American Sociological Review

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Contemporary Hate Crimes, Law Enforcement, and the Legacy of Racial Violence

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This article investigates the association between past lynchings (1882 to 1930) and contemporary law enforcement responses to hate crimes in the United States. While prior research indicates a positive correlation between past levels of lynching and current social control practices against minority groups, we posit an inverse relationship for facets of social control that are protective of minorities. Specifically, we hypothesize that contemporary hate crime policing and prosecution will be less vigorous where lynching was more prevalent prior to 1930. Analyses show that levels of past lynching are associated with three outcome variables germane to hate crime policing and prosecution, but the effect of lynching is partly contingent on the presence of a minority group threat. That is, past lynching combined with a sizeable black population largely suppresses (1) police compliance with federal hate crime law, (2) police reports of hate crimes that target blacks, and in some analyses (3) the likelihood of prosecuting a hate crime case. Our findings have implications for research on law and intergroup conflict, historical continuity in the exercise of state social control, and theories that emphasize minority group threat.

Conflict theories of crime and criminal law posit that the state largely serves the interests of dominant groups in society (Quinney 1974; Turk 1969; Vold 1958) and this function can be expressed in two distinct ways. On the one hand, the legal apparatuses of the state—law and law enforcement—can be used to sub-ject members of subordinate groups to punitive control. Research in the group threat variant of the conflict tradition, for instance, suggests that state social control increases in response to perceived threats from subordinate groups (Behrens, Uggen, and Manza 2003; Jacobs and Carmichael 2001; Jacobs and O'Brien 1998; Liska, Chamlin, and Reed 1985). On the other hand, the state can fail to serve the interests of subordinate groups by offering limited protection from harmful and unlawful behaviors. The state can serve dominant group interests not only by administering punitive sanctions against a subordinate group, but also by "looking the other way" when civilians discriminate against subordinate group members.

One of the more egregious examples of such "malign neglect" on the part of the state in U.S. history is the phenomenon of lynching in the late

nineteenth and early twentieth centuries.[1] Lynching "was a powerful tool of intimidation" for the white population to use against the subordinate black population (Brundage 1997:2). Although lynching was not overtly sponsored by the state, lynching incidents were infrequently prosecuted and lynching "was tolerated (and often applauded) by local politicians and law officers" (Garland 2005:810).[2]

Today, there is a growing interest in hate crime as an extra-legal, indeed illegal, behavior that can be conceptualized as a means of social control. Scholars suggest that, much like lynching, hate crimes do not merely victimize particular individuals. Rather, they constitute a means of controlling the behavior of an entire group through intimidation and often violence (Craig 2002; Perry 2001). While such behavior is hardly a new phenomenon, legislation that mandates additional police attention to hate crimes, and criminal statutes that provide enhanced penalties for crimes motivated by bigotry, are relatively recent concepts in U.S. law. Legal protections "on the books," however, are not always implemented in practice (Jenness and Grattet 2005).

We suggest that racial antagonism is deeply ingrained in some pockets of the country, and there is continuity in how the state, via its law enforcement agencies, reacts to offenses motivated by bigotry. Our research investigates the association between legacies of past lynching and contemporary law enforcement responses to hate crimes. We regard the lynching of blacks nearly a century ago as indicative of two conditions pertinent to this research. First, lynching was perhaps the most egregious expression of overt prejudice and demands for white supremacy during the Jim Crow era. Second,

lynching dramatically depicts the state's failure to protect a racial minority group from violent, extra-legal social control.

Mindful of the categorical differences in race relations when comparing the Jim Crow era with the present, we suggest that racial antagonism dies hard and may take on new forms in different historical periods. In line with Bobo and colleagues' (Bobo, Kluegel, and Smith 1997; Bobo and Smith 1998) notion of a shift from Jim Crow to laissez faire racism, we posit that an undercurrent of racial antagonism persists in some locales but is now manifested in less overt actions. The spectacle of lynching has given way to less violent expressions of interracial conflict. The state's past tendency to "turn a blind eye" to anti-black lynching is now expressed through resistance to policies perceived as giving special treatment to racial minorities. A logical extension of this notion is that in places where bigotry was culturally accepted and institutionalized in the Jim Crow era, law enforcement is now apt to resist "affirmative action" policies that give special attention to discrimination against minorities.

Hate crime statutes represent one type of law that is likely seen as giving special protection to racial minority groups. Although the written laws must apply equally to majority and minority group victims alike to remain constitutional (*Wisconsin v. Mitchell* 1993), we argue that hate crime laws are largely protective of racial minorities for two reasons. First, non-white racial groups (particularly blacks) are statistically most likely to be victims of hate crimes (Messner, McHugh, and Felson 2004). Second, the congressional history of hate crime laws in the United States largely entails testimony by aggrieved minority groups and makes reference to heinous acts ranging from the Holocaust to modern anti-gay violence (see Jenness and Grattet 2001, e.g., pp. 35–37, 55).

We argue that past lynching is likely predictive of current policing and prosecution of hate crime laws. Our reasoning is based on three premises: (1) racial antagonism was a powerful social force underlying the phenomenon of lynching, particularly, but not exclusively, in the South; (2) racial antagonism tends to be deeply ingrained in culture (i.e., it dies hard); and (3) the manifestations of racial antagonism evolve over time; specifically, an important manifestation of racial antagonism in the con-

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[1] *Malign Neglect* is the title of Michael Tonry's (1995) book on racial discrimination in criminal justice administration.

[2] We are careful to point out that local politicians and law enforcement were not always supportive of this practice, and volumes of the *American Negro Year Book* from the 1920s and 1930s describe several efforts to prevent lynching. It appears well accepted by scholars of lynching, however, that a significant proportion of the population did not regard the prevention of lynching during this era as a high priority.

temporary era is resistance to special legal protections for racial minorities. We further argue that the impact of lynching is likely to depend on the relative size of the racial minority, consistent with insights from conflict and threat perspectives on social control.

PAST RESEARCH, THEORY, AND HYPOTHESES

CONTINUITY IN SOCIAL CONTROL— LYNCHING AND ITS LEGACY

Prior research shows considerable continuity between past and present levels of social control, and such path dependence is prominently captured in scholarship on past incidents of lynching and contemporary punitive sanctions. Zimring (2003), for instance, finds a statistical association between past lynching and current executions. He suggests that the death penalty is largely an extension of a vigilante tradition, and the racial overtones associated with lynching continue to motivate the legalization and use of capital punishment. To that end, the racial imbalance in capital punishment (Baldus, Pulaski, and Woodworth 1983; Paternoster 1984) is analogous to disproportionate black victimization in past lynchings (Tolnay, Deane, and Beck 1996).

Temporal continuity in social control is also the focus of related work on other forms of law and punishment. Wacquant (2000) views the history of social control in the United States as a succession of institutions that disproportionately subject minority populations to discriminatory punishment. Chattel slavery gave way to Jim Crow, which was then replaced by the "surrogate ghetto" of the modern penal apparatus (Wacquant 2000). Such continuity entails instrumental and emotive characteristics. Punitive control can serve instrumental ends by maintaining existing power arrangements and strategically limiting minority group power (Behrens et al. 2003). Punitive acts may also entail symbolic and emotional attributes. Lynching served partly as a means of moral enforcement (Wyatt-Brown 1982), consistent with sociological arguments that criminal punishment is invested with moral power (Durkheim 1973; Garland 1990). Lynching was not analogous to other homicides, as its purpose was to incite terror and send a message beyond the immediate victims (Tolnay and Beck 1995). This practice was also

laden with the politics of racial domination (Garland 2005), again akin to contemporary ideas on punishment (Jacobs and Carmichael 2002).

The symbolic and cultural framework surrounding lynching is central to recent sociological research that links past lynching with current violence and sanctioning. For instance, Messner, Baller, and Zevenbergen (2005) propose that lynching was indicative of cultural support for violence. They show that lynching exhibits staying power over several decades, evidenced by the correlation between past lynching and current homicide levels in the American South. Jacobs, Carmichael, and Kent (2005) reach a similar conclusion with respect to punitive sanctions. Accounting for other known covariates of death sentences, their research indicates that past lynching is predictive of current death sentences administered against blacks. Similar to Zimring's arguments, they conclude that the "tradition of legal vigilantism" (Jacobs et al. 2005:672) continues to inform the sanctioning process.

While prior research links past lynching with contemporary social control, this body of work focuses almost entirely on violence (e.g., interracial homicide) or state sanctions (e.g., executions) that disproportionately and adversely affect racial minorities. As an extension to this line of research, we suggest that lynching is also predictive of law enforcement actions that are *protective* of minorities, such as the policing and prosecution of hate crimes, but this association should work in the *opposite* direction. It is tenable, we suggest, that the legacy of lynching has two simultaneous effects. First, past lynching may predict current racial antagonism that subsequently manifests in the commission of hate-inspired crime. Second, law enforcement agencies may respond to hate-motivated attacks less vigorously under the same conditions, thereby reducing the likelihood that such incidents will be reported.[3] We are concerned primarily with the latter proposition, which raises two additional questions: What mechanisms connect past lynching and

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[3] While these two outcomes are conceptually distinct, distinguishing between them becomes complicated in empirical assessments. We address this issue in greater detail below.

current law enforcement practices? And what are the specific implications for hate crime laws?

