From Lynching to Gay Bashing: The Elusive Connection Between Economic Conditions and Hate Crime

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Trends in bigoted violence are often explained by reference to frustrations arising from macroeconomic downturns. Historical and recent time-series studies have turned up significant links between economic conditions and lynchings of Blacks in the pre-Depression South (e.g., Hepworth & West, 1988; Hovland & Sears, 1940). However, replicating the time-series analyses of lynching, extending them through the Great Depression, and applying similar techniques to contemporary data fail to provide robust evidence of a link between economic performance and intolerant behavior directed against minorities. The authors speculate that the predictive force of macroeconomic fluctuation is undermined by the rapid rate of decay in the frustration-bred aggressive impulse and the absence of prominent political actors affixing economic blame on target groups.

Many social science theories trace intergroup antagonism and violence to adverse economic conditions. In sociology, intergroup hostility is frequently attributed to competition for scarce material resources, the effects of which are exacerbated during periods of economic retrenchment (Bobo, 1988; Olzak, 1990; Olzak & Shanahan, 1996). A variant of this hypothesis common in studies of comparative politics focuses on the manipulative role played by political leaders, who encourage out-group hostilities by playing upon economic resentments (Horowitz, 1985). The mediating role of elites also figures prominently in the Marxist thesis that racial antagonism between Black and White workers in the South was fomented by capitalists eager to deflect attention away from class politics, particularly during periods of economic strain (Cox, 1948).

In the field of psychology, one of the most commonly articulated hypotheses linking intergroup antagonism to economic contraction is the frustration–aggression thesis formulated by Dollard, Doob, Miller, Mowrer, and Sears (1939) and elaborated by Miller (1941). This thesis was given its most memorable empirical grounding in a classic paper by Hovland and Sears (1940), in which the authors argued that the frustrations attendant to economic downturns produce aggressive impulses that are directed at vulnerable targets, such as minority groups, even when these groups bear no actual or perceived responsibility for economic decline. Reasoning that "aggressive acts should be more numerous during years of depression than during years of prosperity, since economic conditions, in general, reflect the ease or difficulty with which the customary economic activities of the members of a group can be carried out," Hovland and Sears (1940, p. 301) expected a statistical association between economic conditions and the frequency of anti-Black lynchings in the South.

The nature and strength of this relationship has been the subject of continual revision. Building on its discovery by Raper (1933), Hovland and Sears (1940) reported a strong statistical relationship between lynchings of Blacks in the American South and economic downturns, as gauged by cotton prices and economic growth from 1882 to 1930. A few years later, Mintz (1946) questioned the validity of Hovland and Sears's statistical analysis, arguing that the putative relationship between economic conditions and lynching declines markedly when one removes the nonlinear deterministic trend in the lynching series.

After this exchange, studies of lynching moved away from time-series analysis, focusing instead on interregional variability in the incidence of lynching (cf. Tolnay, Beck, & Massey, 1989).

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Replication data sets and programs may be found on the World Wide Web at pantheon.yale.edu/~gogreen.

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Notwithstanding Mintz's (1946) critique, Hovland and Sears's (1940) findings continued to be widely cited and accepted among social scientists, prompting Reed, Doss, and Hulbert (1987) to dub their argument "a thesis too good to be false." But no sooner had Reed et al. taken social scientists to task for placing stock in a dubious empirical claim than new research appeared. Hepworth and West (1988) presented a detailed replication of the Hovland and Sears analysis using modern time-series techniques.[1] Although there is much to commend in Hepworth and West's analysis, we regard the link between economic performance and lynching as much more tenuous than indicated in this and other similar studies (E. M. Beck & Tolnay, 1990; Olzak, 1990; Tolnay & Beck, 1995).

Replicating these time-series analyses, we focused in particular on sampling and measurement issues. For example, the time-series data to which successive generations of scholars have turned halt abruptly at the cusp of the Great Depression. What happens to the relationship between lynching and economic conditions when the series is extended a few years beyond 1930? For that matter, how do the results change when one uses economic indexes or lynching data other than those available to Hovland and Sears in 1940?

Recognizing the limitations of continual reanalysis of the lynching time series, we also conducted a new test of the macro-economic-strain hypothesis. Unlike other ecological studies that have looked at the links between economic fluctuations and acts of aggression (e.g., Catalano, Novaco, & McConnell, 1997), the present study takes as its dependent variable "hate crime," here defined as acts of violence, vandalism, harassment, and intimidation directed against victims on account of their putative race, religion, ethnicity, or sexual orientation (Wang, 1994). Using monthly data for New York City over a span of 9 years, we tested the relationship between unemployment rates and the frequency of hate crimes directed against gay men, lesbians, Blacks, Whites, Jews, or Asians. Our purpose was to reassess the long purported relation between economic variables and intergroup violence and suggest more refined hypotheses about the conditions under which economic frustration and competition lead to hate crime.

Reanalyzing Hovland and Sears (1940)

In the first effort to replicate the Raper (1933) and Hovland and Sears (1940) analyses using modern time-series techniques, Hepworth and West (1988) rightly pointed out that the statistical techniques used both by Hovland and Sears and their critic Mintz (1946) are crude by contemporary standards. In particular, these early analyses failed to take into account the possibility that both the independent and dependent variables might be serially correlated in ways that could have led to spurious correlations (McLeary & Hay, 1980).

Hepworth and West's (1988) time-series analysis of the lynching series proceeded in two stages. First, each of the independent and dependent variables were "prewhitened" using autoregressive moving average (ARMA) models and polynomials in time so as to remove serial dependencies and trends (for an explanation of this approach, see Catalano et al., 1997; McCleary & Hay, 1980). This prewhitening step also has the theoretical advantage of creating a series of unanticipated changes in the economy. Dollard et al. (1939) argued that frustration results when expected outcomes are blocked, so that, as Berkowitz (1989) pointed out, "deprivation is not necessarily the same as frustration" (p. 61).

