



# Violent Crime Rate Studies in Philosophical Context: A Destructive Testing Approach to Heat and Southern Culture of Violence Effects

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The logic behind the translation of conceptual hypotheses into testable propositions was illustrated with the heat hypothesis. The *destructive testing* philosophy was introduced and applied. This consists of first showing that a predicted empirical relation exists, then attempting to break that relation by adding competitor variables. The key question in destructive testing is "How difficult was it to break the relation?" This approach was used to analyze the heat effect on violent crime rates (Study 1) and on White violent crime arrest rates (Study 2) in U.S. cities. One competitor variable was the particular focus of analysis: southern culture of violence. The heat hypothesis was supported by highly significant correlations between the warmth of a city and its violence rate. This heat effect survived multiple destructive tests. Some support for the southern culture effect was also found, but this effect was more easily broken.

Uncomfortably hot temperatures are associated with increases in aggressive behaviors of many kinds. Field and archival studies have shown that heat is associated with increased rates of murder, rape, assault, spouse abuse, riots, aggressive horn honking, batters getting hit by baseballs, and prison inmate violence (Anderson, 1987, 1989; Anderson & Anderson, in press; Carlsmith & Anderson, 1979; deFronzo, 1984; Haertzen, Buxton, Covi, & Richards, 1993; Kenrick & MacFarlane, 1984; Michael & Zumpe, 1986; Reifman, Larrick, & Fein, 1991). Conversations with colleagues, questions at professional conferences, as well as debates in the literature (e.g., Anderson & DeNeve, 1992; Bell, 1992; Nisbett, 1993; Rotton, 1993) make it obvious that there is both considerable interest in and confusion about the heat effect, how to research it, and the best explanations for it. One problem has been some inconsistency in the meaning of key terms. For this reason, we hereby specify that the *heat effect* is the observation that aggression rates are often positively associated with warmer temperatures and that the *heat hypothesis* is the theoretical conception that uncomfortably warm temperatures produce increases in aggressive motives and (sometimes) aggressive behavior. Another problem concerns ambiguity about the theoretical status of cold temperatures. However, though cold effects are of interest in some contexts (Anderson, Anderson, & Deuser, in press), they are not relevant to the conditions under consideration in this article and will not be discussed further here.

Other difficulties in the heat-aggression literature are not so easily dismissed. We cannot hope to adequately discuss the controversies, strengths, and weaknesses of all the various types of

research on the heat hypothesis in one article. This article focuses on one type of research—the type that Anderson (1989) labeled *geographic region effects*. We chose this type for two reasons: (1) it has some unique logical and methodological problems associated with it, problems that require careful thought about the whole logic of hypothesis testing, and (2) an interesting alternative explanation of the heat effect has been proposed for some geographic region studies of the heat hypothesis, one that we can test empirically. Specifically, several scholars have proposed a *southern culture of violence* theory (SCVT) that may account for much of the variance in violent crime rates among U.S. cities (e.g., Brearley, 1932; Gastil, 1971; Nisbett, 1993; Rotton, 1993; Wyatt-Brown, 1986).

The three main goals of this article are as follows. First, we discuss the logic of examining different types of heat effect studies in research on violent behavior while exploring how the heat hypothesis and the SCVT alternative explanation may be construed as competing or as complementary factors. In this context, the logic of "destructive testing" is introduced. Second, we explore in some detail the weaknesses and strengths of the geographic region approach. Third, we present empirical tests of the unique contributions made by temperature and southern culture variables to the prediction of violence in U.S. cities, using the destructive testing approach.

## Philosophy of Science and the Study of Geographic Region Effects

### *Multiple Layers*

The study of any conceptual notion requires translation into empirically testable propositions, often through several layers of increasing operational specificity. At each level certain assumptions must be made. As one moves from the most abstract (conceptual) level to the most concrete (empirical) level, the number of predictions increases. Ultimately there are many specific empirical instantiations of the same general conceptual hypothesis. Multiple tests of the same hypothesis become possible. Some will be more trustworthy than others, primarily be-

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cause of differences in the assumptions that they carry. For instance, one assumption common to all the empirical-level tests is that the key variables are measured (or manipulated) reliably. For some specific studies this may safely be assumed to be true; for others such an assumption may be implausible. For yet other studies it may be possible to empirically assess the validity of that assumption.

The heat hypothesis—that uncomfortably warm temperatures increase aggressive motivation under certain circumstances—is one such conceptual hypothesis. Figure 1 depicts a simple version of the multiple levels involved, from the most conceptual to the most empirical. Level 1 simply depicts the conceptual hypothesis concerning heat and aggressive motives. Level 2 specifies some of the ways this effect might be manifested. Level 3 becomes even more specific by describing the unit or the setting of the manifestation. Level 4 sketches several specific empirical realizations (Aronson, Ellsworth, Carlsmith, & Gonzales, 1990) of aggressive behaviors expected to be influenced by excessive heat. (Not shown in this figure is another set of pathways involving assessment of aggressive motives in nonbehavioral ways.)

At each level certain key assumptions are made. For example, one key assumption between Level 1 and Level 2 is that “all else is equal.” For example, a proper test of temperature-based geographic regional differences in aggressive motives requires the assumption that the people living in the hotter and cooler regions do not differ in the “aggressiveness” of their genetic makeup. Other assumptions come into play at various levels and depend on the specific theory under consideration. For example, in moving from the general hypothesis that “heat increases aggressive motives” to a more specific prediction that “heat increases aggressive behavior,” the Negative Affect Escape Model (Anderson & DeNeve, 1992; Baron, 1979; Baron & Bell, 1976) must assume that in the test situation the desire to escape from the situation is either: (a) lower than the aggressive motive or (b) clearly impossible to achieve. Finally, the translation of any theory into concrete operationalizations involves assumptions about the adequacy of the empirical methods used to test

that theory, usually conceptualized in terms of validity, reliability, appropriateness of sample, and so on.