FROM EMBRACING OVERT DISCRIMINATION TO RESISTING SPECIAL PROTECTION

Lynching was a complex social phenomenon, and the literature advances numerous explanations for its prevalence. These include psychological and psychoanalytic accounts, arguments about the reliance on popular or vigilante justice to compensate for the perceived ineffectiveness of legal institutions, cultural interpretations highlighting southern notions of chivalry and honor, and "social threat" accounts emphasizing the political and economic competition between blacks and whites (Brundage 1997; Pfeifer 2004; Tolnay and Beck 1995). Despite disagreements about the relative importance of the various alleged causal factors, the distinctive racial character of lynching is hardly disputed. Beck and Tolnay (1997) estimate that of the more than 2,700 southerners lynched between 1882 and 1930, about 8 out of 10 were blacks brutalized by white mobs. Lynching was more than an effort to enforce social conformity in general on behalf of the community. These acts were extreme expressions of racial antagonism designed to intimidate and control the black minority (Brundage 1993, 1997; Tolnay and Beck 1995).

A second feature of lynching was the "malign neglect" by the state. The rampant lynching of blacks signified the state's conspicuous failure to protect a racial minority. Many lynching incidents were perpetrated with impunity, as law enforcement either tolerated such behavior or acquiesced to it (Garland 2005). For example, some local law enforcement officials appeared to be complicit in Mack Charles Parker's abduction from his jail cell immediately before his lynching in 1959 (Smead 1986). Although lynchings were sometimes prevented, and a few organizations overtly sought to end the practice (Tuskegee Institute 1932), lynching was rarely pursued as a criminal offense. Statutes for arresting and prosecuting lynchers were readily available (e.g., homicide statutes), but as Garland (2005:809) notes,

> laws were rarely enforced against the lynchers. Prosecutions were not brought for lack of political will at the state level, or for lack of coopera-

tion in the local community. In practice, lynchers enjoyed immunity from the state or local prosecution. Anti-lynching campaigners . . . appealed for intervention by the federal government. . . . But this appeal to national law also failed.

The cultural norms that motivated and permitted lynching have clearly changed. Today, lynching rarely occurs in the United States, and when such violent crimes of bigotry are perpetrated, they are likely to be dealt with promptly and severely by law enforcement. The 1998 lynching of James Byrd, for instance, resulted in prosecutions, convictions, and death sentences. The absence of overt Jim Crow racism, however, has not been replaced by ardent support for policies designed to prevent discrimination or compensate for past inequities. Bobo and Smith (1998) refer to this categorical shift in race relations as the movement from "Jim Crow" to "laissez faire" racism. The former refers to "overt bigotry" (Bobo and Smith 1998:185), namely, beliefs that blacks are categorically inferior to whites and the concurrent institutionalization of such sentiments (e.g., segregation). Laissez faire racism rejects the inherent inferiority argument; under this paradigm, whites are likely to attribute inequality to individual shortcomings and to "actively resist meaningful efforts to ameliorate America's racist social conditions and institutions [e.g., affirmative action policies]" (Bobo and Smith 1998:186).

The relevance of Bobo and colleagues' ideas in the realm of criminal law is nicely illustrated in Behrens and colleagues' (2003) work on the changing justifications for felon disenfranchisement laws during and after Jim Crow. In the course of debates about felon disenfranchisement in the Jim Crow era, judges and legislators made reference to the enrichment of Anglo-Saxon civilization, and they openly labeled blacks as "menacing" and inherently different from whites (Behrens et al. 2003:570). Significant support still exists today for felon disenfranchisement laws, but they are now justified by race-neutral arguments against repealing laws that disproportionately affect blacks (pp. 570–72).

We advance a similar argument with respect to law enforcement's role in responding to crimes of bigotry, or what are now commonly referred to as hate crimes. Prosecutions following the lynching of blacks were spotty and

convictions "nearly nonexistent" (Klarman 2000:55) during the Jim Crow era. We expect that in counties with a history of lynching a century ago, there is now a greater proclivity to resist policies that might give special treatment to blacks. In this regard, we draw an analogy with prior research on police use of violence. As Jacobs and O'Brien (1998:842) explain with respect to why police would use more violence against blacks under conditions of minority threat, "it is not necessary to claim that privileged groups make direct demands that the police use greater amounts of deadly force. Without restraints, police violence is probable." When privileged groups are threatened, the powerful are less likely to interfere with police tactics, opening the way for more police violence.

With respect to hate crime law, the situation is in some sense reversed. Compliance with and enforcement of hate crime laws place added burdens on police officers and prosecutors. Recording offenses requires complicated judgments about motives, which makes proving offenses more difficult. To the extent that racial antagonism is manifested in resistance to special protections for racial minorities, skepticism about the legitimacy of hate crime laws (i.e., Why should there be special statutes designed largely to protect minorities?) and indifference to enforcement of such laws, or compliance with them, is increasingly likely. Furthermore, without support from the general population, police and prosecutors are likely to follow the path of least resistance and infrequently put these laws into action.

We investigate the following general hypothesis: *the contemporary enforcement of hate crime laws and compliance with hate crime mandates are inversely associated with prior levels of lynching.* Another possibility, which we explicate below, is that the legacy of lynching matters only in the presence of a minority group threat.

RACIAL THREAT AND HATE CRIME LAW

Theories that emphasize racial threat view law as an instrument for subverting economic and political challenges posed by racial minorities. In line with classic explanations of intergroup relations (Blalock 1967), the racial threat thesis postulates that majority population and elites perceive a large or growing racial minority group as threatening. Such threats incite a variety of reactions from majority groups, such as right-wing voting (Giles and Buckner 1993), prejudicial attitudes (Quillian 1996), and expansive state social control (Jackson 1989).

Racial threat is implicated in our assessment of lynching and hate crime law for two reasons. First, black population size as an indicator of racial threat is likely to be predictive of hate-crime law enforcement. Research is mostly supportive of racial threat tenets in the realm of criminal law, as evidenced in work on arrest rates (Liska et al. 1985), policing expenditures (Jackson 1992), and the legalization of capital punishment (Jacobs and Carmichael 2002). These findings are generally congruent with research that connects black population size to prejudice toward blacks in the contemporary United States (Quillian 1996) and to lynchings a century ago (Tolnay et al. 1996).

We propose that if black population size increases both prejudice and social control that adversely affects racial minorities, it may *decrease* social control perceived as protecting minorities. This argument is partly supported by research that finds less compliance with federal hate crime laws in southern cities and counties with large black populations (King 2007). In line with that work, and in concert with research on the state's neglect of blacks' legitimate crime control needs (Anderson 1997, 1999; Hawkins 1987), we hypothesize that black population size is inversely associated with hate crime law enforcement and compliance with federal hate crime laws.

Beyond this direct link between racial threat and hate crime laws, threat may also *condition* the effect of lynching. In their work on capital punishment, Jacobs and colleagues (2005:660) suggest that "in the absence of a black population sufficiently large to pose a current threat, a prior tradition of vigilantism [as indicated by lynching] may not be enough to lead to additional death sentences." To wit, past lynching only increases the contemporary use of capital punishment in areas with sizeable black populations. It is reasonable to expect that racial composition might also modify the link between lynching and the state's protective functions for racial minorities, which implies a statistical interaction. Specifically, we hypothesize that

the association between hate crime law enforcement (including compliance with hate crime law mandates) and lynching becomes increasingly negative as the size of the black population increases.

POLITICS, INTERGROUP CRIME, AND HATE CRIME LAW ENFORCEMENT

Extant theory and research dictate that our analysis account for political partisanship. Politics is an increasingly salient factor in the study of punishment (Beckett 1997; Sutton 2000). In particular, support for conservative politicians is linked with elevated use of criminal punishment (Jacobs and Carmichael 2002; Jacobs and Helms 1996, 1999). Yet conservative politics may decrease hate crime reporting (McVeigh, Welch, and Bjarnason 2003). All our models thus include a measure of political conservatism.

Explanations of state responses to hate crimes must also confront the notion that hate crime law enforcement simply reflects the level of intergroup crime in a jurisdiction. This "reactive explanation," commonly put forth as an alternative to the threat perspective (Jacobs and O'Brien 1998), suggests that hate crime policing and prosecution are direct functions of the level of intergroup crime motivated by animus. We thus test our hypotheses concerning lynching, racial threat, and law enforcement responses to hate crimes while accounting for the level of intergroup violence (to the extent possible with the available data).

DATA, MEASURES, AND METHODOLOGY

SAMPLE, UNITS OF ANALYSIS, AND THE LYNCHING VARIABLE

Our samples and units of analysis are determined by the strategic independent variable—lynching. We assess the effect of past lynching on current law enforcement responses to hate crimes using two different samples and three outcome variables. First, we use rich lynching data for a sample of counties and county clusters in 10 southern states: Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, South Carolina, and Tennessee. County-level lynching data for these states come from Tolnay and Beck's inventory of southern lynching.[4] Tolnay and Beck (1995:259) developed their inventory by first creating an "unconfirmed master list" of all lynchings that occurred within these states, as reported in previously used and publicly available inventories. They then validated these incidents through meticulous cross-checking with newspaper stories "to determine whether the event truly was a lynching and to correct for factual errors that may have been included in the original inventories" (p. 260). Their final product is "a confirmed inventory" of lynching in the South from 1882 to 1930 (p. 260). Tolnay and Beck's register is the "gold standard" for research on lynching. Our primary analyses are thus based on the 10 southern states for which these data are available.