Hepworth and West (1988) evaluated the causal link between lynchings and economic performance by examining the contemporaneous correlations among the whitened series. They found no apparent relationship between lynching and cotton prices but did find a sizable and statistically significant correlation between lynchings of Blacks and the "Ayres index" (Ayres, 1939, p. 204), an early effort to chart national economic performance used by Hovland and Sears (1940).

One important feature of Hepworth and West's (1988) analysis is that they took base-10 logarithms of each series in order to "stabilize the variance" of the whitened residuals (p. 243).[2] This decision has important consequences. Logarithms have the effect of transforming levels into ratios, such that the change from 50 to 100 lynchings is the same as from 5 to 10. Logarithms thus greatly amplify the relative magnitude of lynchings after 1920. For example, Hovland and Sears (1940) reported 102 anti-Black lynchings during 1898, 51 during 1922, and 17 during 1925. The corresponding $\log_{10}$ lynchings are 2.01, 1.71, and 1.23. Although the drop in absolute numbers is greater between 1898 and 1922, logarithms record a smaller proportional decrease. One empirical question, then, regards the extent to which the results hinge on the log transformation of the lynching series or the inclusion of data from 1920 to 1930.

In Table 1, we present our replication of the Hepworth and West (1988) analysis. Consistent with their findings, we obtained a modest relationship ($-.20$) between the whitened (per acre) value of cotton and the whitened log of Black lynchings. We also found a strong contemporaneous correlation ($-.42$) between the whitened Ayres index and the whitened log of Black lynchings.[3] But as Table 1 makes apparent, the Ayres index correlated appreciably with only the whitened log of Black lynchings for the period 1882–1930. Discard the last 10 observations or use the untransformed number of lynchings as the

[1] E. M. Beck and Tolnay (1990), working without reference to Hepworth and West (1988), advanced a more comprehensive model of lynchings, in which the economic circumstances of Southern Whites played a central role. This essay later appeared in somewhat modified form in their award-winning book *A Festival of Violence* (Tolnay & Beck, 1995). Another analysis of the link between economic factors and lynching appears in Olzak (1990). Although each of these time-series investigations seems to lend strong support to the long-standing claim that lynchings of Southern Blacks fluctuated in response to changing economic conditions, none is robust against slight alterations in the specification of their models.

[2] The log transformation does not in fact eliminate the problem of heteroskedasticity. Examining the residuals from Hepworth and West's (1988) model of log lynchings, we found the White F statistic for heteroskedasticity to be 6.02, $p = .002$.

[3] We were surprised to discover that the cross-correlations between the whitened Ayres index and the whitened lynching series are significantly positive at Lags 2 ($r = .30$) and 3 ($r = .30$). The unexpected pattern holds for the cross-correlation between whitened cotton, lagged three periods, and whitened lynching ($r = .37$). Aside from the contemporaneous cross-correlation, no other cross-correlations are significantly negative.

Table 1
Economic Correlates of Lynching: Replication of Hepworth and West (1988)

Dependent variable	Ayres index	Whitened Ayres index	Whitened per acre value of cotton	Percent change in real per capita GNP
		1882–1930		
Number of anti-Black lynchings	−.17	−.17	−.02	.03
Whitened log of anti-Black lynchings	−.45	−.42	−.20	−.17
Whitened number of anti-Black lynchings	−.22	−.20	.03	−.06
		1882–1920		
Number of anti-Black lynchings	−.30	−.28	.01	.04
Whitened log of anti-Black lynchings	−.26	−.22	.09	−.04
Whitened number of anti-Black lynchings	−.15	−.10	.15	.02

Note. Entries are contemporaneous Pearson correlations. Ayres index and number of anti-Black lynchings are drawn from Hovland and Sears (1940, p. 304). Whitened log of anti-Black lynchings, whitened Ayres index, and whitened per acre value of cotton are derived from the procedures described in Hepworth and West (1988, pp. 243–244). Annual change in real per capita gross national product (GNP) is drawn from Series F 1–5 of *Historical Statistics of the United States: Colonial Times to 1970* and Table R-40 in Kuznets (1961). For creation of the whitened number of lynchings, see Appendix A.

dependent variable, and the correlation drops markedly. For example, the correlation between the whitened Ayres index and the whitened number of lynchings is −.20 for the period 1882–1930, and the correlation between the whitened Ayres index and the whitened log of lynchings is −.22 for the period 1882–1920.[4] Cotton prices bear a slightly *positive* relationship to every variant of the lynching series through 1920.

Moreover, the results obtained using the Ayres index evaporate altogether when one substitutes an alternative measure of per capita national economic performance.[5] A commonly used measure for purposes of charting fluctuations in national prosperity (cf. Kuznets, 1961; U.S. Bureau of the Census, 1975) dating back to the 19th century is the percentage change in real disposable gross national product (GNP) per capita. Among its virtues is the fact that change in GNP requires no prewhitening. This series bears only a weak correlation (−.17) with the whitened log-lynching series for the period 1882–1930 and none at all for any other time period or variant of the dependent variable.

Nor do alternative estimation approaches yield support for the hypothesis that lynchings change in response to economic fluctuations. Because the annual number of Black lynching victims is an event count, linear regression models of the sort used by Hepworth and West (1988) may produce misleading results, as they make no allowance for the fact that the dependent variable is limited to the range of nonnegative integers (King, 1989). Taking logs of the dependent variable has been shown to be an inadequate, and possibly counterproductive, remedy (King, 1988). Event count models, however, offer no support for the macroeconomic hypothesis.[6]

In summary, one sees a connection between lynching and economic conditions only if one uses one particular measure of economic conditions for one particular time period for one particular transformation of the dependent variable. Indeed, if we extend the Hovland and Sears (1940) data beyond 1930 (cf. Guzman, 1952, p. 278), we do not find lynchings on the rise as the country spiraled into a massive economic recession (Table 2). Between 1930 and 1931, real per capita GNP declined by 8.5%, and yet lynchings dropped from 20 to 12. The following year, real per capita GNP dropped again, this time by an astonishing 15.4%, and lynchings fell to 6. During the remainder of the decade, lynchings jumped up and down in a manner that bears no consistent relationship to absolute levels of prosperity or year-to-year changes.

