciary (trials and court rulings against extreme right activists) and internal security agencies. It also included coordinated, large-scale police actions that were not a direct reaction to extreme right events (such as raids and house searches). Informal, situational repression (34.7%) is defined by its immediate, physical link to extreme right mobilization events. The actor involved in this case is, by implication, always the police. To be classified as repression, a police intervention involved either arrests or the use of violence. To allow for a test of the legitimacy hypothesis, a further distinction was made between situational repression against nonviolent protests (9.5 %) and against violent protests (25.1 %).<sup>9</sup> Each variable was then aggregated by federal states for the cross-sectional analysis (n=16) and to monthly counts for the time-series analysis.

### THE SIXTEEN STATES OF THE FEDERAL REPUBLIC COMPARED

Regarding the level of extreme right mobilization and the levels and forms of repression, there are substantial differences among the sixteen German federal states. As table 1 shows, the five states that formerly belonged to the GDR display high levels of extreme right mobilization, and the ten western states show relatively low levels. There are also substantial differences within both groups. Because the state of Berlin resulted from a fusion of the former West and East German parts of the city, it occupies an intermediary position between the western and the eastern states.<sup>10</sup> For numbers of participants, not presented in the table, the picture is a bit different, with a less clear East-West division. Mecklenburg-Vorpommern has the highest level of mobilization, followed by Bavaria and Brandenburg.

Several factors have been suggested to explain why extreme right mobilization was so much stronger in the former GDR. Among them are high levels of unemployment and the breakdown of social networks and norms as a result of reunification (Heitmeyer 1993). Another factor is the weakness of extreme right political parties in the East, which may have led extreme rightists to find an outlet in extraparliamentary mobilization instead of elections. (Koopmans 1996).<sup>11</sup> In addition, the weakness, lack of experience with unconventional protest, and disorganization of the repressive apparatuses in the new federal states have been suggested as explanations (Willems 1994). Probably, a full explanation

<sup>9</sup> Since institutional repression is by definition not directly linked to specific mobilization events, a similar distinction could not be made for this form of repression.

<sup>10</sup> In line with this, most of the Berlin protests occurred in the eastern part of the city.

<sup>11</sup> While extreme right extraparliamentary mobilization and violence in particular are much stronger in the East, parties such as the *Republikaner* and the *Deutsche Volksumion* have a larger membership and have been much more successful in elections in the western states.



**Table 1.** Levels of Extreme Right Mobilization and Repression in the Sixteen *Bundesländer* (aggregate numbers of events per million inhabitants, 1991-1994)

<i>Bundesländer</i> <sup>a</sup>	All extreme right events	Nonviolent events	Violent events	Acts of institutional repression	Institutional repression per event	Acts of situational repression	Situational repression per event
Bremen (W) <sup>b</sup>	2.9	0.0	2.9	8.8	3.00	0.0	0.00
Hesse (W)	4.0	2.1	1.9	3.3	0.83	1.2	0.30
North Rhine-Westphalia (W)	4.0	0.4	3.6	2.8	0.70	1.0	0.24
Baden-Württemberg (W)	4.4	1.6	2.8	4.0	0.91	1.0	0.23
Rhineland-Palatinate (W)	4.8	1.1	3.7	5.3	1.11	1.3	0.28
Bavaria (W)	4.9	2.1	2.8	3.1	0.64	1.4	0.29
Schleswig-Holstein	8.0	1.1	6.8	7.6	0.95	1.5	0.19
Lower Saxony (W)	8.7	1.5	7.3	6.2	0.71	1.9	0.22
Saarland (W)	9.3	0.0	9.3	0.9	0.10	0.9	0.10
Hamburg (W)	9.6	1.8	7.8	4.8	0.50	3.0	0.31
Berlin (E/W)	11.1	2.9	8.2	7.9	0.71	2.9	0.26
Saxony (E)	14.4	3.0	11.4	6.4	0.44	4.9	0.34
Saxony-Anhalt (E) <sup>c</sup>	22.5	3.5	19.0	8.4	0.38	9.8	0.44
Thüringen (E)	23.6	7.0	16.6	9.7	0.41	7.7	0.33
Mecklenburg-Vorpommern (E)	45.6	4.2	41.4	12.1	0.26	14.7	0.32
Brandenburg (E)	46.8	13.3	33.6	21.5	0.46	15.6	0.33