As noted, counties serve as the focal geographic units. However, because county borders have changed over the past century and thus current boundaries do not always mirror the period from which our lynching data are generated, we aggregate adjacent counties into clusters in some cases. We created clusters in accordance with the Horan and Hargis (1995) county template, which includes an aggregation key indicating which counties should be combined to account for boundary changes that occurred between 1880 and 1990. To retain as much geographic detail as possible, we do not replace individual counties with county clusters in two situations: (1) when a county cluster had no lynching activity and (2) when such activity occurred only after boundary changes were completed. In these cases we can determine whether a modern county experienced a lynching between 1882 and 1930; where no lynchings occurred, there is no reason to create county clusters. This "southern sample" consists of 726 counties and county clusters.[5]

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[4] Stewart E. Tolnay and E. M. Beck generously provided the lynching data. For a description of data collection methods, see Tolnay and Beck (1995).

[5] In supplementary analyses, we created additional clusters and reestimated the models. We did this because some law enforcement agencies are covered by other agencies in a different county. For example, a few agencies report no crime data to the FBI, but instead send their data to an agency in another county that submits those data, plus their own, to the FBI. We deal with this in the current analysis by controlling for general crime reporting as a predictor vari-

Because our theoretical argument concerns the legacy of lynching, which constitutes a violent act laden with symbolic power of domination and suppression (Garland 2005), we measure the lynching count rather than the lynching rate. The symbolic significance of lynching is unlikely to be diluted in citizens' minds by the proportionate representation of lynching in the immediate area (cf. Tolnay et al. 1996). Moreover, we include only those lynching incidents that had one or more black victims. This maintains continuity with our theoretical argument, which concentrates on the racial antagonism associated with lynching. Our main independent variable thus depicts the frequency of lynchings with black victims in county clusters from 1882 to 1930.

Although we focus primarily on the 10 southern states identified above (because of the high quality of the lynching data), we supplement this set of regional analyses with an assessment of lynching and law enforcement responses to hate crimes for all counties in the United States. Replicating the analysis of 10 southern states speaks to the robustness and generalizability of our findings, with the caveat that the national lynching data are of lesser quality outside the states in the Tolnay–Beck sample.[6] For this "national sample," we catalogued all lynching incidents with black victims for counties outside the aforesaid 10 southern states using data from the NAACP's 1919 publication, *Thirty Years of Lynching in the United States, 1889–1918*. This publication's goal was to compile detailed information on lynching incidents that had been reported in various sources, such as the *Chicago Tribune* and the Tuskegee Institute, and to publish these "facts" about lynching without editorial commentary. Appendix II of the publication is a chronological listing of persons lynched in the United States between 1889 and 1918. Nearly all the cases include information on the race of the victims and the city (and typically county) of the lynching.[7] We aggregated

the NAACP data to the county level and merged these data with the counts based on the Tolnay and Beck inventory to create a national sample. As with the 10-state sample, we formed clusters in other states using the Horan and Hargis template. The Appendix lists counties that were not formed into clusters because all their lynching activity occurred after modern boundaries were in place. Some clustering was also required to link data for 1990 and 2000 (e.g., in Alaska). These counties are also listed in the Appendix.

Because the NAACP data are available for a period of 30 years, whereas Tolnay and Beck's inventory encompasses 49 years, we annualized the lynching counts for the supplementary analyses of all U.S. counties. That is, we divided the NAACP lynching count by 30 and the Tolnay and Beck count by 49 to standardize for the number of years for which data were recorded. We are thus able to examine whether any effect of lynching observed in the southern sample can be replicated in a national sample of counties and county clusters.

DEPENDENT VARIABLES

Crime statistics can reflect the actual amount of criminal offending or the reporting practices of law enforcement agencies (Black 1971; Kitsuse and Cicourel 1963); more likely, it will be some combination of the two. This duality with respect to crime statistics complicates our inquiry because we cannot know whether hate crime data furnished by the government reflect the true number of offenses motivated by bigotry (as used by Medoff 1999) or law enforcement's willingness to execute hate crime statutes (the approach adopted by McVeigh et al. 2003). Mindful of this "realist versus constructivist" debate, we use three separate dependent variables that cover two primary institutions of for-

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able, but there is no substantive difference in the findings when dealing with the issue via additional clustering. The results are consistent for the 10-state and national samples (described below).

[6] See Appendix B in Tolnay and Beck (1995) for an extended discussion of the types of errors often found in publicly available inventories on lynching.

[7] We recorded 494 lynching incidents with black victims (outside the 10 southern states described

above) from the NAACP (1919) publication. In eight cases, no city or county information was provided, and thus we could not code the geographic location of those incidents. In 18 cases, the NAACP listed a city but not a county. With the assistance of secondary sources (namely Abate 1974; Forstall 1996; Gannett 1902, 1975), we found the corresponding county in two of those cases. We dropped nine other incidents because the county identifier in the NAACP data appears to be wrong. In all, we have county identifiers for 461 incidents. The Appendix provides a full list of changes we made to the NAACP data.

mal social control—police departments and district attorneys' offices.

The police represent the gateway to the criminal justice system, and crimes not identified by the police are not likely to receive attention from other law enforcement agencies. Bell (2002), for instance, reports that police investigations of hate crimes have notable implications for charging decisions made later in the adjudication process. We thus measure hate crime policing via two measures. Our first policing variable measures *compliance with the Hate Crime Statistics Act* (HCSA). The HCSA requires the Justice Department to acquire and publish data about crimes that manifest prejudice based on certain group defining characteristics, including race, ethnicity, sexual orientation, and disability (see Public Law 101-275, section b(1); also Public Law 103-322 and Public Law 104-155). Compliance with the HCSA is recognized as a first step toward policing hate crimes (Jenness and Grattet 2001), and it is viewed as "critical" for hate crime law enforcement (U.S. Department of Justice 2002). Such compliance indicates a basic recognition of the law, even if a law enforcement agency reports no hate crime offenses. To that end, investigating compliance with the HCSA has the advantage of making no assumptions about the prevalence of offending motivated by bigotry in an area, because policing agencies can comply with the law even if they report no hate crime incidents. We suggest that failure to comply with the HCSA data collection mandate, particularly when compliance with other federal crime reporting initiatives is in evidence (general *Uniform Crime Report* reporting), signifies a resistance to or ambivalence about the hate crime law. In addition, this variable is useful because the HCSA applies to all policing agencies in the United States, regardless of state statutes, although compliance is both voluntary and highly variable (McDevitt et al. 2000).

We focus primarily on sheriffs' departments in this analysis because the lynching data are organized at the county level. Sheriffs largely correspond to counties and, with some exceptions, their responsibilities include law enforcement and crime reporting. Using smaller units of analysis, such as municipal policing agencies within counties, would be problematic because

our focal independent variable, lynching, is measured at the county (or county-cluster) level.

In singular counties not grouped into clusters, we measure the compliance variable by the number of quarters that the sheriff's office submitted hate crime reports in compliance with the HCSA between 1992 and 2003.[8] Where counties are clustered because of border changes since 1880, we take the average number of compliant quarters for the cluster.[9] That period, 1992 to 2003, corresponds to the first year that data were generated as a result of the HCSA and ends with the most recent available year when this analysis began. There is noticeable variation in the degree of compliance during our period of study (see Table 1 for descriptive statistics), with several departments failing to comply for the duration of the study. We suspect that compliance with the federal hate crime law is not randomly distributed; rather, we expect it to covary with historical lynching activity and racial demographics.

While the compliance variable has the advantage of making no assumptions about the actual prevalence of hate crimes, it fails to capture the degree of actual hate crime reporting. That is, policing agencies could ceremonially "comply" with the letter of the law without recording a single hate crime offense. We thus include a second outcome variable that measures *the number of anti-black motivated hate crimes reported by police* in the county or county cluster. We include hate crimes reported by sheriffs' departments and local policing agencies within the county cluster because the data can be meaningfully aggregated to the county (or county-cluster) level.[10] Since we focus partly on the concept of racial threat, and because our lynching variable is used to indicate historical tolerance for extralegal violence against blacks, our "hate crime

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[8] The Department of Justice requests that law enforcement agencies submit quarterly hate crime reports. Our data cover 12 years; thus there are 48 quarters in which law enforcement agencies were "at risk" of compliance (12 years $\times$ 4 quarters per year).

[9] See the Appendix for exceptions and additional notes about the law enforcement agencies.

[10] We exclude hate crimes reported by state patrols or regional police.