The Ayres index is available through 1938, so using it and the updated lynching series, we approximated the Hepworth and West (1988) analysis for a longer time span. Again, no relationship whatsoever turned up between changes in GNP and

[4] Resampling experiments ($n = 500$), in which correlations are calculated on the basis of 39 observations, are drawn at random (with replacement) from the whitened Ayres and whitened log-lynching series, and produced an average correlation of −.41.

[5] Although immortalized in the work of Hovland and Sears (1940), Ayres's (1939) measure of economic performance, which encompasses a wide array of different indicators and standardizes the variance of economic swings across business cycles, has left no trace in the annals of economic history. Even encyclopedic works such as Kuznets's (1961) *Capital in the American Economy* make no mention of it. For the period 1882–1930, the Ayres index and percentage change in real disposable gross national product correlated at .58.

[6] We treated lynchings as a negative binomial process in which the underlying lynching rate in a given year (θ_t) is a function of an economic predictor and lynching in the previous year: $\theta_t = \exp(\beta_0 + \beta_1 X_t + \beta_2 L_{t-1})$. The negative binomial was selected because it allows for overdispersion, or more variance than would be anticipated by a Poisson model (King, 1989, p. 52). This analysis, however, again attests to the weak relationship between cotton prices and lynchings. Regardless of which economic condition we used, we found little evidence of a negative relationship between the economy and lynching (replication data sets, programs, and output may be found on the World Wide Web at pantheon.yale.edu/~gogreen).

Table 2
Economic Correlates of Lynching: Replication of Hepworth and West (1988) Through the Great Depression (1882–1938)

Dependent variable	Ayres index	Whitened Ayres index	Whitened per acre value of cotton	Percent change in real per capita GNP
Number of anti-Black lynchings	.36	−.08	−.06	.09
Whitened log of anti-Black lynchings	−.21	−.13	.04	.06
Whitened number of anti-Black lynchings	−.05	−.17	−.13	−.03

Note. Entries are contemporaneous Pearson correlations. The Ayres index is derived from Ayres (1939), and the number of anti-Black lynchings is drawn from Guzman (1952). Whitened log of anti-Black lynchings, whitened Ayres index, and whitened per acre value of cotton are derived from the procedures described in Appendix A. Annual change in real per capita gross national product (GNP) is drawn from Series F 1–5 of *Historical Statistics of the United States: Colonial Times to 1970* and Table R-40 in Kuznets (1961).

either lynchings or log-lynchings (Table 2). The correlation between the whitened Ayres index and the log of lynchings falls from −.42 to −.13, with all of the dropoff occurring by the time the series is extended to 1933. As before, we found trivial correlations between the log of cotton value and lynchings, in contrast to recent work, which purported to show that cotton prices—but not national economic conditions—affected the frequency of lynchings (E. M. Beck & Tolnay, 1990; Tolnay & Beck, 1995). Contrary to the predictions of the economic model, the economic catastrophe of the Great Depression, which set back real per capita GNP by decades, did not return the South to pre-World War I rates of racial violence. Given the post hoc fashion in which the frustration–aggression interpretation was superimposed by Hovland and Sears (1940) onto the empirical regularity previously discovered by Raper (1933), faulty out-of-sample prediction of this magnitude is quite telling.

At the risk of belaboring the point, we would add that the same results obtained when we analyzed the number of lynching incidents rather than the number of victims. Bowling (1993) has argued forcefully that the former is the appropriate dependent variable in analyses of hate crime, and the two lynching series differ somewhat. Drawing upon a thorough inventory of lynching incidents created by Tolnay and Beck (1995), we constructed an alternative series based on the number of lynching episodes in the Deep South. Thus, if on a given day a lynch mob kills *n* victims in a given county, we treat that as one lynching event, rather than *n* events. White lynching parties in Alabama, Arkansas, Georgia, Mississippi, and South Carolina claimed 1,452 victims between 1882 and 1930; the number of lynching events was 1,193. Replicating the Hepworth and West (1988) analysis with the alternative lynching series (Figure 1), however, has little effect on the results. For the period 1882–1930, the correlation between this whitened series and the GNP series is −.17; with whitened cotton prices, the correlation is −.06.

It seems clear that the bivariate relationship between lynchings and economic conditions reported by Hepworth and West (1988) is quite fragile. Slight alterations in the measures used or the time period studied produced substantially weaker correlations. Moreover, the small number of implicit degrees of freedom that remain after Hepworth and West prewhitened a variety of different variables undermines further the significance of the few large correlations that they report. Because multivariate analyses of Southern lynchings (Beck & Tolnay, 1990; Olzak, 1990; Tolnay & Beck, 1995) are, if anything, even more vulnerable to this and other methodological critiques (see Green, Glaser, & Rich, 1996), we find the case for macroeconomic explanations of this form of racial violence unpersuasive.

Contemporary Hate Crime

The logic of frustration–aggression, as articulated by Hovland and Sears (1940), implies that hate crimes directed against target groups such as gay men and lesbians will tend to become more numerous in periods of recession, as the frustrations engendered by economic contraction find expression in attacks against a vulnerable scapegoat. The thesis that deteriorating economic conditions increase rates of aggression against vulnerable out-groups is often invoked by public interest groups and scholars to explain hate crime. For example, in its report on hate crime in 1993, the Los Angeles County Commission on Human Relations cited recession and economic displacement

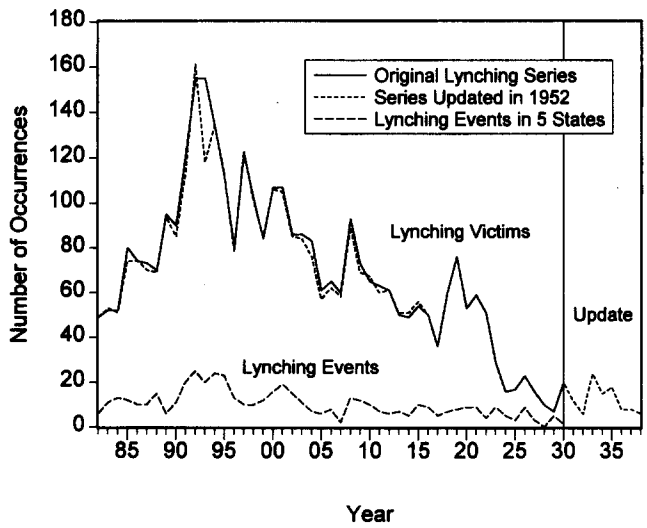

Figure 1. Anti-Black lynching: Victims and events.