One key consequence of making such assumptions is that no single test of a conceptual hypothesis is definitive, whether it supports or contradicts the prediction. The conceptual hypothesis is insulated by the multiple layers of conjecture that necessarily accompany operationalization. Obviously, some empirical tests are more conclusive than others. The most definitive stand out in the history of science precisely because they are so unusual in their definitiveness. One of our favorite examples is Harlow's (1958) study on wire and terrycloth “mothers” of infant monkeys. Even though all food was given by means of a nipple on the wire mother, the monkeys spent most of their non-feeding time with the soft terrycloth mother. This finding strongly contradicted one prevailing conceptual hypothesis of the time—that infant-mother attachment results solely from the mother being the infant's source of food.

Another consequence of such assumptions is that null results are usually less informative than confirming results. How does this happen? Most assumption violations reduce the likelihood of obtaining the predicted results rather than increase it. When a study “works,” researchers see it as validation of various untested assumptions as well as confirmation of the conceptual hypothesis. When a study doesn't work, the common reaction is to acknowledge that there are many possible reasons for a specific study to fail, only one of which is that the conceptual hypothesis may be wrong. For instance, the failure may be attributed to a poor empirical realization of the dependent variable.

Though this type of thinking is self-serving in some respects, it is usually a reasonable way to approach science. Indeed, the history of scientific advances is filled with examples of a valuable new paradigm “coming together” only after a long search for ways to “properly” test new conceptual notions (cf. Kuhn, 1962). Within social psychology the development of the forced-compliance paradigm to test cognitive dissonance theory is a prime example. Festinger is reported to have tried a number of ways to experimentally test dissonance theory, with limited

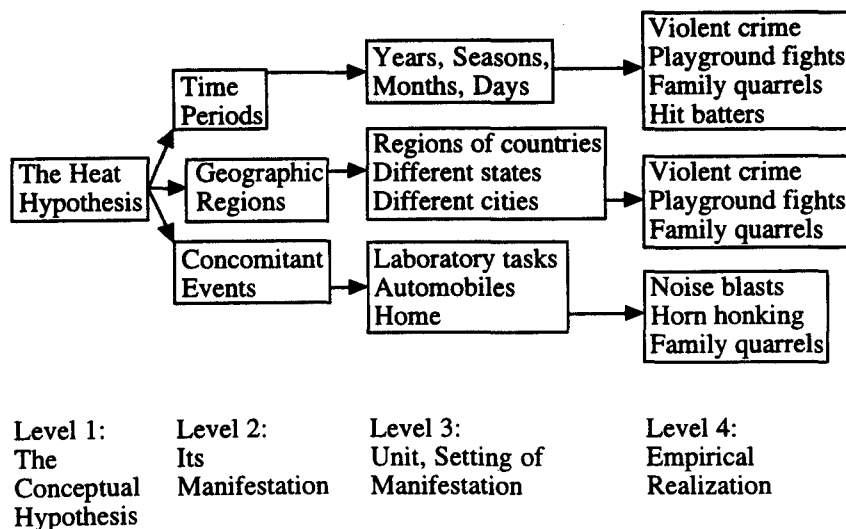


Figure 1. Multiple levels from a conceptual hypothesis to empirical realizations: The heat hypothesis.

success. Finally, Festinger and Carlsmith (1959) succeeded in getting the various parameters set in the range necessary for replicable dissonance results to be obtained.

### *Falsifiability and Triangulation*

Despite this tendency to ignore null results, the key to testing conceptual hypotheses remains conducting empirical tests that are falsifiable. The fewer untested assumptions necessary to get from the conceptual hypothesis to the empirical test, the more falsifiable and powerful the test becomes. Whenever a conceptual hypothesis survives an empirical test, it gains strength. When it survives many tests that use many varied methods, we become even more confident that the hypothesis is correct. This follows directly from the analysis of assumptions and the notion of falsifiability. Different methods are likely to involve different assumptions. When a conceptual hypothesis survives many potential falsifications based on different sets of assumptions, we have a robust effect.

This is the basic logic of triangulation that the first author (Anderson, 1989) applied to the heat hypothesis. That review concluded that the evidence from a wide array of field and archival studies supported the heat hypothesis. Subsequent studies that used still more varied techniques contribute additional support to this triangulation-based conclusion. For example, Reifman et al. (1991) showed that the likelihood of a batter getting hit by a pitched baseball increases at hot temperatures. Similarly, Haertzen et al. (1993) showed that prison inmate violence increases during the summer months.

Triangulation can fail if there is some flawed assumption that is both: (a) common to all the approaches and (b) violated in a way that biases the data in a supportive rather than a contradictory direction. Our present confidence in the basic heat hypothesis and in the heat effect derives largely from the unlikelihood of these necessary features both being true.

Still, it is quite possible that a particular type of study within the overall set can be explained by alternative conceptual hypotheses. The existence of a good alternative explanation for a given finding should reduce the supportive value of that finding, but it does not transform the finding into a falsification. How much impact the alternative explanation should have depends on several factors, including its range of applicability. If the SCVT alternative adequately accounts for differences in violent crime rates among cities, support for the heat hypothesis from this geographic region type of study should be weakened. However, the fact that the SCVT cannot account for other heat effects (e.g., none of the time period or concomitant effects) limits its relevance to the heat hypothesis. On the basis of parsimony one would still be tempted to attribute the observed city differences in violent crime to heat, though with lessened confidence. Thus, although it is important to explore plausible alternative explanations, the logic of triangulation provides a broader perspective that should not be ignored.

### *Controlling for Flaws Versus Destructive Testing With Competitors*

In this final philosophy of science section we wish to highlight differences between controlling for flaws versus using competitors in a destructive testing process. This distinction is relevant

to any domain of science, but here we focus on its relevance to testing heat effects in geographic region type studies. It is important to note that this is a distinction that we have only recently come to understand and that one of our earlier archival studies (Anderson, 1987) could have used to clarify some points but did not do so. We believe that this distinction will be helpful to others as well.