Table 1. Descriptive Statistics for Southern Sample (N = 726)

	Mean	SD	Min.	Max.
Dependent Variables				
Compliance with federal hate crime law	20.2	13.41	0	45.00
Racially-motivated hate crimes that target blacks, as reported by police[a]	3.53	20.22	0	480
Hate crime prosecution in 2000 (N = 674)	.18	.38	0	1.00
Independent Variables				
Lynching	3.37	5.53	0	61.00
Percent black (1990)	20.17	18.86	0	86.12
Percent black (2000)	20.31	19.32	0	86.13
Population logged (1990)	10.32	1.12	7.55	15.62
Population logged (2000)	10.45	1.14	7.64	15.85
Percent urban (1990)	32.04	26.65	0	99.96
Percent urban (2000)	35.54	28.13	0	99.90
Percent divorced (1990)	9.93	1.62	4.80	19.08
Percent divorced (2000)	12.46	1.50	7.23	19.79
Percent ages 15 to 29 (1990)	22.44	3.35	14.03	47.41
Percent ages 15 to 29 (2000)	20.20	3.36	12.94	44.46
General crime reporting to UCR – dummy measure	.97	.16	0	1.00
General crime reporting to UCR – ordinal measure	1.34	.53	0	2.00
Percent voting for Bush in 1992	40.88	9.34	12.94	74.96
Percent voting for Bush in 2000	54.98	10.72	12.35	84.02
Black county commissioners per 100,000 (logged)	1.58	1.97	0	6.79
Any elected black sheriff	.06	.23	0	1.00
White on black homicides	.56	3.42	0	83.00
Percent born in state (1990)	75.78	13.45	17.22	97.24
Percent born in state (2000)	72.05	13.45	19.42	96.51
Percent black officers in county cluster (1990)	16.78	21.26	0	100
White unemployment (1990)	5.80	2.43	1.37	18.39
White unemployment (2000)	4.80	1.71	.39	12.93
White poverty (1990)	14.86	6.55	2.29	52.20
White poverty (2000)	12.63	5.18	2.40	45.31
Black–white unemployment ratio (1990)	2.7	1.13	0	11.75
Black–white unemployment ratio (2000)	2.91	1.89	0	26.84
Black–white poverty ratio (1990)	3.11	1.05	.78	8.96
Black–white poverty ratio (2000)	2.85	1.00	.56	9.38
Years under NIBRS	2.29	4.29	0	14
Police per thousand (agency – Table 2 only)	.73	.62	.02	7.45
Police per thousand (county cluster – Table 3 only)	1.55	.75	.2	7.45
Hate crimes reported in 2000	.9	5.25	0	121

Note: Missing data on the hate crime prosecution variable decreases the valid N to 674. The N is also smaller for the following variables that were included only in analyses of counties with 500 or more black residents: percent black officers in the county, black–white unemployment ratio, and the black–white poverty ratio.

[a] The maximum value is an outlier, which is omitted from analysis, although the results are consistent when it is included.

reporting" outcome variable focuses specifically on the number of hate crimes that target blacks, as identified by police. This dependent variable represents a count of the crimes motivated by animus toward blacks as reported by policing agencies between 1992 and 2003. As depicted in Table 1, this variable includes a tremendous range of hate crime reporting activity. During the 12 years under investigation, the number of anti-black hate crimes reported in the 10 southern states varies from zero in some counties to a high of 480.[11]

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[11] The high value represents an outlier and corresponds to a cluster of counties. We omit that case when estimating the effects of our predictor variables on the outcome of hate crime reporting, although the results are consistent when including the case. The same is true for the supplementary analy-

Our assumption is that higher levels of hate crime reporting, in part, indicate greater attention by police to identifying and formally reporting potential anti-black hate crime incidents.[12] Still, we acknowledge that the interpretation of this variable is somewhat problematic because police identification of hate crimes to some extent reflects the actual volume of hate crime offenses. This is, in part, why we examine multiple dependent variables, although we offer two additional arguments in support of using this measure. First, our models include a measure of interracial violence perpetrated by the dominant racial group against a racial minority—the number of white-on-black homicides. To the extent that this measure is correlated with the level of racially-motivated hate crimes, the confounding of offending with police reporting practices should be reduced. Second, we note that the main effect of lynching on anti-black hate crimes may be positive if the variable measuring reported anti-black hate crimes captures some actual crimes of bigotry, although it would be consistent with our account if the interaction term for percent black and lynching is negative.

Because policing captures only one institutional facet of law enforcement, we include an additional variable germane to prosecution. Prosecution is arguably a more proximate indicator of law enforcement than is compliance with federal reporting statutes or overall hate crime reporting. We estimate the likelihood of a hate crime prosecution, which we measure by the presence (coded 1) or absence (coded 0) of a single hate crime prosecution during a one-year period beginning in 2000.[13] An additional analysis of the number of hate crime prosecu-

tions would be desirable, but data limitations confine our analysis of this dependent variable to the presence or absence of a prosecution. The only source containing data on prosecutions for all U.S. counties is the 2001 National Prosecutors Survey (NPS; conducted by the U.S. Department of Justice.) The NPS is a biannual survey that monitors trends in prosecution. The 2001 survey is a full census of all district attorneys' offices that operate in state courts with felony jurisdiction in the United States. Among the information included in this survey is whether a district attorney's office prosecuted special categories of crime, including hate crime. For the year in question, 18 percent of the offices in our southern sample prosecuted one or more hate crime cases (20 percent in the national sample). We are unable to disaggregate this measure into types of hate crime prosecution (e.g., racial motivation, religious motivation), so this variable includes some additional error compared with the previous two measures. It would, however, be consistent with our theory if past lynching, particularly in combination with current racial threat, decreases the likelihood of hate crime prosecution.[14]

ADDITIONAL INDEPENDENT VARIABLES

In addition to lynching, a second focal independent variable concerns racial threat. Consistent with prior work on prejudice and punishment, we measure racial threat as the percentage black in a county or county cluster. Because our hate crime policing data begin in 1992, we use 1990 U.S. Census data on racial composition for the policing dependent variables and 2000 data for the prosecution outcome variable. We also include a product term for lynching and percent black to test for an interaction effect. We note, however, that the statistical interaction may have different implications for the respective dependent variables. When

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sis of all counties in the nation, where one case exceeds a value of 2,000. We removed that case from the analysis, but there is no meaningful change in the coefficients and standard errors when it is included.

[12] McVeigh and colleagues (2003:846), for instance, suggest that hate crime reports "reflect different incentives to call acts of bias to the attention of local authorities, as well as different incentives that influence law enforcement agents to respond to, and report, hate crimes."

[13] The exact one-year period could vary slightly depending on the date that the National Prosecutors Survey was actually completed, but this period covers 12 months in late 2000 and early 2001 for all offices.

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[14] We coded clustered counties 1 if any of the counties were under the jurisdiction of a district attorney's office that prosecuted a hate crime. The NPS data include some missing cases on the hate crime prosecution measure, thus reducing our sample size. We dropped counties in some states from the supplementary analysis of prosecution in the national sample; those cases and the justifications for dropping them are discussed in the Appendix.

assessing compliance, we expect the effect of lynching to become increasingly negative as the percent black increases. For our measure of anti-black reported hate crimes, where the main effect of lynching is likely positive, we expect this effect to be tempered in the presence of a large black population because of suppressed reporting practices. We thus expect consistency in the direction of the interaction coefficients even if specific tipping points may differ. A negative interaction coefficient would be particularly revealing compared with recent work on other types of social control. Whereas the interaction between lynching and percent black was found to be *positive* for types of social control that are punitive against blacks (Jacobs et al. 2005), we expect that the same interaction is negative for social control perceived as protective of blacks.

Our analysis also accounts for three indicators of political control. First, all models control for the general level of political conservatism. For the policing dependent variables, we measure political conservatism as the percentage of a county or county cluster that voted for President George H. W. Bush in the 1992 presidential election. This indicator provides a standardized measure of conservatism since all eligible citizens chose from the same slate of presidential candidates. Because our prosecution measure pertains to the years 2000 to 2001, this set of analyses uses the percentage voting for George W. Bush in the 2000 election as a comparable indicator of conservatism. Second, we use the number of black elected county commissioners per 100,000 residents as an indicator of black political representation.[15] Third, all models include a dummy variable indicating that the county, or at least one county in a cluster, had a black elected sheriff at some point between 1994 and 2002. These latter two measures are reasonable indicators of black political control, or at least of an under-

lying propensity to elect blacks to lead local political institutions, which could potentially mediate the purported effects of black population size and lynching.

It is also imperative to control for characteristics of law enforcement agencies in the respective counties. We control for a sheriff's department's propensity to comply with *any* data collection mandates when modeling the compliance dependent variable. Using county-level *Uniform Crime Report* (UCR) data furnished by the FBI, we employ two control variables to account for that propensity. First, we coded a general crime reporting dummy variable as 1 if a sheriff's department submitted any crime data in accordance with the UCR for 1992 to 2003; we coded complete abstainers 0. Where counties are clustered, we coded the cluster as 1 if any of the sheriffs' offices reported crime information. Second, we include an ordinal variable consisting of three categories: agencies that reported no data for the UCR ("abstainers"), those that reported some of the time ("partial reporters"), and finally departments that always reported general crime information to the UCR ("consistent reporters"). Our assumption is that this measure will account for a general proclivity to report crime information and should be strongly and positively correlated with the compliance dependent variable. We can then assess the effects of lynching and black population size on hate crime law compliance net of the propensity to report general crime data.[16]

Beyond these agency indicators, we also measure the number of years that an agency has submitted crime data as part of the National Incident Based Reporting System (NIBRS). Because hate crime reporting is structurally a part of that system, there should be a positive correlation between participation in NIBRS and hate crime reporting. We calculate this variable by subtracting the year an agency began the NIBRS program from 2004 (agencies not participating are coded 0). In addition, we use data from the U.S. Department of Justice to measure

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[15] Specifically, we counted the number of black elected commissioners in a county or county cluster for each year between 1994 and 2002, standardized that measure by population size (in 1990), and then logged the variable to reduce extreme skew (we added a constant of 1 before logging). Data for black commissioners and black elected sheriffs were provided by the Joint Center for Political and Economic Studies. Data are not available for 1992 and 1993.