among the causes of hate crime (1994, p. 30). In its 1991 Audit of Anti-Semitic Incidents, the Anti-Defamation League of B'nai B'rith linked the "deteriorating American economy" to "record-setting levels of anti-Jewish acts" (1992, p. 19), a sentiment echoed in a report issued by North Carolinians Against Racial and Religious Violence (1992, p. 20). In their widely cited monograph, Levin and McDevitt (1993, pp. 45–54) highlighted "zero-sum economic thinking" in their analysis of the factors that motivate hate crime, and Pinderhughes (1993) underscored economic resentments and frustration in his ethnographic account of White youth and hate crime in New York City.

Despite the frequency with which hate crime is attributed to poor economic performance, evidence linking the two phenomena is rather sparse. Analyses of contemporary hate-crime patterns have appeared only recently. A panel analysis of 100 North Carolina counties traced over the period 1987–1993 undertaken by Green and Rich (in press) revealed weak links between county unemployment rates and anti-gay and -lesbian or anti-Black hate crime. Similarly, Krueger and Pischke (1997) and Green, Strolovitch, and Wong (in press) found no link between economic conditions and racially motivated hate crime in their cross-sectional studies of German localities and New York City communities, respectively. However, Stenner (1995) traced White-on-Black racial violence in the United States between 1960 and 1989 and found unemployment and inflation to be significant predictors, though the statistical analysis is crude by the standards of the Hepworth and West (1988) study.

The question, then, is whether macroeconomic downturns in fact precipitate greater incidence of hate crime. In an effort to assess the temporal link between economic conditions and hate crime, we assembled monthly hate-crime statistics during the period 1987–1995 for the New York City boroughs of Brooklyn, Queens, Manhattan, and the Bronx. Our bias crime data came from records of the New York City Police Department's Bias Incident Investigative Unit (BIIU). The BIIU is a detective unit responsible for documenting, investigating, and analyzing unlawful acts committed against a person, group, or place because of the race, religion, ethnicity, or sexual orientation of the victim. Our data included all incidents that were reported to the BIIU between January 1987 and December 1995. Data from years prior to 1987 are not available to the public. Incidents are reported to the BIIU either by patrol officers who respond to police calls or by precinct commanding officers. Although New York State's bias-crime statute covers only "aggravated harassment," the BIIU documents and investigates all types of crimes that are suspected to have occurred because of bias. The fact that these data are gathered by a single source using consistent reporting practices over time makes it more reliable than the less systematic national hate-crime data used by Stenner (1995).

The monthly pattern of hate crimes directed against gay men and lesbians, Jews, Blacks, Whites, and Asians is presented in Figure 2. Although it is often supposed that hate crimes are on the rise, these data suggest a stable, or perhaps slightly decreasing, rate of hate crime since the late 1980s. Another noteworthy feature of these data is the widely varying rate of hate-crime reports across victim groups (note the disparate ranges on the y axes of Figure 2). One cannot tell from these data whether the cross-group differences reflect actual rates of victimization or willingness to report incidents to the police, but our doubts about the cross-group comparability of hate-crime rates led us to analyze each victimization series separately.

The independent variable was the unemployment rate, the only economic indicator available on a monthly basis. Because we wished to gauge the level of economic discomfort, we used actual, rather than seasonally adjusted, unemployment rates supplied by the U.S. Bureau of Labor Statistics. The fact that the unemployment rate for the four large boroughs move in lockstep (with contemporaneous correlations above .98) encouraged us to model the time series of hate crime occurring for this region as a whole, rather than constructing time series borough by borough. As noted in Appendix B, the unemployment rate for New York City seems to follow an ARMA(1,1) pattern, which is what we would expect if the citywide unemployment rate were a first-order autoregression process but were measured with random sampling error (Beck, 1985). On the other hand, the results would be identical if one were to model unemployment as an integrated moving average process (ARIMA[0,1,1]) or to allow for nonlinear deterministic trends.

The statistical analysis used here was modeled after Hepworth and West's (1988) assessment of the link between lynching and economic conditions. The period under study covers a complete economic cycle: Unemployment starts at 7.5% in 1987, drops to 4.3% the following year, gradually climbs to 13.0% in 1992, and thereafter falls to 7.0%. We prewhitened the unemployment series and each monthly hate-crime series. The specific models used to prewhiten each series are reported in Appendix B. Next, we examined the cross-correlations between whitened unemployment and each whitened hate-crime series. Support for the notion that hate crime follows macroeconomic downturns would be suggested by statistically significant cross-correlations at low lags or by smoothly declining cross-correlations after lags of a certain order. Either pattern would warrant a more elaborate transfer function model in which unemployment is assumed to influence hate crime.

Table 3 reports cross-correlations to Lag 12 between whitened unemployment and hate-crime series. If there were an immediate causal relationship between economic conditions and hate crime, we would expect to see significant positive correlations between hate crimes and Lag 0 or Lag 1 unemployment rates. No such cross-correlations turned up. Instead, we found long strings of weak cross-correlations at lower lags. Of the four significant correlations, two were in the appropriate direction, and these turned up as theoretically unintelligible spikes at Lags 5 and 7. Evidently, the innovations (i.e., the components not anticipated by past unemployment levels) in the unemployment series do not anticipate shifts in the various hate-crime series.

As noted in Appendix B, we repeated this exercise using a wide array of different specifications. Linear and nonlinear trends were introduced for both hate crime and unemployment series, with similar results. We also experimented with scales constructed from two or more hate-crime series, but again found no intelligible patterns suggesting a link between unemployment rates and acts of bigotry.