Notes: <sup>a</sup> *Länder* in ascending order of the number of extreme right events

<sup>b</sup> W= former West Germany

<sup>c</sup> E= former East Germany

combines several factors which are difficult to disentangle, especially given the low number of cases. To keep the problem manageable, I will therefore focus exclusively on the relation between repression and mobilization. As table 1 shows, the common idea that repression was less intense in the East does not hold. Both the numbers of situational police interventions and the number of acts of institutionalized repression were higher in the East and the two measures of repression display strong positive correlations with mobilization.<sup>12</sup>

This result is in line with the positive relations found in a large number of cross-sectional studies on the repression-mobilization nexus. However, as I argued above, such correlations partly also reflect the opposite causal effect, namely that of levels of mobilization on levels of repression. To filter out this component, the table also shows *relative* levels of both forms of repression, calculated as the number of repressive acts per act of extreme right mobilization. For situational repression this does not change the picture fundamentally: states with high levels of extreme right activity still display relatively high levels of repression (correlation .47 with the total number of mobilization events). This suggests that the higher levels of mobilization in the East cannot be a result of a lack of repressive police intervention (perhaps they are a result of repression inconsistency, but that cannot be determined here). However, the relative level of institutional repression does turn out to be low in the East, and now correlates negatively with levels of mobilization (-.42 with the total number of mobilization events). Thus, as expected, formal, institutional repression seems to have a stronger deterrent effect than situational police intervention, which even may have had an escalating effect on mobilization.

To investigate the effects of both variables simultaneously, regression analyses were conducted that also allow us to differentiate between relative levels of situational repression against both nonviolent and violent protests. Table 2 shows the effects of these variables on the numbers of events and participants for all mobilization events, nonviolent and violent protests.

**Table 2.** Regression Effects of Different Types of Repression on Extreme Right Mobilization in the Sixteen *Bundesländer* 1991-1994<sup>a</sup>

Types of Repression <sup>b</sup>	All Events	Nonviolent Events	Violent Events	All Participants	Nonviolent Participants	Violent Participants
Situational against Nonviolence	.54+ *	.42	.54**	.57***	.63**	.41**
Situational against Violence	.32	.34	.29	.17	.27	.32*
Institutional	-.02	.03	-.03	-.40**	.05	-.40*
R-squared	.47**	.31	.45*	.80***	.48**	.74***

Notes: \*  $p < .10$ ; \*\*  $p < .05$ ; \*\*\*  $p < .01$ ;  $N = 16$

<sup>a</sup> Numbers of participants are natural-log transformed

<sup>b</sup> Relative levels of situational repression were used in the analysis, i.e., the number of acts of situational repression as a proportion of the number of mobilization events (also see text).

<sup>12</sup> The correlations between the number of mobilization events and situational and institutional repression are .98 and .84, respectively.

The results of the regression analyses confirm the usefulness of the distinction between situational and institutional repression. Situational repression in the form of police interventions against extreme right protests seems to have been an ineffective tactic that only contributed to further escalation. Institutional repression, on the contrary, which directly affects the mobilization capacities of the challenger, seems to have had a deterrent effect on mobilization, although it is limited to numbers of participants. The results also provide support for the legitimacy hypothesis, which states that repression against nonviolent protests will have an escalating effect. Indeed, while situational repression against movement violence had no significant effect on levels of mobilization, repression against nonviolent events is associated with higher levels of five out of the six mobilization variables.

### A TIME-SERIES ANALYSIS OF REPRESSION AND MOBILIZATION

Although the cross-sectional analysis, by using relative instead of absolute levels of repression, takes into account the interactive relationship between repression and mobilization, it does not capture the dynamic aspect of that relationship. The monthly development of mobilization and repression during the period 1991-1994 is shown in figure 1. Extreme right mobilization remained at a low level until September 1991, when the events in Hoyerswerda and the political reactions to them set in motion a strong escalation. A similar escalation occurred about one year later, following the antiforeigner riots in Rostock in August 1992. Although both waves of mobilization subsided quickly, extreme right mobilization has stabilized since 1993 at a level that is considerably higher than the prewave level. Situational repression has closely—though not exactly—followed this development, which is not surprising since this type of repression is directed against concrete events of collective action. Institutional repression, on the contrary, was negligible until after the events in Rostock. The attention these events drew abroad and the actual or perceived damage they did to Germany's image in the international community, were certainly one of the main reasons for this increase in institutional repression. This development was strengthened after the attack in Mölln in November 1992. Since then, institutional repression against the extreme right has remained at a relatively high level.