[16] As described below, we use the dichotomous "general crime reporting" variable only to predict "zero-counts" in our zero-inflated negative binomial model. We use the ordinal measure to predict the actual number of compliant quarters.

the number of officers per capita in the policing agency (for the compliance outcome variable) or the number of police officers per capita in the county (for hate crime reporting) as controls for law enforcement capacity.[17] It is plausible that larger policing agencies are better able to absorb additional data collection responsibilities. Finally, in some models restricted to counties (or clusters) with 500 or more black residents, we control for the percentage of black police officers using data from the 1990 U.S. Census Equal Employment Opportunity File (ICPSR 9929). It seems reasonable to expect that the policing of hate crimes against a racial minority group will positively correlate with the representation of this group in law enforcement.

We also control for several other potential correlates of hate crime policing and prosecution.[18] All analyses control for the logged population of the county or county cluster, along with the percent urban. In addition, we account for the percent divorced and the percentage of the population age 15 to 29 because, in theory, these proxy measures of social disorganization could influence police investigation practices (Borg and Parker 2001). We also control for the percentage of whites in the civilian labor force who are unemployed and the percentage of whites below the poverty line because it is plausible that any effect of racial threat could be attributed to economic competition and adverse economic conditions for the majority group (data are from the 1990 and 2000 U.S. Censuses).

In some models restricted to counties with more than 500 black residents, we further test whether black/white unemployment and poverty ratios are consequential. We calculate these ratios as the percentage of the black population unemployed divided by the percentage white

unemployed and the percentage of blacks in poverty divided by the percentage of whites in poverty, respectively. For each indicator, higher values indicate that blacks do worse relative to whites. An additional control variable measures the percentage in a county (or cluster) that was born in the state of residence. This measure is potentially important because cultural traditions may change appreciably where there is significant population turnover.

As mentioned above, we also include a proxy measure of interracial criminal violence, as indicated by the number of white-on-black homicides using data from Fox (2005). Following standard practices, this measure refers to murders and non-negligent manslaughters, as recorded by police in the FBI's *Supplementary Homicide Reports* (SHR), that occurred in 1986, 1987, 1992, 1993, 1994, and 1995. We use these years to account for the fact that police in Florida provided no information to the FBI from 1988 to 1991.[19] We exclude homicides recorded by state police departments because the county of incident is often missing or may be incorrect. Furthermore, to create an unambiguous measure of interracial homicide, we exclude incidents involving more than one victim or offender (Williams and Flewelling 1988).[20] Since both ethnic heterogeneity and the legacy of lynching predict interracial violence (on lynching, see Messner et al. 2005; on heterogeneity, see South and Messner 1986) and formal social control (on lynching, see Zimring 2003; on race, see Jackson 1989), a viable alternative hypothesis is that racial threat and

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[17] Data are missing for a small number of cases (six in the southern sample and 68 in the national sample). We used mean substitution for these cases. Additional models that omit these cases from analysis yield the same substantive results and are available from the authors upon request.

[18] Given the different time frames for our policing and prosecution dependent variables, we measure the control variables at 1990 for policing and at 2000 for prosecution, unless otherwise noted.

[19] Agencies in Kentucky contributed no data to the *Supplementary Homicide Reports* in 1988. From 1996 to 2003, agencies in Florida provided no data to the FBI, so we use the same 1986 to 1995 measure of white-on-black homicides for the analysis of prosecution. Finally, results presented below hold when white-on-black argument-related homicides, or those that occurred in the context of lovers' triangles, alcohol-induced brawls, narcotics-induced brawls, arguments over money or property, or other arguments, are controlled instead.

[20] The homicide measure does not reflect Fox's adjustment for missing data. In a personal communication with one of the authors, Fox (January 2006) advised that his weighting scheme not be used in county-level analyses.

the legacy of lynching increase interracial violence, which in turn influences state responses to interracial crimes. We test the impact of lynching and race on our outcome variables net of our indicator of interracial violence.

When using hate crime prosecution as the dependent variable, we also control for the number of hate crimes reported by police in a county (or cluster) for the year 2000. This control variable is important because police reporting of hate crimes is likely to be consequential for prosecution.

The policing and prosecution of hate crimes could also be influenced by state laws or directives from state agencies, such as the respective attorneys general. To account for potential state-level influences, we include dummy variables for all states, with one state omitted as a reference category. By fixing the state effect, we essentially investigate within-state variation in our outcome variables of interest.

METHODS

We use binary logistic regression to assess the effects of our predictor variables on our dichotomous outcome variable—hate crime prosecution. The respective measures of hate crime law compliance and police reports of racially-motivated hate crimes are continuous, but also highly skewed, and they each include multiple zeros. Ordinary Least Squares regression may be problematic when the distribution is positively skewed and heteroskedasticity is present. However, Poisson-based estimators can provide nonbiased estimates for positively skewed event counts (Osgood 2000). We use negative binomial models because overdispersion is present in both variable distributions.

We estimate the models for hate crime law compliance using zero-inflated negative binomial regression because the distribution includes many zeros and several of these cases are systematically related to an agency's general crime reporting practices (i.e., non–hate crime reporting to the FBI). As Cameron and Trivedi (1998) spell out in detail, and Jacobs and Carmichael (2004) illustrate for the case of death sentences, the zero-inflated procedure is suitable for a distribution with many zero-counts and where some cases are at reduced risk, or no risk at all, of experiencing an event, such as law enforcement agencies that generally do not report crime

data.[21] The zero-inflated routine simultaneously estimates two models: one on the likelihood of a zero-count and another on the expected count of the outcome variable. When modeling zero-counts, we rely on a single control variable presumed to be strongly correlated with the absence of hate crime reporting—an agency's general crime reporting record. Agencies that generally fail to report crime data are not expected to report hate crime data, but for reasons that may have little to do with racial threat and the legacy of lynching.

We use both the negative binomial and the zero-inflated models for the analysis of anti-black hate crimes reported. The models yield nearly identical substantive results, but the zero-inflated model is more appropriate for one analysis, as explained below.

RESULTS

We begin with an analysis of compliance with the HCSA, measured by the number of quarters a department reported hate crime data for 1992 to 2003 (Table 2). Model 1 reports estimates for additive effects based on our zero-inflated negative binomial model; Model 2 includes the interaction term for lynching and racial composition; and Model 3 adds race-specific measures such as the black–white unemployment and poverty ratios. Intuitively, the results in Model 1 indicate that the degree of hate crime law compliance increases with the degree of general crime reporting to federal authorities (b = .191), and the likelihood of reporting zero hate crimes decreases substantially if an agency reports general crime data (b = –4.441). The number of compliant quarters also increases with the length of participation in the NIBRS program (b = .082). Our measure of law enforcement capacity (police officers per capita) is significantly and negatively correlated with the outcome variable, which may appear counter-intuitive. Notably, the correlation is positive and significant in models that omit the state dummy variables. One plausible interpretation is that such counties are more invested in puni-

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[21] These agencies would technically still be at risk, but prior research suggests the likelihood of reporting hate crimes is highly dependent on general crime reporting (King 2007).

Table 2. Zero-Inflated Negative Binomial Regression Coefficients: Number of Quarters that County Sheriffs' Departments Submitted Hate Crime Reports Regressed on Predictor Variables

	Model 1	Model 2	Model 3
Lynching of blacks	−.005	.004	.004
	(.003)	(.005)	(.006)
Percent black	−.0001	.0006	−.0007
	(.001)	(.001)	(.002)
White unemployment	−.006	−.006	
	(.008)	(.008)	
White poverty	−.010*	−.010*	
	(.003)	(.003)	
Black–white unemployment ratio			−.008
			(.019)
Black–white poverty ratio			.042*
			(.019)
Logged population	−.004	−.004	−.014
	(.022)	(.022)	(.026)
Divorce rate	.004	.005	−.0007
	(.009)	(.009)	(.011)
Population ages 15 to 29	−.002	−.001	−.001
	(.004)	(.004)	(.005)
Percent urban	.0001	.00004	.0003
	(.0008)	(.0008)	(.001)
Crime reporting (ordinal)	.191*	.189*	.237*
	(.028)	(.028)	(.033)
Voting for Bush in 1992	−.002	−.002	.0005
	(.002)	(.002)	(.002)
White-on-black homicides	−.001	−.002	−.0006
	(.003)	(.003)	(.003)
Percent born in state	−.001	−.0008	−.0002
	(.001)	(.001)	(.001)
Years reporting NIBRS	.082*	.081*	.062*
	(.016)	(.016)	(.020)
Percent black officers			.003*
			(.001)
Police officers per capita	−.073*	−.071*	−.099*
	(.026)	(.026)	(.036)
Elected black county commissioners	−.004	−.004	−.0009
	(.010)	(.010)	(.011)
Black county sheriffs	.025	.045	.036
	(.058)	(.058)	(.062)
Lynching of blacks × percent black		−.0003*	−.0003
		(.0001)	(.0002)
Constant	3.909*	3.835*	3.572*
	(.261)	(.263)	(.331)
Zero-value			
Any UCR reporting	−4.441*	−4.440*	−4.145*
	(.602)	(.602)	(.612)
N	726	726	562
χ^2 / df	892.41 / 25	896.95 / 26	699.05 / 27

Notes: The state dummy variables, which are included in the model but not shown in the table, are statistically significant, indicating that state effects account for part of the variation in compliance. Many predictor variables, such as political conservatism (−), percent black (−), percent born in state (−), and population size (+) are significant when the state dummies are omitted from the model. Police force size is positive and significant when state dummies are omitted. Standard errors are in parentheses.