The sole way to build a case for the macroeconomic-conditions hypothesis using these data was to perform a naive regression (in levels) of hate crime on unemployment, ignoring shared (and potentially incidental) trends in these variables. This

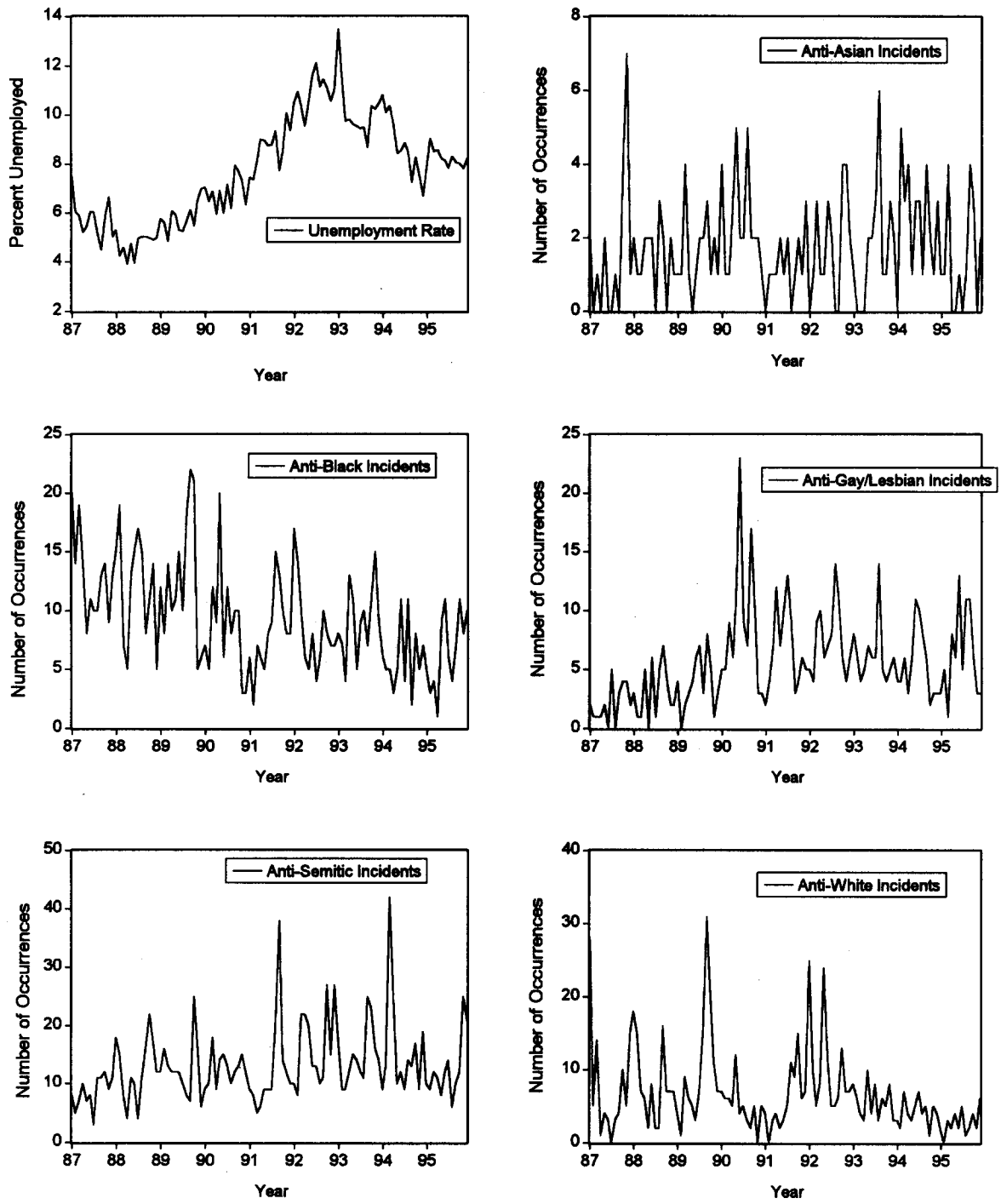

Figure 2. Unemployment rates and monthly incidence of hate crime in New York City, by victim group.

method yielded significant "effects" for two hate-crime series, anti-gay and -lesbian and anti-Semitic incidents, but these two coefficients turned out to be fragile. If one divides the 108-month time series into two 54-month series, the coefficients associated with unemployment drop sharply, the implication being that unemployment rates over the 1987–1995 period are correlated fortuitously with omitted variables that have led to a gradual rise in the reported incidence of anti-Semitic and anti-gay and -lesbian hate crime. In summary, our analysis of hate crime in New York City turned up little evidence linking racial, religious, ethnic, or homophobic incidents to fluctuating economic conditions.

Is the failure to obtain significant cross-correlations the result of faulty data? Given the uncertainties that surround any attempt

Table 3
Cross-Correlations Between Whitened Monthly Unemployment and Whitened Hate-Crime Rates, by Type of Victim, February 1987 to December 1995

Lag of unemployment rate	Anti-Black	Anti-gay and -lesbian	Anti-Semitic	Anti-White	Anti-Asian
0	.06	.12	−.10	.04	.04
1	−.18	.07	−.03	−.11	.10
2	−.03	−.04	.12	.08	−.14
3	.05	.06	.11	.08	−.24#
4	.03	−.02	.14	.05	.08
5	−.18#	−.00	.20*	.12	.07
6	−.06	.12	−.09	.05	−.05
7	.03	.22*	−.10	−.02	.06
8	.05	−.01	.00	.01	.06
9	−.08	.02	.04	.01	−.15
10	.14	.12	.04	.03	−.01
11	−.04	−.05	.17	.06	.08
12	−.02	−.17	−.06	.04	−.03