The process of conducting empirical research is largely a matter of trying to control for various irrelevant factors so that a pure and sensitive indicator of the relation between the independent variable (IV) and the dependent variable (DV) can be obtained. The goal is to rule out alternative explanations. Alternative explanations come in two very different forms, however. Psychologists (and perhaps other scientists) frequently fail to distinguish between the two forms. One is what we have here labeled *flaws*. The other we label *competitors*.

*Flaws.* Flaws are alternative explanations resulting from clear-cut mistakes. Let's assume that a research team wants to test the conceptual hypothesis that living in large urban environments increases aggression. They sample cities ranging in size from 20,000 to 2 million and correlate size with the number of violent crimes committed in those cities. They would find that the number of violent crimes is higher in the larger cities than in the smaller ones. An obvious alternative explanation is that the greater number of crimes in the larger cities is solely the result of the greater population, not that the urban environment somehow caused urban citizens to be more aggressive. This is an example of an alternative explanation based on a flaw. The proper DV for the hypothesis under consideration would have been rate of violent crime rather than number of violent crimes. In more formal terms, a flaw exists when the alternative explanation demonstrates a translation error in the empirical realization of either the IV or the DV. Although most flaws are filtered out in the journal reviews, subtle ones occasionally survive the review process and can be found in most research literatures.

*Competitors.* Contrast the flaw example with the following hypothetical case. A research team wants to test the conceptual hypothesis that heat increases aggressive motives. Their empirical operationalization is to create a heat index for a large number of cities and to correlate it with the violent crime rate in those cities. They also assess the "southernness" of each city. The zero-order correlation between heat and violent crime is positive, as is the correlation between southernness and violent crime. Finally, the partial correlations between each of these two predictors and violent crime rate are zero when the other predictor is statistically controlled. We maintain that even in this case, the zero-order correlation between heat and aggression does not constitute a flaw. The theory predicted that the correlation would be positive, and it was. Although the alternative explanation—southernness—is certainly a competitor to the heat explanation, this situation does not constitute a methodological flaw. More formally, a competitor: (a) is an alternative explanation for a well-specified and measured effect, (b) is based on an additional variable that is or may be in some way confounded with the target variables, (c) is hypothesized to eliminate the predicted relation between the target variables.

Competitor variables themselves vary considerably in theoretical and epistemological status. What effect should their existence in a given study have on judgments concerning the conceptual hypothesis? It depends on the specific procedures used

to generate and test for competitors and on the theoretical status of each competitor.

Figure 2 illustrates what happens when a competitor is partialled out of the main IV-DV relation. Each circle represents that variable's variance. Overlapping regions indicate the proportion of shared variance, in other words, the correlation (actually  $r^2$ ) between the two variables. (See any multiple regression textbook for further descriptions of partitioning variance among multiple sources and for use of Venn diagrams in illustrating these descriptions, e.g., J. Cohen & Cohen, 1983; Pedhazur, 1982.) In Figure 2, A + C represents the relation between heat and violent crime rates across a sample of cities. Similarly, A + B represents the relation between some competitor variable (or set of competitors—see J. Cohen & Cohen, 1983) and violent crime. Region A, of course, also represents the portion of violent crime variance that is shared by heat and its competitor, frequently called the *confounded variance*. Regions C and B represent, respectively, the variance in violent crime rates uniquely associated with heat and its competitor. For simplicity, let's assume that all the shared (confounded) variance in violent crime rates of cities is actually due to differences in heat. If one includes a competitor that happens to correlate positively with both heat and aggression, then partialing it out reduces the estimate of the true heat effect in an inappropriate way. Even if only a portion of the confounded variance (Area A in Figure 2) actually belongs to heat, standard statistical procedures remove all of it and thus overcorrect for the competitor. This problem is compounded when large numbers of competitors are considered willy-nilly and are kept because they have some impact. The problem is reduced when competitors are restricted to those with well-established theoretical ties to the conceptual variables of interest.

In both the "willy-nilly" and the "theoretically meaningful" cases, the introduction of competitors is a type of *destructive testing*, a term borrowed from structural engineering and materials science (Timoshenko, 1953; Wilson, 1984). The analogy works this way: A new metal alloy has been developed, and initial tests have shown it to be fairly strong; to find out just how strong it is, a series of destructive tests are conducted. That is, increasing stresses are applied to a sample of the new alloy until it breaks. There is no question about whether it can be broken, only how much stress it can take before it does break. In a sense,

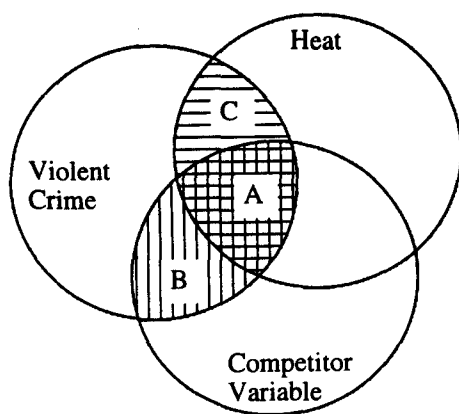


Figure 2. Destructive testing with competitor variables.

destructive testing is a hybrid of the traditional theory-centered approach to science and the result-centered approach advocated by Greenwald, Pratkanis, Leippe, and Baumgardner (1986) as a means of avoiding bias resulting from the theory-centered approach.