* $p \leq .05$ (all tests two-tailed).

tive social control and concomitantly less apt to comply with civil rights–related laws.

The results for the lynching measure and its interaction with racial composition are of particular theoretical interest. Net of the control variables, Model 1 shows that past lynching is negatively associated with hate crime law compliance, albeit at a more permissive level of statistical significance (b = −.005; p < .10, two-tailed). Counties and county clusters with frequent lynchings in the past are less likely to comply with the HCSA. This coefficient is modest with respect to magnitude; an increase of 10 past lynchings yields an expected 5 percent decrease in the number of compliant quarters $(1 - e^{-.005*10})$.

Consistent with our interaction hypothesis, the coefficient for the product term (Model 2) suggests that the legacy of lynching is contingent on black population size. In that model, the lynching coefficient is not significantly different from zero when the black population is zero. However, the interaction coefficient shows that the effect of lynching on compliance becomes smaller and ultimately turns negative after the black population surpasses about 13 percent, and it becomes increasingly stronger in the negative direction thereafter. This contingent association aligns with recent arguments that the legacy of lynching is associated with law enforcement outcomes only in the presence of a sizeable black population (Jacobs et al. 2005).

Model 3 in Table 2 provides a test of robustness by incorporating additional race-specific measures. The interaction coefficient in Model 3 remains significant, albeit at a more modest level of statistical significance (p < .10, two-tailed). This is based on a smaller N because the sample is limited to counties and clusters with 500 or more black residents to permit the inclusion of control variables for black officers and measures of black–white economic inequality. The results in this model also indicate that compliance increases with the proportion of black police officers in a county (b = .003). Net of such race-specific measures, a main effects model (without the interaction term) shows that lynching is negatively and significantly correlated with compliance (model not shown).

Table 3 shows the negative binomial regression coefficients for our second outcome variable, the number of anti-black hate crimes reported by police. Looking first at the main effects model (Model 1), a few of the control variables are significantly associated with reported anti-black hate crimes. Police report such crimes with greater frequency in more populous areas (b = 1.003), where the number of police officers per capita is higher (b = .609), and where there is a sizeable young population (b = .06). Net of the control variables, and unlike the previous analyses of compliance, the main effects model (Model 1) in Table 3 indicates a positive association between past lynchings and reported hate crimes that target blacks (b = .028). This result is consistent with Messner and colleagues' (2005) finding that lynching is positively associated with white-on-black argument-related homicides, although at first glance this finding appears to contradict our hypothesis.

Yet, Models 2 and 3 in Table 3 again point to an interaction between past lynchings and current black population size. The models suggest that lynching has a positive effect on police reports of anti-black hate crimes where no blacks reside, but this effect diminishes as the percent black increases, ultimately reversing direction as the black population surpasses about 40 percent. In Models 1, 2, and 3 in Table 3, we find little support for our hypothesis that anti-black hate crimes are inversely related to past lynching of blacks, but the direction and significance of the coefficient for the interaction term is consistent with our hypothesis concerning racial threat and the legacy of lynching.

Table 3 raises a question that warrants additional scrutiny: Why is the main effect for lynching positive for anti-black hate crimes reported by police, in apparent contradiction to one of our predictions? One possibility is that policing agencies in counties with a history of racial antagonism encounter offenses motivated by racial animus with some frequency. While the inclination in such counties may be to refrain from reporting these as hate crimes, some agencies may report them either on their own volition or due to incentives such as state directives or participation in NIBRS. If true, we would expect a legacy of lynching to decrease the likelihood of reporting *any* anti-black hate crimes, but to increase reports among counties reporting one or more anti-black hate crimes.

To shed some light on this possible scenario, we turn to Model 4 in Table 3. This model presents a zero-inflated negative binomial model

Table 3. Negative Binomial Regression Coefficients: Number of Racially-Motivated Hate Crimes that Target Blacks Reported by Police, Regressed on Predictor Variables

	Model 1	Model 2	Model 3	Model 4 (zero-inflated)
Lynching of blacks	.028*	.079*	.070*	.038*
	(.012)	(.025)	(.025)	(.012)
Percent black	−.005	−.0006	−.002	−.002
	(.008)	(.008)	(.010)	(.009)
White unemployment	−.024	−.016		−.046
	(.051)	(.051)		(.053)
White poverty	−.029	−.029		−.021
	(.020)	(.020)		(.021)
Black–white unemployment ratio			.039	
			(.101)	
Black–white poverty ratio			.213*	
			(.098)	
Logged population	1.003*	1.010*	1.006*	.801*
	(.123)	(.123)	(.134)	(.135)
Divorce rate	−.067	−.059	−.050	−.062
	(.055)	(.055)	(.060)	(.057)
Population ages 15 to 29	.060*	.058*	.066*	.052*
	(.020)	(.020)	(.022)	(.020)
Percent urban	.005	.004	.0004	.006
	(.005)	(.005)	(.005)	(.005)
Voting for Bush in 1992	.002	.004	.006	.002
	(.009)	(.009)	(.011)	(.009)
White-on-black homicides	−.063	−.064	−.045	−.046
	(.033)	(.034)	(.036)	(.033)
Police officers per capita	.609*	.642*	.736*	.660*
	(.139)	(.139)	(.159)	(.154)
Percent born in state	−.002	−.0004	.0007	−.004
	(.007)	(.007)	(.008)	(.007)
Percent black officers			−.009	
			(.007)	
Elected black county commissioners	.109	.113	.086	.010
	(.059)	(.059)	(.060)	(.061)
Black county sheriffs	.461	.572	.695*	.553
	(.312)	(.311)	(.315)	(.316)
Lynching of blacks × percent black		−.002*	−.002*	
		(.0008)	(.0008)	
Constant	−11.205*	−11.778*	−13.244*	−8.809*
	(1.496)	(1.513)	(1.755)	(1.656)
Zero-count				
Logged population				−3.354*
				(1.146)
Lynching of blacks				.158*
				(.081)
Percent black				.044
				(.026)
Constant				29.560*
				(10.463)
N	725	725	561	725
χ^2 / df	568.27 / 23	574.59 / 24	487.40 / 25	439.42 / 23

Notes: Dummy variables for states are included in the analysis but not shown in the table. Standard errors are in parentheses.

* $p \leq .05$ (all tests two-tailed).

with lynching and percent black predicting the likelihood of a zero-count for hate crime reporting (net of population size; bottom part of the table), as well as the number of anti-black hate crimes reported (top part of the table). The results are consistent with the previous account. The odds of reporting zero anti-black hate crimes increase with each past lynching (b = .158). At the same time, when looking at policing agencies that report one or more hate crimes, the frequency of reported hate crimes increases with each past lynching (b = .038).

We also acknowledge a plausible alternative interpretation of our findings in Table 3. Research convincingly shows that hate crimes perpetrated against blacks are more frequent where the black population is small, presumably because whites are emboldened and seek to protect traditionally "white turf" (Green, Strolovitch, and Wong 1998; Lyons 2007). It follows that a legacy of lynching could be associated with current hate crime behavior against blacks, and that this tendency is exacerbated where the black population is small. The interaction term in Model 2 is consistent with this explanation if one assumes that official statistics reliably measure the prevalence of hate crime offending, and we suggest that the coefficients may partly reflect such an account. Still, three sets of findings support our interpretation of the coefficients. First, lynching is negatively correlated with compliance (Model 1 of Table 2), a dependent variable that is not contingent on the level of offending. Second, a legacy of lynching increases zero-counts of anti-black hate crimes (Model 4 in Table 3), which is congruent with our assumptions. And third, if the reporting data entirely reflect actual offending, and if blacks are more often hate crime victims where they lack numbers and power, then we would expect significant and negative coefficients for the respective main effects of percent black, black sheriffs, and black commissioners. Yet Model 1 in Table 3 shows that percent black is not statistically significant and the latter two coefficients are actually positive in direction.[22]

Table 4 shows logistic regression coefficients for the last dependent variable, hate crime prosecution. Several variables are associated with prosecution. As expected, a hate crime prosecution is more likely where more hate crimes are reported by police (b = .103). We also find a negative association between political conservatism (voting for Bush) and hate crime prosecution (b = −.036). With respect to the focal independent variables, the evidence is consistent with the racial threat perspective as articulated above; the odds of a hate crime prosecution decrease by about 4 percent for each percentage increase in the black population ($100*[1 - e^{-.039}]$). Models 2 and 3 in Table 4, however, show that the interaction coefficient is in the predicted direction but it is not statistically significant.[23] An important limitation of the hate crime prosecution variable is that it does not distinguish between racially-motivated hate crime cases and cases not entailing racial bias.