Note. Unemployment rate for New York City (excluding Staten Island) is prewhitened using a first-order autoregressive moving average model. The anti-Asian hate-crime series required no prewhitening. Other series were prewhitened using a Lag 1 autoregressive specification, with the exception of the anti-gay/lesbian series, which was prewhitened assuming autoregressive errors at Lags 1 and 12. See Appendix B for details on prewhitening models and alternative specifications.
* Indicates correlations significant at $p < .05$ with proper sign. # Indicates correlations significant at $p < .05$ with improper sign.

to measure the frequency of hate crime, no statistical analysis of this kind is above suspicion. Nonetheless, certain properties of our hate-crime data inspire confidence. First, when broken down by precincts, the data revealed quite robust relationships (corroborated by survey data) between rates of minority in-migration and rates of hate crime in predominantly White areas (Green et al., in press). Second, consistent with the thesis that integration of predominantly White neighborhoods precipitates hate crime, anti-Black hate crime has gradually diminished over time, as both the number of New York's White neighborhoods and the rate of Black in-migration into such areas have diminished. Third, the time-series dynamics of hate crime can in no way be construed as unsystematic. During an era that will be remembered for its Black–White tensions, the correlation between anti-Black and anti-White hate crime was .53 (.40 between the two whitened series), suggesting cycles of attack and reprisal. However, as one would expect, no correlation exists, for example, between anti-gay and -lesbian and anti-Black hate crime. Although one might cast doubt on the reporting of anti-gay and -lesbian hate crime, the fact is that anti-gay and -lesbian incidents closely track monthly rates of aggravated assault in Manhattan (contemporaneous $r = .53$). The New York City hate crime data are not formless; they are simply not structured by macroeconomic indicators.

Conclusion

Given the practical limitations that confront any study of bigoted violence, no single empirical finding can be regarded as decisive. But as these results accumulate, defenders of the notion that hate crime rises with a declining economy are hemmed in by untoward findings. We have seen that the statistical link between hate crime and economic circumstances turned up in the case of lynching for just one time span (1882–1930), one measure of economic conditions (Ayres's index of national economic conditions), one measure of lynching (the log of lynching victims), and one test statistic (contemporaneous, as opposed to lagged, correlations). Alter, extend, or replicate this analysis, and the relationship evaporates.

The absence of a robust relationship between changing economic conditions and hate crime warrants a reexamination of the theoretical and experimental premises on which frustration-based models of hate crime are constructed. For decades, scholars have alluded to the putative connection between economic downturns and attacks against vulnerable minorities when illustrating the practical applicability of the frustration–aggression displacement hypothesis (cf. Reed et al., 1987). The widespread belief that Whites are especially prone to lash out against minorities during times of economic recession is bolstered by reference to a large stock of laboratory evidence showing that humans or rodents tend to aggress against available targets after being shocked, denied expected rewards, insulted, and the like. However, a closer look at the theoretical postulates and lab evidence suggests some important limiting conditions to the frustration–aggression hypothesis.

One frequently neglected property of the frustration–aggression relationship is the rate at which the effects of frustration dissipate over time. Consider first experiments using animal subjects. Miller (1948), in one of the first laboratory demonstrations of displaced aggression, observed heightened aggression in rats subjected to electric shock when targets were presented 2 s afterward, but this effect was nonexistent after just 6 s. Azrin, Hutchinson, and Sallery (1964) as well as Roediger and Stevens (1970) obtained similarly rapid rates of decay in aggressive impulses with monkeys and rats, respectively.

Among the scores of frustration–aggression displacement studies involving human participants (e.g., Fitz, 1976; Holmes, 1972; Konecni & Doob, 1972; Miller & Bugelski, 1948), none directly examined decay over time. Konecni (1975), studying aggression that was not induced by frustration, found that aggressive impulses attenuated over time, even without opportunity to aggress. Similarly, Doob and Climie (1972) found that an initial difference in aggressiveness between those exposed to aggressive versus neutral materials disappeared 15 min later. Buvinic and Berkowitz (1976) also found that aggressive responses (i.e., retaliation for an insult) decayed dramatically after a 1-hr delay. Accordingly, Zillmann, Hoyt, and Day (1974) lamented the prevailing assumption that aggressive impulses last long, or at least until they are quenched, stating

> in recognition of the characteristically quite moderate excitatory elevation and typical decay gradients associated with exposure to aggressive materials, and under the assumption that the effect on post-exposure behavior is mainly modified by activated arousal, it may be expected that the duration of effects is a fraction of what has been generally anticipated and extremely short-lived indeed. (pp. 291–292)

One countervailing reason to expect aggressive impulses to persist over time is that cognitive mediation (i.e., interpretation, rehearsal, and rumination of the instigation) justifies and maintains the impulse (Berkowitz, 1984). Individual differences in tendency to ruminate have been shown to moderate aggressiveness (Bushman & Geen, 1990; Caprara, 1986; Collins & Bell, 1997). On the other hand, Zillmann (1988) provided evidence that cognition can diminish aggression through reassessment of the situation. Nevertheless, even if researchers assume that rumination can extend the life span of aggressive impulses, it is unclear whether this process applies to displaced aggression, wherein the target of aggression may not be the subject of rumination. It is telling, also, that none of our aggregate data showed a lagged temporal relationship between economic downturn and hate crime.

Absent a proximal target, the aggressive impulses bred by frustration may dissipate before an attack occurs. Lynching and contemporary hate crime tend to be group activities (Brundage, 1993; Garofalo, 1991) that require more coordination and persistence than more simple forms of aggression, such as domestic violence. The decay of aggressive impulses may account for the fact that macroeconomic downturns coincide with surges in child abuse (Steinberg, Catalano, & Dooley, 1981) and civil commitments for danger to others (Catalano et al., 1997) but not with hate crime.