The initial test of strength is analogous to our zero-order correlation. It is a simple test of the basic theoretical hypothesis. Adding increasing stresses is analogous to trying out different competitors and combinations of competitors. The relevant question about the obtained relation between heat and aggression is not "Can it withstand all possible attempts to reduce it to nonsignificance?" What is of interest in such destructive testing is how much stress the target relation can withstand. The ultimate judgment concerning the strength of the target relation (here, heat and aggression) is somewhat subjective and will therefore differ from scholar to scholar. However, there are reasonable rules of thumb that all scholars can apply. If a large number of competitors is tried and the most stressful ones kept, one should expect to see destruction occur fairly early and should not downgrade the strength estimate too much. Furthermore, a priori competitors with good theoretical and conceptual grounding should be taken more seriously than post hoc competitors of dubious conceptual relevance.

Similarly, if part of the destructive testing process consists of degrading the quality of the target IV or DV, then competitors should succeed in destroying the effect even earlier. Normally, researchers would not consider degrading their measures of key constructs. However, such degrading of IVs and DVs might be of interest in cases where the best measures are composites. For instance, in the heat effect literature it is common to create a composite violent crime index based on standardized measures of murder, rape, and assault rates. One might be interested in seeing how well the general conceptual hypothesis fares for each type of crime separately. When this is done, however, it is imperative to remember that degrading the measure in this way is itself a form of destructive testing.

### *Destructive Testing Versus Statistical Equating*

How does the perspective of destructive testing relate to another common view of multiple regression procedures known as *statistically equating the observations on other control dimensions*? Statistically, the approaches are similar. One examines the effect of the target IV (e.g., heat) and partials out variance associated with the competitors (in the case of destructive testing) or with the controls (in the case of statistical equating). Conceptually, however, they are quite different. Destructive testing assumes that one has started with a significant effect, that is, a predicted zero-order correlation between two variables of interest. Statistical equating makes no such assumption. Indeed, the only test of relevance in a statistical equating procedure occurs after the control variables have been statistically controlled.

A second difference in the two approaches concerns the importance of selecting competitor (control) variables with theoretical relevance. As noted earlier, for destructive testing the competitor variables need not have a priori theoretical relevance to be of interest, though such relevance does play a role in interpreting the strength of the original effect. For statistical equating purposes, however, we maintain that a firm theoretical

grounding is necessary. If the main hypothesis test is to come only after statistically equating for control variables, then a fair test requires that the control variables be theoretically meaningful and distinct from the independent variable of interest. This is important for two reasons. If researchers allow themselves too much free rein, they can conduct too broad a search for control variables that enhance (by means of suppressor-like effects) the estimated effect of the target IV, or they can conduct too broad a search for control variables that depress (by means of confounding effects) the estimated effect of the target IV. Requiring a control variable to be a priori theoretically meaningful and distinct from the target IV helps us all avoid fooling ourselves or our colleagues.

One example of a control variable that does not meet this requirement in the heat-city-crime example described earlier is the use of latitude as an indicator of southernness (e.g., Rotton, 1993). As will be discussed more fully in a later section, SCVT posits that a certain region of the United States, specifically the Old South, developed a culture of violence that persists to this day. Conceptually, latitude does not well represent this idea because many cities with southern latitudes are not southern by the theoretical perspective and because there are some midlatitude cities (e.g., in Virginia) that are southern by the SCVT. Latitude certainly correlates with southernness as described by SCVT. However, it is probably even more highly correlated with heat indexes for U.S. cities. That is, latitude may be seen as a fairly good substitute measure of how hot various U.S. cities are. Indeed, some early studies of the heat hypothesis used latitude as an indirect measure of hotness of climate (Lombroso, 1899/1911). Thus, when one statistically controls for latitude, one is probably equating the cities more on temperature than on cultural southernness. In other words, latitude is neither a meaningful index of SCVT nor is it sufficiently distinct from heat indexes to qualify as a control variable. As should be obvious, latitude is also a less-than-ideal measure of temperature. Even within the continental United States, other geographic features, such as proximity to large bodies of water, prevailing currents in these bodies of water, and prevailing wind patterns influence the climate of a city. One could use latitude in a destructive testing process, but its lack of conceptual meaningfulness and empirical distinctiveness means that a successful "break" should have little impact on judgments of heat effect strength. The finding that partialing out one temperature surrogate measure (latitude) reduces or eliminates the violent crime effect of another (heat index) is not informative.

In sum, there are similarities and differences between the destructive testing and the statistical equating approaches to analyzing and interpreting such complex data. Very often the differences don't matter, but sometimes they do. We believe that the best approach to tests of the heat effect in the city violent crime studies is the destructive testing approach. We illustrate this approach in presenting our results later in this article.

### Weaknesses and Strengths in the Geographic Region Approach to the Heat Hypothesis

#### *Weaknesses*

There are three weaknesses common to all geographic region studies of the heat hypothesis. First, these studies are necessarily

correlational in nature. This precludes the use of random assignment procedures to control for extraneous variables. Thus, one must make a number of assumptions to fairly test the heat hypothesis. Some of the assumptions are untestable. Of these, some seem fairly safe to make. For example, it seems likely that people living in different geographic regions do not systematically differ in their genetic predispositions to aggression.

Second, multicollinearity among potentially relevant variables is high. In other words, there are lots of extraneous differences between geographic regions that differ in temperature, and some of those differences may themselves be correlated with aggression and with temperature. Generally, such confounds are more likely to hurt a specific hypothesis than to help it, but it is certainly possible that such confounds could artifactually create an apparent heat effect.

The third problem is sometimes called the *ecological fallacy* (see Pedhazur's [1982] discussion of "cross-level inferences"). The dependent variable of interest (aggression) is measured at an aggregate level (e.g., murder rate in a city), but the researcher is primarily interested in drawing inferences about individuals. Even if the data show that aggression is more prevalent in hotter regions than in cooler ones, they won't show whether the aggression occurred during the hotter periods of time. This problem is lessened to some extent by other studies that have shown that aggressive behavior is more prevalent during hotter years, hotter seasons, hotter months, and hotter days than during cooler ones (Anderson, 1989; Anderson & Anderson, in press; Anderson & Groom, 1995). A better solution to the ecological fallacy problem derives from the logic of multiple levels of translation and falsifiability. Even though the heat hypothesis is stated at the level of individuals, one translation of the hypothesis leads to a clear prediction framed at the aggregate level. The aggregate-level prediction (e.g., warmer cities will have higher violent crime rates than cooler ones) is falsifiable, so the test is a legitimate one. Nonetheless, the dangers of cross-level inferences must be kept in mind.