The preceding analyses are limited to the South, the region for which the lynching data are of the highest quality. As an additional test to incorporate a wider geographic area, we use data on all counties from the NAACP's catalog of lynching incidents from 1889 to 1918 to test our interaction models for each dependent variable. These results, shown in Table 5, are consistent with the analysis of the 10 southern

exhibit an interactive effect on crimes based on racial bias. Given the logic of this argument, there is no reason to expect an interaction between lynching and black population size when modeling *non–racially-motivated* hate crimes. Indeed, in additional analyses not shown here but available from the authors upon request, neither the main effect of lynching nor its interaction with percent black is statistically significant when counts of non–racially-motivated hate crimes are regressed on the same set of predictor variables.

[23] The interaction coefficient for percent black and lynching is statistically significant and negative in alternative model specifications in which the dummy variables for states are omitted and the standard errors are adjusted for clustering within states. In these models, the interaction effect is very similar to that found in the analysis of hate crime law compliance in Table 2. It is likely that variation in state laws and other state directives covary with both the legacy of lynching and prosecution.

[22] One additional point concerning the analysis of anti-black hate crimes is noteworthy. Our argument emphasizes racial antagonism and racial threat, and thus we predict lynching and racial composition to

Table 4.   Logistic Regression Coefficients: Prosecution of a Hate Crime Case on Predictor Variables

	Model 1	Model 2	Model 3
Lynching of blacks	−.008	.045	.021
	(.029)	(.058)	(.061)
Percent black	−.039*	−.034*	−.029
	(.015)	(.015)	(.018)
White unemployment	.043	.043	
	(.104)	(.104)	
White poverty	−.074	−.073	
	(.045)	(.045)	
Black–white unemployment ratio			−.203
			(.147)
Black–white poverty ratio			.410*
			(.155)
Logged population	.343	.313	.393
	(.231)	(.232)	(.252)
Divorce rate	−.087	−.085	−.126
	(.094)	(.094)	(.105)
Population ages 15 to 29	.037	.037	.035
	(.040)	(.040)	(.040)
Percent urban	−.017*	−.017	−.018
	(.009)	(.009)	(.009)
Voting for Bush in 2000	−.036*	−.035*	−.022
	(.016)	(.016)	(.020)
White-on-black homicides	.011	.029	.043
	(.086)	(.091)	(.094)
Hate crimes reported in 2000	.103*	.105*	.103*
	(.051)	(.051)	(.051)
Percent born in state	−.028*	−.026*	−.029*
	(.012)	(.012)	(.013)
Elected black county commissioners	.065	.064	.066
	(.095)	(.094)	(.097)
Black county sheriffs	.481	.538	.562
	(.545)	(.549)	(.556)
Lynching of blacks × percent black		−.002	−.002
		(.002)	(.002)
Constant	1.613	1.554	−.519
	(2.845)	(2.859)	(3.390)
N	674	674	538
χ^2 / df	133.11 / 23	134.31 / 24	103.55 / 24

Notes: Dummy variables for states are included in the analysis but not shown in the table. Standard errors are in parentheses.
* $p \leq .05$ (all tests two-tailed).

states. In Models 1 through 5, which includes an analysis of hate crime prosecution, the interaction coefficient is negative and statistically significant (p value for the interaction term in Model 6 is .064). Bearing in mind our earlier cautions about the quality of this national data set, these results suggest that the joint impact of lynching and racial composition on law enforcement responses to hate crimes hold when extending our sample beyond the South.

SUMMARY AND DISCUSSION

The results from our analyses of hate crime policing and prosecution are largely in line with our theoretical premise that the legacy of lynching, which we use as an indicator of historical racial antagonism and the state's failure to protect a minority group, is predictive of contemporary law enforcement responses to hate-motivated crimes. Past lynching is negatively correlated with hate crime law compliance

Table 5. Coefficients for Lynching, Percent Black, and Their Product Term for Analysis of all U.S. Counties

	(Model 1) Compliance	(Model 2) Compliance >500 blacks	(Model 3) Anti-black hate crimes	(Model 4) Anti-black hate crimes >500 blacks	(Model 5) Prosecution	(Model 6) Prosecution >500 blacks
Corresponding model from southern sample	*Table 2, Model 2*	*Table 2, Model 3*	*Table 3, Model 2*	*Table 3, Model 3*	*Table 4, Model 2*	*Table 4, Model 3*
Coefficients						
Lynching of blacks	.247	.242	3.499*	2.790*	5.182*	3.928
(annualized)	(.161)	(.189)	(.865)	(.853)	(2.156)	(2.232)
Percent black	.0008	−.0002	−.001	.0002	−.021*	−.015
	(.0008)	(.001)	(.005)	(.006)	(.010)	(.011)
Lynching * percent black	−.013*	−.013*	−.093*	−.072*	−.184*	−.144
	(.005)	(.006)	(.031)	(.029)	(.077)	(.078)
N	2,801	1,312	2,823	1,326	2,561	1,318

Notes: Analysis includes dummy variables for states and controls for all variables in the corresponding models from Tables 2, 3, and 4. Additional data notes are included in the Appendix. Standard errors are in parentheses.

* $p \leq .05$ (all tests two-tailed).

by policing agencies (Table 2, Model 1) and increases the likelihood of reporting no hate crimes (Table 3, Model 4). Moreover, for two of the three dependent variables in analyses restricted to 10 southern states, and for five of six analyses of the national sample of U.S. counties, the interaction coefficient for past lynching and our measure of current racial threat is negative and significant. In other words, a history of lynching in combination with a relatively large racial minority is associated with lesser compliance with, and enforcement of, hate crime legislation.

We readily acknowledge several important limitations in this research. First, as discussed earlier, a question that affects virtually all work on hate crime offending and law enforcement responses is whether the number of reported hate crimes and the legal processing of these crimes actually reflect the prevalence of this type of behavior. Or, do reports and prosecutions of hate crimes reflect a willingness to enforce hate crime laws, consistent with the constructivist perspective? It is difficult to adjudicate between these competing ideas without independent measures of offending and enforcement. Mindful of this difficulty, we examined hate crime law enforcement via multiple outcome variables and multiple institutions (police and prosecutors), and by introducing proxy measures of plausible intervening mechanisms to the extent possible. Still, while our measures of reporting and compliance behave in a manner consistent with a constructivist interpretation of the outcome variables, future work might attempt to replicate our findings with data on related outcomes, such as arrests, judges' sentencing enhancements, and convictions. In addition, qualitative inquiry into policing agencies and prosecutors' offices could shed direct light on the activities and orientations of those who enforce hate crime laws (cf. Bell 2002), thereby enhancing the veracity of our interpretations.

Second, given the aggregate nature of the data, we are unable to identify the specific actors involved in the enforcement of hate crime legislation, the characteristics of these actors, and the nature of the decision-making processes associated with the recording and prosecution of hate crimes. A particularly

important issue for future research to investigate is how the individual attribute of race interacts with the features of the larger social context. For example, what political pressures and organizational constraints impinge upon *black* enforcement agents (police, sheriffs, and prosecutors) who work in settings characterized by deeply ingrained traditions of racial antagonism? How do such actors adapt to these pressures and, in some cases, overcome organizational constraints that might discourage vigorous enforcement of hate crimes?

Finally, our indicator of lynching is ultimately an indirect proxy for an intervening mechanism—cultural traditions. We argue that lynch mobs' actions in the past reflected the racial antagonism of the time and place and the state's failure to protect the predominant racial minority, and this general cultural orientation has dissipated but not completely evaporated over time. Deeply ingrained traditions "die hard," and we propose that traces of the cultural sentiment that permitted lynching linger into the present and are manifest today via lax enforcement of laws that deal with hate crimes. However, direct measures of such latent cultural traditions and contemporary cultural orientations are unavailable. We also cannot tally the number of lynchings *prevented* by whites who viewed the practice as contemptible, which could further elaborate our findings.[24] Nevertheless, our results demonstrate a coherent series of relationships involving lynching, racial composition, and multiple indicators of hate crime law enforcement that align with our theoretically grounded account.

With these caveats in mind, the analyses further underscore other researchers' claims about historical continuity in the exercise of social control (Jacobs et al. 2005; Wacquant 2000; Zimring 2003). They also largely support Bobo and Smith's (1998) suggestion that race relations have morphed from overt discrimination into skepticism about laws that assume a protective role for racial minorities, in this case evidenced by our focus on blacks. Jim Crow racism has largely ceased, but race and the history of racial antagonism remain vital for understanding variation in state responses to crimes of bigotry.