What, then, are we to make of those instances in which economic downturns do translate into racial strife? History suggests that political elites and organizations play a mediating role by attributing blame and fomenting public resentment toward minority groups in times of economic contraction. For example, Wade (1987) documents the Ku Klux Klan's strategy of recruiting and demonstrating in economically depressed regions during the 1970s. Olzak (1990) establishes a link between turn-of-the-century wage competition and anti-Black urban violence orchestrated in part by labor unions. Similarly, Foner (1974) reported that some antebellum politicians and labor and industry leaders argued in speeches and editorials that emancipated Blacks would pose overwhelming labor competition to White workers (see also Calavita, 1996; Saxton, 1971, for a description of economic scapegoating and dehumanization of immigrants). Similar episodes of stigmatization of Blacks as "strike breakers" and "scabs" by labor leaders were documented in the early 1900s (Kennedy, 1969) and 1960s (Marable, 1983). Propaganda about Black laborers is credited with fostering the racial violence of the 1917 St. Louis riots (Foner, 1974). These historical examples support the hypothesis that political actors can transform economic strife into intergroup conflict and violence.

It is important to distinguish uncoordinated activities carried out sporadically by small groups (a description that encompasses most lynchings and contemporary hate crimes) from systematic campaigns set in motion by political organizations. The latter are bolstered by propaganda efforts that call attention to economic frustrations while at the same time attributing blame to particular target groups. This mobilization strategy may, in effect, revitalize flagging aggressive impulses and focus otherwise diffuse attributions.

The mediating role of political organizations may help explain the divergent paths of two central propositions offered by Hovland and Sears (1940). Although that essay is best remembered for its analysis of lynching, it was also an early effort to assemble evidence linking economic performance to voter support for an incumbent administration. The evidence for the former proposition now seems quite weak, whereas the influence of macroeconomic outcomes on voting behavior has been established repeatedly in a variety of different countries (cf. Lewis-Beck, 1990). It is telling that the economic stewardship of the incumbent regime is a primary focus of elite discourse during electoral campaigns, whereas justifications of lynchings offered by the public officials of the time tended to focus on the moral, rather than economic, depredations of Blacks (National Association for the Advancement of Colored People, 1919). The relationship between economic discontent and intergroup aggression may hinge, then, on the ways in which political leaders and organizations frame and mobilize such grievances.

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Appendix A

Models Used to Prewhiten Series: Lynching Analysis

Series 1. Number of Blacks lynched, 1882–1930, as a function of a second-order polynomial in time and AR(1) errors. Data from Hovland and Sears (1940). Used in Table 1.

$$Y = 83.016 - 1.913(YR) - 0.051(YR^2) + e[1 - .67B]^{-1} \quad \text{(A1)}$$

Adjusted $R^2 = .80$. $Q(12) = 8.8$, $p = .64$. Standard error = 15.84. N of residuals = 48. White's F test for heteroskedasticity: 1.78, $p = .16$.

Series 2. Number of Blacks lynched, 1882–1938, as a function of a linear time trend and ARMA(1,3) errors. Data from Guzman (1952). Used in Table 2.

$$Y = 70.722 - 2.019(YR) + e[1 - .39B^3][1 - .64B]^{-1} \quad \text{(A2)}$$

Adjusted $R^2 = .84$. $Q(12) = 6.7$, $p = .76$. Standard error = 15.00. N of residuals = 56. White's F test for heteroskedasticity: 2.70, $p = .08$.

Series 3. Base-10 log of 1 plus the number of Blacks lynched, 1882–1938, as a function of a second-order polynomial in time and ARMA(1,6) errors. Data from Guzman (1952). Used in Table 2.

$$Y = 1.910 - 0.014(YR) - .0006(YR^2) + e[1 - .34B^6][1 - .32B]^{-1} \quad \text{(A3)}$$

Adjusted $R^2 = .88$. $Q(12) = 12.7$, $p = .24$. Standard error = .127. N of residuals = 56. White's F test for heteroskedasticity: 3.73, $p = .02$.

Series 4. Ayres index, 1882–1938, as a function of a second-order polynomial in time and ARMA(1,5) errors. Data from Ayres (1939). Used in Table 2.

$$Y = 4.617 - 0.019(YR) - .0322(YR^2) + e[1 - .42B^5][1 - .52B]^{-1} \quad \text{(A4)}$$

Adjusted $R^2 = .62$. $Q(12) = 10.7$, $p = .39$. Standard error = 8.21. N of residuals = 56. White's F test for heteroskedasticity: 2.84, $p = .05$.

Series 5. Percentage change in real gross national product per capita, 1882–1938. Data are from *Historical Statistics of the United States: Colonial Times to 1970* (Series F-15) and Kuznets (1961); Table R-40).

No whitening required: $Q(12) = 8.2$, $p = .77$. White's F test for heteroskedasticity (with a linear trend term): 3.3, $p = .05$.

Series 6. Base-10 log of 1 plus the farm value of cotton, 1882–1938, as a function of a second order polynomial in time and AR(1) errors. Data from Ayres (1939) and updated using Table 24 of U.S. Department of Agriculture (1951–1952) *Statistical Bulletin 99: Statistics on Cotton and Related Data.*

$$Y = 1.360 + .0098(YR) - 0.0003(YR^2) + e[1 - .70B]^{-1} \quad \text{(A5)}$$

Adjusted $R^2 = .69$. $Q(12) = 10.7$, $p = .47$. Standard error = .093. N of residuals = 56. White's F test for heteroskedasticity: 5.58, $p = .002$.

Series 7. Number of anti-Black lynching events as a quadratic function of time and AR(1) errors. Data from Tolnay and Beck (1995).

$$Y = 31.27 - .40(YR) - 0.0337(YR^2) + e[1 - .21B]^{-1} \quad \text{(A6)}$$

Adjusted $R^2 = .61$. $Q(12) = 6.0$, $p = .88$. Standard error = 6.90. N of residuals = 48. White's F test for heteroskedasticity: .59, $p = .62$.

Note. The model specifications used to remove serial dependencies from each time series are described using notational system (see Box, Jenkins, & Reinsel, 1994; Hepworth & West, 1988): B = the backshift operator, such that $Be_t = e_{t-1}$; Y = the series to be prewhitened; e = disturbances associated with the series Y; AR(p) = pth order autoregressive disturbances; ARMA(p,q) = an autoregressive moving average model with p and q order lags; ARIMA(p,1,q) = an autoregressive moving average in which the dependent variable is the difference between Y and Lag Y; YR = year minus 1906; ARCH LM = A Lagrange multiplier test for autoregressive-conditional heteroskedasticity among the disturbances; $Q(k)$ = Ljung-Box statistic for assessing serial correlation among the disturbances at the kth lag. Other series used in Tables 1 and 2 follow the whitening procedures in Hepworth and West. All estimates were obtained using Econometric Views 2.0 software (Lilien et al., 1994).