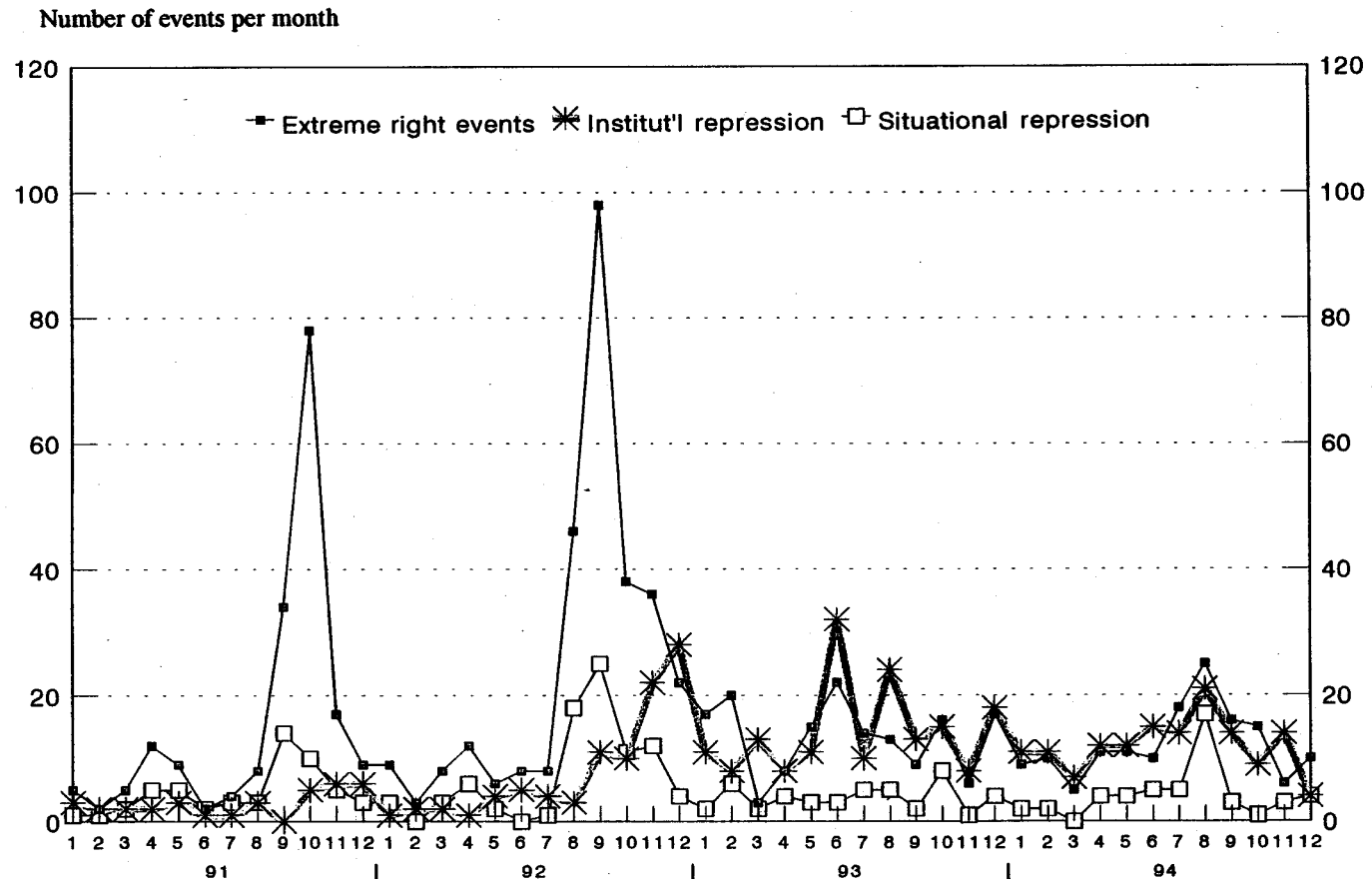
Table 3 shows the results of a time-series analysis of the effects of repression on mobilization. Instead of the levels, the monthly increases or decreases of each of the variables were used (i.e., first-order differences). This strategy is better suited to capture the dynamic nature of the interaction between repression and mobilization and produced higher coefficients than when absolute levels of mobilization and repression were used. All repression variables were in addition lagged one month, so that the coefficients in the table indicate the strength of the effect of changes in the level of repression on changes in the level of mobilization in the following month.