Our finding that the effect of lynching on law enforcement responses to hate crimes is contingent on current racial threat is also consistent with recent research on other facets of social control, such as death sentences (Jacobs et al. 2005). It appears, however, that this interplay increases punitive actions that disproportionately fall on minorities (see, e.g., Jacobs et al. 2005) while decreasing law enforcement protective of minorities. In light of this, our conclusion that the social control of intergroup conflict is in part a function of both current racial threat and a cultural tradition of apathy toward the protection of minorities suggests promising topics for future research on related laws and policies. For example, our model would predict that reports of employment discrimination are less likely to be investigated where past lynching episodes are numerous and the current black population is relatively large. The results are also germane to other institutions that often assume a protective function for lower-class minority groups, such as indigent defense systems. It would be consistent with our account if the same counties that scarcely enforce hate crime laws also provide less access to effective defense counsel. These ideas represent a sampling of research topics that might be informed by our model, and empirical work on such issues could further test the credibility of our interpretations.

We close by emphasizing that this research partly corroborates prior work on state responses to hate crimes by further illustrating the social construction of hate crimes (McVeigh et al. 2003) and explaining the gap between written and practiced law (Jenness and Grattet 2005). We go beyond extant work by drawing theoretical and empirical attention to the cultural underpinnings of state responses to crimes entailing bigotry. Although all states are "at risk" of reporting hate crimes and complying with federal law, substantial intraregional and intrastate variation exists. Laws that remain dormant in some places are enforced, or complied with, in

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[24] While we cannot measure prevented lynchings, we did measure the continuation of lynching after the Great Migration began and elites increasingly challenged the practice (Tolnay and Beck 1995). We reestimated the models using the number of lynchings with black victims from 1916 to 1930 (as opposed to the full 1882 to 1930 period), and the substantive results are consistent with those reported in the text. These results are available from the authors upon request.

others. We investigated such variation to shed light on the social control of intergroup conflict and to illustrate the current consequences of conflict-laden histories. History reveals clues to contemporary criminal justice behavior, but a full understanding of such behavior requires consideration of current social structures in combination with cultural legacies.

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APPENDIX

ADDITIONAL NOTES ON DATA

We made the following changes to the NAACP data, using SAS. The following two lines of code assign county identifiers based on city identifiers:

if state = 'Texas' and city = 'Hedsville' then county = 'ROBERTSON';
if state = 'Texas' and city = 'Lang' then county = 'GILLESPIE';

The following lines of code edit the NAACP county identifier to match that found in the Horan and Hargis (1995) template:

if state = 'Illinois' and county = 'VERMILLION' then county = 'VERMILION';
if state = 'Illinois' and county = 'SAINTCLAIR' then county = 'STCLAIR';
if state = 'Indiana' and county = 'WARWICK' then county = 'WARRICK';
if state = 'Maryland' and county = 'ALLEGHANY' then county = 'ALLEGANY';
if state = 'Maryland' and county = 'ARUNDEL' then county = 'ANNEARUNDEL';
if state = 'Maryland' and county = "PRINCEGEORGE'S" then county = 'PRINCEGEORGES';
if state = 'Missouri' and county = 'GREEN' then county = 'GREENE';
if state = 'Missouri' and county = 'GREENLEE' then county = 'GREENE';
if state = 'Michigan' and county = 'SAINTCLAIR' then county = 'STCLAIR';
if state = 'Missouri' and county = 'ST.CHARLES' then county = 'STCHARLES';
if state = 'Missouri' and county = 'SAINTLOUIS' then county = 'STLOUISCOUNTY';
if state = 'Oklahoma' and county = 'MANNFORDCREEK' then county = 'CREEK';
if state = 'Texas' and county = 'FT.BEND' then county = 'FORTBEND';
if state = 'Texas' and county = 'BEAVER' then county = 'POLK';
if state = 'Texas' and county = 'GARFIELD' then county = 'RUSK';
if state = 'Virginia' and county = 'ACCOMAC' then county = 'ACCOMACK';
if state = 'Virginia' and county = 'ALBEMARIE' then county = 'CHARLOTTESVILLECITY';
if state = 'Virginia' and county = 'ALEXANDRIA' then county = 'ALEXANDRIACITY';
if state = 'Virginia' and county = 'ALEGHANY' then county = 'ALLEGHANY';
if state = 'Virginia' and county = 'CULPEPPER' then county = 'CULPEPER';
if state = 'Virginia' and county = 'FARQUHAR' then county = 'FAQUIER';
if state = 'Virginia' and county = 'HOUSELOUISA' then county = 'LOUISA';
if state = 'Virginia' and county = 'MEEKLENBURG' then county = 'MECKLENBURG';
if state = 'Virginia' and county = 'MERCER' then county = 'TAZEWELL';
if state = 'Virginia' and county = 'NOTTAWAY' then county = 'NOTTOWAY';
if state = 'Virginia' and county = 'PITTSYLVANIA' then county = 'DANVILLECITY';
if state = 'Virginia' and county = 'ROANOKE' then county = 'ROANOKECITY';
if state = 'Virginia' and county = 'WARWICK' then county = 'NEWPORTNEWSCITY';
if state = 'Texas' and county = 'WENO' then county = 'COLORADO';

We dropped NAACP lynchings with the following county identifiers because the identifiers appear to be wrong:

   if state = 'Missouri' and county = 'LIBERTY' then drop = 1;
   if state = 'Texas' and county = 'AVALON' then drop = 1;
   if state = 'Texas' and county = 'PASO' then drop = 1;
   if state = 'Texas' and county = 'SANPETE' then drop = 1; *6 lynchings dropped;
   if drop ne 1;

We did not form the following counties with lynching activity into clusters because the activity occurred entirely after modern county boundaries were in place. In Texas: Reeves, Valverde, Crockett, Kinney, Pecos, Schleicher, and Sutton. In Virginia: Charlottesville City, Albemarle, Pittsylvania, Danville City, Roanoke, Roanoke City, Salem City, Buchanan, Dickenson, Russell, Wise, Norton, Elizabeth City, Newport News City, Warwick, Warwick City, and Hampton City. In Wyoming: Albany, Converse, Goshen, Laramie, Niobrara, and Platte. In Arkansas: Cleburne, Independence, Van Buren, and White. In Florida: Monroe. In Kentucky: Ballard and Carlisle. In North Carolina: Cumberland, Durham, Hoke, Orange, Robeson, and Wake. In South Carolina: Charleston. In Tennessee: Fentress, Overton, and Pickett.

We clustered the following counties to link various data from 1990 and 2000. In Alaska, we formed the Yakutat borough, the Skagway-Hoonah-Angoon census area, and the Skagway-Yakutat-Angoon census area into one cluster. Also in Alaska, we clustered Denali, Yukon-Koyukuk, and Southeast Fairbanks. In Virginia, we clustered South Boston and Halifax, as well as Alleghany and Clifton Forge City. In Montana, we clustered Yellowstone National Park (part), Gallatin, and Park. In Hawaii, we clustered Kalawao and Maui. Finally, in New York, we clustered Bronx, Kings, New York, Queens, and Richmond counties.

DATA CODING NOTES ON POLICING AND PROSECUTION

We used sheriffs' departments for the compliance outcome variable (Table 2). If there was no sheriff's department, but a county police department was available, we used the latter. If no county or reasonable substitute (with county-level jurisdiction) was available, we coded the case as missing. Three states in our supplementary analysis of the national sample were particularly problematic: Connecticut, Alaska, and Virginia. In the former two states, sheriffs' offices were systematically missing because of law enforcement organization in those states, and we could identify no county policing agency or a reasonable proxy. (None of these states are in our focal sample of 10 southern states.) For Virginia, we used the municipal policing agency's degree of compliance for county FIPS codes greater than 500. The issues described above are relevant only for Models 1 and 2 of Table 5.

Table A1 lists additional special cases where, for reasons detailed in the table, we used a non–sheriff's department policing agency. With three exceptions, these apply only to the supplementary analysis of compliance in the national sample (Table 5, Models 1 and 2).

We aggregated counties coterminous with New York City boroughs into one "New York" cluster.

Given the unique organization of district attorneys' offices in Connecticut (corresponding to county subunits) and no county-level data in the National Survey of Prosecutors for Alaska, we dropped these states from the analysis of prosecution in Models 5 and 6 in Table 5.

We had to drop several county clusters for Model 6 in Table 5 because the model is restricted to counties with more than 500 blacks; hence the much smaller N for that analysis.

Table A1.   Non-Sheriff's Department Policing Agencies

FIPS	Location	Description
12025	Miami-Dade, FL	Miami-Dade County Police Department is used.
13189	Mcduffie, GA	County Policing Agency is used (ORI #GA09400).
47037	Davidson, TN	Nashville-Davidson Metro Police Department is used.
8031	Denver, CO	Denver Municipal Police is used because county and city borders are essentially the same.
1101	Washington, DC	Omitted because state dummy variables are included, and thus more than one agency within a unit (e.g., state) is required.
15003	Honolulu, HI	Honolulu Police Department is used because no county-level agency is available.
29189	St. Louis Co., MO	St. Louis County Police Department is used.
29510	St. Louis City, MO	St. Louis City Police Department is used.
30023	Deer Lodge, MT	Deer Lodge County Law Enforcement is used.

Note: FIPS refers to the Federal Information Processing Standards Code for the county.

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