#### *Strengths*

There are six strengths shared by geographic region studies of the heat hypothesis. First, the dependent measures of aggression are clear and meaningful. Violent crime rates are unquestionably important indicators of aggression and aggressive motives.

Second, these indicators of aggression can be quite reliable as well. Reliability of such indicators depends on at least four factors: population size, time period, frequency, and reporting bias. Population size and time period combine to produce the unit of analysis. Murder rates in small towns over a 1-month period will be unreliable, because there are so few murders in this population/time unit. Murder rates in large countries over a 1-year period will be quite reliable. The frequency of the behavior also influences reliability. Murder is considerably less frequent than rape, which in turn is less frequent than assault. In this domain, less frequent behaviors generally produce less stable rate estimates. Finally, bias in reporting rates influences reliability. We know that rape is highly underreported, for instance. If the underreporting is constant across the various observational units (e.g., cities), then it has relatively little impact on the reliability of rape rate measures. However, systematic bias in underreporting will certainly decrease the utility of the measure. Murder, of course, seldom goes unre-

ported (or undiscovered). In most geographic region studies all three measures are likely to be quite reliable. An even more reliable index of aggressive behavior can be produced by combining these specific violent crime rates into an overall composite index.

A third strength of geographic region studies is that the heat hypothesis makes a clear prediction that warmer regions should have higher aggression rates. This hypothesis is somewhat protected by the multiple translation layers between it and its empirical realization, as is true of any conceptual hypothesis. However, it is less screened from falsification than is often the case in the social sciences.

The fourth strength concerns Type I errors. Extraneous confounds are at least as likely to weaken as to strengthen the predicted heat effect. This is a weakness from the standpoint of detecting a true heat effect, but it is a strength in terms of protecting against "discovering" a false heat effect.

Fifth, the strongest confounds are likely to be variables that can be measured and assessed, either by means of the destructive testing or the statistical control approach. For example, it is well known that murder rates are higher among the residents of impoverished inner cities. One can assess degree of poverty in the cities and use this information either as a competitor or as a control variable.

Sixth, problems associated with geographic region studies differ considerably from those associated with other types of heat hypothesis studies. From the triangulation perspective this reduces the probability that consistently supportive results could occur from the same underlying systematic bias. The different lines of research can thus provide converging evidence.

### City Violence, Heat, and Theories of Violent Southern Culture

Several researchers have tested the heat hypothesis by examining violent crime rates in different cities (see Anderson, 1989, for a review). These studies have consistently revealed higher aggression rates in warmer cities. Our views on several issues have changed somewhat, however, and are best discussed in the context of a study the first author reported several years ago (Anderson, 1987). In this section we summarize that study, noting some specific choices made and highlighting changes in our thinking that will be reflected in the two studies we report in this article. Finally, we summarize the SCVT notion and its relation to the heat hypothesis.

#### *Anderson (1987), Study 2*

*Summary of main findings.* This study examined violent and nonviolent crime rates in the United States in 1980 for 260 Standard Metropolitan Statistical Areas (hereafter called *cities*) as a function of various climate variables and 14 social variables such as poverty, unemployment, and education. The social variables were used to create social models of various crime indexes.<sup>1</sup> Then climate variables (e.g., number of hot days, number of cold days) were entered into the regression model to see if they added unique variance to the prediction of crime rates. Each of the four temperature-related variables significantly predicted violent crime rate.

*Choices.* In constructing and analyzing this data set, several difficult choices were made. For instance, many cities had several

possible weather reporting stations from which to choose. Although we no longer have exact records of how this was done (data were gathered in 1984), the first author recalls that an attempt was made to use the weather station closest to downtown, rather than airport stations. Other cities had no weather stations reporting data for 1980. In those cases, a weather station from an adjacent Standard Metropolitan Statistical Area was used. These choices were deemed acceptable, because weather patterns for nearby locations are practically identical.

Another set of choices involved use of composites versus individual measures of the key IV (temperature) and DV (violent crime). The violent crime variables (murder, rape, and assault rates) were combined into a composite violent crime index, whereas the temperature variables (number of hot days, number of cold days, heating degree days, cooling degree days) were not.

Finally, the data analysis was a mixture of what we now call the statistical equating approach and the destructive testing approach. It resembled the statistical equating approach in two ways: (a) control variables were used to create a social model of violent crime, and then the temperature variables were tested, and (b) only the violent crime composite index was used. It resembled the destructive testing approach in three ways: (a) the individual temperature measures were used rather than a composite, (b) competitor variables without a sound theoretical basis were included, and (c) zero-order correlations between temperature and violent crime were reported.

*Changes in thinking.* In the first section of this article we present our current thinking about the logic of testing conceptual hypotheses, including discussions of translation layers between conceptual hypotheses and empirical realizations, falsifiability, and the distinction between the destructive testing and the statistical equating approaches. We included those ideas because we believe they will be useful to many social psychologists working with complex conceptual hypotheses. In addition, those ideas led to our current approach to testing the heat hypothesis with city violence data.

The most important change involves adopting a destructive testing approach. This means that the initial tests should involve the simple correlation of the most reliable indicators of temperature and violent behavior. Thus, we now use a temperature index in which all the measures of the temperature of a city are combined by means of summed *z* scores. Similarly, we believe that a composite *z* score index of violent crime is most appropriate, though this is not a change from the 1987 article. (Use of *z* scores gives equal weight to each component, whereas use of raw scores would weight each component as a direct function of its standard deviation.)