In time-series analyses, one always has to take into account serial correlation in the dependent variable, i.e., the fact that usually the value of a variable at  $t=0$  is partly determined by its value at  $t=-1$ . The most straightforward way to control for such effects is to include the lagged dependent variable among the independent variables. In the case at hand, however, this strategy could not be followed because of multicollinearity problems caused by the high correlations of the mobilization series with the situational repression series.<sup>13</sup> Therefore, a more complicated, two-step procedure had to be followed.

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<sup>13</sup> This is of course a result of the fact that, by definition, occurrences of situational repression depend on the occurrence of mobilization events.

Figure 1. Development of Extreme Right Mobilization and Repression, 1991-1994



Mobilization

**Table 3.** Regression Effects of Different Types of Repression on Extreme Right Mobilization 1991-1994 (Beta coefficients)<sup>a</sup>

Types of Repression <sup>b</sup>	All Events	Nonviolent Events	Violent Events	All Participants	Nonviolent Participants	Violent Participants
Situational against Nonviolence $t = 1$	.12	.05	.11	.06	.09	.09
Situational against Violence $t = -1$	.40***	.22	.44***	.09	.04	-.04
Institutional	-.26*	-.11	-.25*	-.03	.06	-.20
R-squared	.23***	.06	.26***	.01	.02	.05

Notes: \*  $p < .10$ ; \*\*  $p < .05$ ; \*\*\*  $p < .01$ ;  $N = 16$

<sup>a</sup> Numbers of participants are natural-log transformed. All variables are first-order differences. Dependent variables are residuals from ARIMA analyses and have thus been corrected for first-order autoregression (in the case of participants) or first-order moving-average processes (in the case of events).

In a first step, serial correlation was filtered from the dependent variables through ARIMA analyses. The resulting residuals—free from serial correlation—were then, in the second step, used as dependent variables for the regression analyses reported in table 3.

As the table shows, the level of explained variance as well as the regression coefficients are much lower than in the cross-sectional analyses. This may indicate that repression contributes more to explaining *where* than to explaining *when* extreme right mobilization occurred. Partly it may also be a result of the two-step procedure described above. The serial correlation that the ARIMA procedure filters from the dependent variable may partly have been caused by one or more of the independent variables, resulting in an underestimation of the effects of these variables on the dependent variable.

The results confirm the differential effects of institutional and situational repression. As in the cross-sectional analysis, institutional repression tends to decrease levels of (particularly violent) mobilization, although the effect is only significant for numbers of events. Situational repression, on the contrary, tended to increase the number of mobilization events in the following month. Closer inspection of this relation using "moving regressions"<sup>14</sup> revealed that the strength of the escalating effect of situational repression depended particularly on the inclusion of September 1991 and August 1992 in the analysis, i.e., the months in which the riots in Hoyerswerda and Rostock occurred. This indirectly supports the hypothesis that particularly inconsistent repression contributes to escalation. As indicated before, the police and other authorities reacted to these riots with a highly chaotic mixture of repression, nonintervention, and concessions. Moreover, on several occasions the rioters emerged victorious from direct confrontations with the police, who were forced to withdraw as

<sup>14</sup> This method consists of running a series of regressions to portions of the time series, either moving "forward" trying out different starting points for the series, or moving "backward" trying out different end points (see Isaac and Griffin 1989). Although a positive (though not always significant) effect of situational repression on the number of mobilization events could be traced, independent from the start or end dates which were chosen, the magnitude of the effect dropped substantially when the Hoyerswerda and Rostock months were excluded from the analysis.

a result of insufficient manpower, logistical errors, or contradictory orders from above. As a speaker of the Rostock police put it after the riots:

The police did not show strength, but weakness. And when the police show weakness against such violent groups, then you . . . immediately get a stronger willingness to engage in violence on the side of the rioters. . . . This whole episode shows that victories against the police are an important motivation for violent rioters. . . . At the same time we were able to make a lot of arrests. . . . But that has not necessarily had a de-escalating effect on the willingness to engage in violence (cited in Willems et al. 1993: 230).

Finally, on one point the results of the time-series analysis clearly diverge from those of the cross-sectional analysis. The legitimacy hypothesis, which received support from the cross-sectional analysis, is not confirmed by the time series data. Contrary to this hypothesis, repression against violent events contributed more to escalation than repression of nonviolent protests.