(*Appendixes continue*)

Appendix B

Models Used to Prewhiten Series: Contemporary Hate-Crime Analysis

Series 1a. Number of anti-Semitic hate crimes, as a function of AR(1) errors. Used in Table 3.

$$Y = 13.23 + e[1 - .35B]^{-1} \quad \text{(B1)}$$

Adjusted $R^2 = .12$. $Q(12) = 8.94$, $p = .63$. Standard error = 5.99. N of residuals = 107. ARCH LM F test: .04, $p = .84$.

Series 1b. Number of anti-Semitic hate crimes, as a function of AR(1) errors and linear trend.

$$Y = 10.28 + .05(\text{month}) + e[1 - .31B]^{-1} \quad \text{(B2)}$$

Adjusted $R^2 = .14$. $Q(12) = 10.55$, $p = .48$. Standard error = 5.92. N of residuals = 107. White's F test for heteroskedasticity: 1.13, $p = .33$.

Series 2a. Number of anti-Black hate crimes, as a function of AR(1) errors. Used in Table 3.

$$Y = 9.23 + e[1 - .41B]^{-1} \quad \text{(B3)}$$

Adjusted $R^2 = .17$. $Q(12) = 12.37$, $p = .74$. Standard error = 4.06. N of residuals = 107. ARCH LM F test: 2.88, $p = .09$.

Series 2b. Number of anti-Black hate crimes, as a function of AR(1) errors and linear trend.

$$Y = 12.82 - .06(\text{month}) + e[1 - .25B]^{-1} \quad \text{(B4)}$$

Adjusted $R^2 = .25$. $Q(12) = 9.50$, $p = .58$. Standard error = 3.86. N of residuals = 107. White's F test for heteroskedasticity: 1.94, $p = .15$.

Series 3. Number of anti-Asian hate crimes. Used in Table 3.

No whitening required. No linear or nonlinear trends. $Q(12) = 12.06$, $p = .44$. N of residuals = 108. White's F test for heteroskedasticity (with insignificant linear trend): .32, $p = .72$.

Series 4. Number of anti-White hate crimes, as a function of AR(1) errors.

$$Y = 6.30 + e[1 - .37B]^{-1} \quad \text{(B5)}$$

Adjusted $R^2 = .16$. $Q(12) = 6.72$, $p = .82$. Standard error = 4.90. N of residuals = 107. ARCH LM F test: .15, $p = .69$. No significant linear or nonlinear trends.

Series 5a. Number of anti-gay and -lesbian hate crimes, as a function of AR(1,12) errors. Used in Table 3.

$$Y = 6.39 + e[1 - .33B]^{-1}[1 - .27B^{12}]^{-1} \quad \text{(B6)}$$

Adjusted $R^2 = .22$. $Q(12) = 9.73$, $p = .46$. Standard error = 3.35. N of residuals = 96. ARCH LM F test: .12, $p = .73$.

Series 5b. Number of anti-gay and -lesbian hate crimes, as a function of ARMA(1,5) errors and quadratic trend.

$$Y = .21 + .22(\text{month}) - .0016(\text{month}^2) + e[1 - .21B^5][1 - .29B]^{-1} \quad \text{(B7)}$$

Adjusted $R^2 = .29$. $Q(12) = 13.48$, $p = .20$. Standard error = 3.22. N of residuals = 107. White's F test for heteroskedasticity: 1.10, $p = .35$.

Series 6a. Unemployment rate in four largest boroughs of New York City, as a function of ARMA(1,1) errors. Used in Table 3.

$$Y = 8.28 + e[1 - .40B][1 - .97B]^{-1} \quad \text{(B8)}$$

Adjusted $R^2 = .87$. $Q(12) = 11.84$, $p = .30$. Standard error = .78. N of residuals = 107. ARCH LM F test: .04, $p = .84$.

Series 6b. Unemployment rate in four largest boroughs of New York City, as a function of ARMA(1,1) errors and quadratic trend.

$$Y = -1.20 + .29(\text{month}) - .0019(\text{month}^2) + e[1 - .41B][1 - .90B]^{-1} \quad \text{(B9)}$$

Adjusted $R^2 = .88$. $Q(12) = 12.62$, $p = .25$. Standard error = .75. N of residuals = 107. White's F test for heteroskedasticity: 2.07, $p = .11$.

Series 6c. Unemployment rate in four largest boroughs of New York City, as a function of ARIMA(0,1,1) errors.

$$[1 - B]Y = .01 + e[1 - .42B] \quad \text{(B10)}$$

Adjusted $R^2 = .10$. $Q(12) = 11.63$, $p = .39$. Standard error = .78. N of residuals = 107. ARCH LM F test: .01, $p = .91$.

Note. The model specifications used to remove serial dependencies from each time series are described using notational system (see Box, Jenkins, & Reinsel, 1994; Hepworth & West, 1988). B = the backshift operator, such that $Be_t = e_t - 1$; Y = the series to be prewhitened; e = disturbances associated with the series Y; AR(p) = pth order autoregressive disturbances; ARMA(p,q) = an autoregressive moving average model with p and q order lags; ARIMA($p,1,q$) = an autoregressive moving average in which the dependent variable is the difference between Y and Lag Y; month = months elapsed since January 1987; ARCH LM = A Lagrange multiplier test for autoregressive-conditinoal heteroskedasticity among the disturbances; $Q(k)$ = Ljung-Box statistic for assessing serial correlation among the disturbances at the kth lag. All estimates were obtained using Econometric Views 2.0 software (Lilien et al., 1994). Data are from New York Police Department and the U.S. Bureau of Labor.

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