The destructive testing approach also specifies that various competitor variables be added to the model to determine the strength of the predicted heat-aggression link. Furthermore, even though theoretically and empirically sound competitors are to be taken more seriously than others, these other competitors may also be used to test the strength of that link.

Finally, it is important to note that viewing the various measures of temperature as different indicators of how warm a city is has led to a change in view concerning other temperature

<sup>1</sup> Not all 14 social variables were kept in the final social models. See Anderson (1987) for details.

measures. In previous writings the first author has questioned the relevance of 30-year average temperatures to testing the heat hypothesis (Anderson, 1987, 1989). On realizing that even the number of cold days a city experiences in a year can be seen as a (negative) indicator of how warm the city is, we realized that 30-year averages may also be seen as indicators. (Note that other criticisms of prior studies that have used 30-year averages remain valid.) Whether 30-year averages prove to be more or less valuable than our same-year measures is largely an empirical question in which we at present have little interest.

### *Theories of Southern Culture of Violence*

*A brief overview.* In 1748, the French scholar Charles de Secondat Montesquieu related climate to several variables, including crime.

You will find in the northern climates peoples who have few vices, enough virtues, and much sincerity and frankness. As you move toward the countries of the south, you will believe you have moved away from morality itself: the liveliest passions will increase crime. (Montesquieu, 1748/1989, p. 234)

Foreshadowing recent economic approaches, Barry and colleagues (Barry, Bacon, & Child, 1957; Barry, Child, & Bacon, 1959) showed (cross-culturally) that food accumulation practices are related to socialization on dimensions of independence and self-reliance. More directly relevant to present concerns, numerous "culture of violence" explanations have been offered for the high violent crime rates in the U.S. South, ranging from the sociological (e.g., Gastil, 1971; Hackney, 1970) to the evolutionary and economic (Nisbett, 1993). The various viewpoints are not necessarily contradictory.

Sociological approaches focus on the development of southern culture prior to the Civil War, primarily in the Old South and Confederate states. Some attribute a violent southern culture to the relatively long time in which the South was a wilderness frontier (Gastil, 1971; Hackney, 1970). Another explanation posits that Cavaliers initially settled the South and brought with them an ideal of personal honor and virtue that one was expected to defend (Cash, 1941; Nisbett, 1993), though the historian Wyatt-Brown (1986) argued that there was little difference between the Southern and the New England colonies in this regard. The institutionalization of slavery has also been regarded as the key factor in the development of a violent southern culture (Cash, 1941; Franklin, 1956; Gastil, 1971; Hackney, 1970; Wyatt-Brown, 1986). Weapon ownership and pride in military displays are further explanations for the development of a violent southern culture (Franklin, 1956; Gastil, 1971).

The validity of some of these explanations has been challenged on methodological (Erlanger, 1975; Loftin & Hill, 1974) and theoretical grounds. In a reanalysis of Gastil's (1971) study of homicide and SCVT, Bailey (1976) concluded that the effect of southernness on homicide rates diminishes greatly when appropriate socioeconomic variables are partialled out. Some investigations of regional differences in attitudes toward violence (e.g., Erlanger, 1975) and of gun ownership (e.g., O'Connor & Lizotte, 1978) have revealed no differences between southern and nonsouthern samples. Nisbett (1993) clarified that homicide and other violent behaviors are higher in the South than in nonsouthern areas when they are performed in the interest of

self-protection. Southerners and nonsoutherners may therefore differ primarily in violent behavior that is perceived as self-defensive and not on more gratuitous violence. Many of the researchers that have countered SCVT have emphasized the importance of structural, demographic, and situational variables in understanding southern violence.

Nisbett's approach (1993; D. Cohen & Nisbett, 1994) similarly posits an SCVT that revolves around "honor" but focuses on evolutionary aspects that might have contributed to its development. (See Buss [1990] for an excellent general perspective on evolutionary social psychology.) In its simplest form the argument is that various cultural prescriptions develop out of the economic circumstances in which a particular group of people exists. If the means of production require high levels of risk by the male producers, then for that group of people to succeed its males must be socialized to be very protective of their products and to be willing to take high risks. When herding is a major part of the necessary economic system, risks tend to be high because there are always those who are willing to steal from another, and the relative isolation requires individuals to protect their herds (and their honor) by themselves. Nisbett (1993; D. Cohen & Nisbett, 1994) reported several studies in support of this view and suggested that it accounts for the regional differences in homicide found in the United States. Of course, this view also requires the assumption that once a culture of violence develops it will persist even after the economic circumstances giving rise to it have shifted. Otherwise, SCVT would be irrelevant to aggression in urban environments. What is not clear, and what Nisbett, Polly, and Lang (1995) are currently investigating, is how long such cultural differences persist after the economic circumstances change. Of course, this emphasis on the herding aspects of the frontier South is strongly contradicted by Wyatt-Brown's (1986) analysis of the link between the culture of honor and the institution of slavery, which was used primarily for the farming economy, not for herding.

In sum, SCVT views posit that for one or more historical, sociological, or socioeconomic reasons, the Old South region of the United States has developed a more violent culture than other regions. More specifically, this perspective predicts that the Old South should have higher violent crime rates than other regions of the United States.

*Relation to the heat model.* Although both SCVT and the heat hypothesis attempt to explain the high violence rates often found in southern U.S. cities, they need not be seen as mutually exclusive. Nisbett and colleagues (D. Cohen & Nisbett, 1993; Nisbett et al., 1995) have suggested that SCVT might account for regional differences in homicide, and Rotton (1993) specifically postulated that it accounts for the violent crime rate differences reported in Anderson (1987), but we think it very difficult to disentangle these two conceptual hypotheses. This difficulty arises from the multiple ways in which temperature differences among regions could affect aggression, one of which is through the development of a culture of violence. Figure 3 presents several models of how SCVT and heat might be related to violent crime.