## CONCLUSION

In the theoretical part of this paper, three dimensions of repression—its degree of consistency, degree of legitimacy, and the distinction between situational and institutional repression—were suggested as potentially relevant for the assessment of the effects of repression on mobilization. With regard to one of these dimensions the results are highly consistent. As has been suggested by several studies, a distinction should be made between the immediate, situational repression of the police directed against concrete protests, and the indirect, more formalized and usually more consistent repression by higher-level state institutions that affects the mobilization capacities and public legitimacy of challengers more generally and fundamentally. Whereas situational repression tends to contribute to escalation, institutional repression may effectively succeed in lowering the level of protest.

Perhaps the distinction between these two effects is the clue to the effectiveness of totalitarian regimes in reducing the level of protest. On the one hand, such regimes are characterized by an extremely high level of institutional repression, which greatly reduces the mobilization capacities of potential challengers. At the same time, such regimes hardly have to resort to situational repression because protest is usually stifled before it can emerge into the streets. Thus, in authoritarian states, situational repression rarely contributes to escalation. However, once political opportunities open up and protest does emerge into the streets, as happened in Eastern Europe at the end of the 1980s, such regimes suddenly find out that repression no longer works. Attempts to control manifest protest through arrests and police violence are then likely to be counterproductive, unless the regime is prepared (and capable) to crush the challenge with massive bloodshed, as in Budapest 1956, Prague 1968, or Peking 1989.

The importance of the degree of consistency of repressive measures could not be assessed directly in the analyses, since consistency is hard, if not impossible to quantify. The broader literature on the German extreme right in the 1990s has, however, identified the inconsistent and sometimes chaotic reactions of the police and other responsible authorities to several xenophobic riots, and particularly those in Hoyerswerda and Rostock, as an important catalyst of extreme right mobilization. This reading of events suggests that the escalating effect of situational repression demonstrated in the time-series analyses could be attributed to the impact of inconsistent repression during the months in

which the Hoyerswerda and Rostock riots occurred.

The legitimacy hypothesis, however, found no consistent support in the data. The cross-sectional analysis showed, in line with the legitimacy hypothesis, that repression of nonviolent protests contributed most to escalation. The time-series results, however, showed situational repression against violent protests to have the strongest escalating effect. In interpreting these results we should take into account the specific characteristics of the case at hand. To begin with, the action repertoire of the German extreme right was very radical to begin with: three quarters of its collective action were violent, and the majority of nonviolent protests were illegal. Moreover, the movement could hardly claim any legitimacy among the wider public, which rendered public solidarity with the movement unlikely. Clearly one cannot simply compare police violence against peaceful civil rights marchers with similar repression against a demonstration of right-wing extremists displaying nazi symbols and shouting antiforeigner slogans. This underlines the need for a more context-sensitive analysis of the repression- mobilization nexus.

A cautious remark regarding the possibility of generalization of the deterrent effect of institutional repression on mobilization is also in order, at least for democratic political systems. To begin with, institutional repression had a significant deterrent effect on only two out of the six mobilization variables, both in the cross-sectional and in the time-series analysis. Thus, the efficacy of institutional repression was modest, even though extreme right repression in Germany included far-reaching measures that went to the limits of repression within a democratic political context. Organizations were banned not primarily because of their proven involvement in violent or illegal action, but because of their ideas, which were ruled to be "hostile to the constitution." In addition, the wearing of neonazi symbols and the distribution of many extreme right journals and books were banned. In the context of Germany's historic legacy and the deadly violence against foreigners in the 1990s, such measures were seen as justified by a broad majority of the political elite and have found support among the wider public. Whether the same would be true in other European countries is doubtful. Indeed, no other European country has applied similar measures to the extreme right. It is even more doubtful whether such institutional repression would work when applied against other, more peaceful movements with less perverse ideas. If applied to such movements, intense institutional repression may well provoke solidarization with the attacked movement in the name of the defense of civil liberties and democratic values. In other words, perhaps more than the legitimacy of repression, it is the legitimacy of the movement actor that may have a decisive impact on the efficacy of coercion. Since protest as a form of political participation has become widely accepted in democratic societies, the control forces' maneuvering space may in many cases be limited to that form of repression that does not usually work, i.e. the situational, reactive interventions of the police.

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