Model A depicts SCVT as caused by an extended frontier, the institution of slavery, and a herding economy. (The other sociological causes are left out to clarify the diagram.) More important, in this model SCVT is the only cause of differences in violent crime rates among U.S. cities. Model B similarly allows only

one cause of violent crime differences, but here heat is the cause. Model C depicts the case in which both SCVT and heat independently contribute to violent crime rate differences. If SCVT and heat were only weakly correlated, and if Models A, B, and C were the only possibilities, then we would be tempted to adopt the statistical equating approach in which the two predictors are each tested only after the effects of the other had been statistically removed. However, both premises are false: measures of southernness and heat are more than weakly correlated, and there are at least two more possible models that invalidate the statistical equating approach for this research topic. Model D shows that heat differences among regions may have contributed to the development of a violent southern culture. Reducing or eliminating the heat effect on violent crime by partialing out southernness indicators does not invalidate this model or the importance of temperature. Model E is similar, except that it postulates that historical temperature and contemporary temperature contribute independently to current rates of violent crime, the former through SCVT and the latter more directly. Distinguishing between these latter two models is practically impossible, because the relative heat of cities has not changed from the time when the southern culture developed to the present. In other words, heat measures of cities taken prior to the Civil War (if available) would correlate so highly with contemporary heat measures, that they would become repetitive in the statistical sense (Pedhazur, 1982) and therefore would be impossible to disentangle.

The most one can hope to do with this type of geographic region study is to adopt a destructive testing approach in which the main hypothesis is simply that hotter regions will on average have higher rates of aggressive behavior. If there are no major flaws in the measures, support of this hypothesis would mean

that an opportunity to falsify was successfully passed. Then destructive testing, including use of an SCVT-derived measure of southernness, becomes important.

*Assessing southernness.* Assessing southernness from the perspective of SCVT is not easy. Four procedures have been used and their merits debated. Latitude is one measure that is particularly inappropriate, as discussed earlier. It meets neither the theoretical requirement of capturing the meaning of SCVT nor the empirical requirement of being sufficiently distinctive from measures of temperature.

A direct and theoretically appropriate way to measure SCVT is to classify regions as being southern or not, on the basis of the theory and historical classifications. The U.S. Bureau of the Census defines regions in a way that closely parallels the writings of proponents of SCVT. Thus, one could use a simple two-level south-nonsouth distinction based on U.S. Bureau of the Census classifications. Some scholars view this as too restrictive and point out that SCVT also includes the idea that the culture has been exported to other parts of the country as well. Gastil (1971) created a "southernness index" of states based primarily on migratory patterns from the region where the southern culture supposedly originally developed. More recently, Rotton (1993) proposed that the voting rates for George Wallace in the 1968 presidential election might serve as an indicator of SCVT. Though far from perfect, these latter three measures of SCVT appear to have some value. We used these variables to create a "southern culture of violence index" (SCVI).

Study 1: U.S. City Crime Rates Reexamined

The present study is based on 1980 crime rates in the United States. Because of the long time since publication of Anderson's

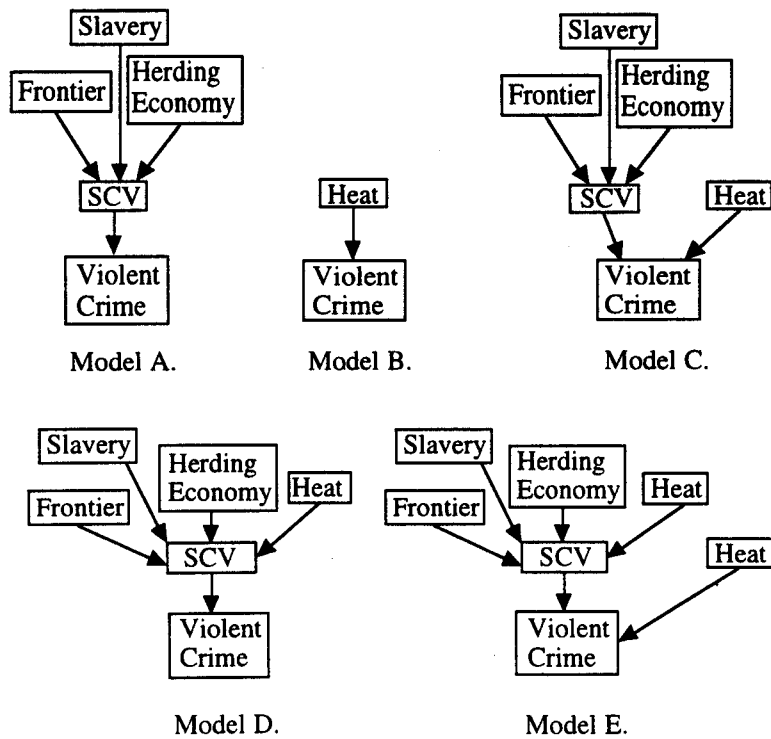


Figure 3. Possible models of the southern culture of violence (SCV) and heat hypotheses.

(1987) article and periodic attempts by the first author to regain valuable storage space, the original data have been discarded. Thus, we created a new data set with several important differences from the original, noted in the descriptions of the variables that follow. The statistical analysis is quite different from the original for two main reasons. First, we have adopted the destructive testing approach. Second, this new data set includes a theoretically meaningful competitor variable that was lacking in the original, namely, SCVI.

### Variables

**Aggressive behavior.** Three violent crime rates were obtained from the 1980 Federal Bureau of Investigation *Uniform Crime Reports of the United States* (U.S. Department of Justice, 1981a). Murder rates were available for 253 cities; rape and assault rates were available for 7 additional cities. Our main focus is on the violent crime index (vcrime), a summed  $z$  score composite of these three indicators of aggressive behavior. This index proved sufficiently reliable, with a Cronbach's alpha of .80.

**Temperature.** We examined the 1980 volume of *Climatological Data: National Summary* for the four indicators of the heat of each city (U.S. Department of Commerce, 1980a, 1980b). These were (a) number of hot days ( $\geq 32.2^\circ\text{C}$ ,  $90^\circ\text{F}$ ), (b) number of cold days ( $\leq 0^\circ\text{C}$ ,  $32^\circ\text{F}$ ), (c) cooling degree days (amount of cooling needed to maintain a comfortable base temperature of  $18.3^\circ\text{C}$ ,  $65^\circ\text{F}$ ), and (d) heating degree days (amount of heating needed to maintain a comfortable base temperature of  $18.3^\circ\text{C}$ ,  $65^\circ\text{F}$ ). A number of cities did not have complete yearly weather data; in Anderson (1987) adjacent cities were used in those cases. In constructing the present data set we were able to locate monthly reports from some of the cities (U.S. Department of Commerce, 1980a), so they were summed and used rather than weather data from adjacent cities. For the remaining cities ( $n = 25$ ) we used weather data from adjacent cities. The selection of adjacent cities was restricted to those not used elsewhere in the data set; the average distance from the target city was just under 19 miles. From these four temperature indicators we created a composite index by converting each to  $z$  scores, multiplying the negative indicators by  $-1$  (number of cold days, heating degree days), and summing. This index yielded a Cronbach's alpha of .93 and is labeled *hotscale* throughout the remainder of the article.

**Southernness.** The main competitor variable of interest was southernness. We created a composite variable by converting three indicators to  $z$  scores and summing them. One indicator was the simple south-nonsouth distinction based on U.S. Bureau of the Census classifications. Cities in southern states were assigned a score of 1; nonsouthern cities were given a score of 0. The second measure of southernness was Gastil's (1971) southernness index. The third indicator was the percentage of voters who voted for George Wallace in the 1968 presidential elections (based on county voting totals; Scammon, 1970). This SCVI yielded a Cronbach's alpha of .89.<sup>2</sup>

**A note on composites.** For statistical reasons we converted all composite measures used in this article to  $z$  scores before performing the various regression analyses. This facilitated examination of some unexpected curvilinear effects of SCVI, reported in later sections.

**Competitor variables.** Twelve additional "social" competitor

variables were included in the destructive testing phases of the analyses. They included: unemployment rate, per capita income, poverty rate, mobility (percentage of the population living in a different home in 1975), high school education (percentage of the  $\geq 25$ -year-old population that had graduated), college education (percentage of the  $\geq 25$ -year-old population that had attended 4 or more years), population size, percentage Black population, percentage Spanish (a U.S. Bureau of the Census designation) population, percentage of population less than 18 years old, percentage of population 18-64 years old, and median age of the population. Data for all of the "social" competitor variables were obtained from the U.S. Department of Commerce (1983a, 1983b).

### Results and Discussion

We examined scatter plots of each predictor variable with each crime measure to see if there were any obvious nonlinear relations. Unexpectedly, SCVI appeared to be curvilinearly related to the crime measures. We therefore included a quadratic term ( $\text{SCVI}^2$ ) in all analyses. As noted earlier, the SCVI score was in  $z$  score format. This reduces the correlation between the linear and quadratic terms and allows simultaneous testing of the linear and quadratic terms. Additional analyses that examined the linear SCVI effects in various models without the quadratic term produced practically identical results in both studies and therefore are not presented.

The first step in destructive testing is to see if the basic hypothesis is supported. The heat hypothesis predicts that warmer cities will be positively associated with more violent behavior. As expected, *hotscale* correlated quite strongly with vcrime,  $r(251) = .61, p < .001$ . Warmer cities had higher violent crime rates than did cooler cities. This sets the stage for a series of destructive tests, to see how strong the relation between temperature and aggressive crime actually is. Figure 4 presents the best-fit line relating temperature to violent crime.

We also examined the relation between SCVI and violent crime. Both the linear and the quadratic components of SCVI were related to violent crime,  $F_s(1, 250) = 114$  and  $31$ , respectively,  $ps < .001$ . Figure 4 displays these SCVI results as well, showing that increases in southernness (SCVI) was related to increased violence, but only up to a certain point. Beyond that point, further increases in SCVI were associated with slight de-

<sup>2</sup> One might reasonably question the inclusion of a two-level (south vs. nonsouth) variable in such a composite index. It might, for instance, reduce the internal reliability of the index and might yield low correlations with the criterion variables. In fact, the reliability of the index is higher with this variable (.89) than without it (.81). In addition, its zero-order correlations with murder, rape, and assault were higher than the corresponding correlations for the Wallace vote indicator. Thus, we deemed it an acceptable indicator for use in the SCVI. One might also wonder whether the differential reliabilities of *hotscale* and SCVI might affect their susceptibility to destructive testing. In the present studies, the differences are small enough that we regard the point as moot. In general, one might expect highly reliable indexes to perform better than moderately reliable ones, but the situation is not that simple. A moderately reliable index occasionally works better than a highly reliable one, specifically when the former is composed of items that each contribute nonredundantly to a purer measure of the underlying conceptual variable.

clines in violent crime. The linear effect is congruent with the various versions of SCVT. The quadratic SCVI effect is not.

*Destructive testing with vcrime and hotscale.* There are numerous (almost uncountable) ways to proceed with destructive testing. We present a systematic series of destructive tests that are simple in conception and direct in implementation. In all cases, we present the results in terms of the partial correlation between a predictor variable and an aggressive-crime variable, after partialing out the effects of a competitor or set of competitors. We conducted the first set of destructive tests using the composite measures of violent crime and of temperature. First, we partialled out the linear and quadratic effects of SCVI. Next, we added 10 of the 12 social predictors described earlier. Because of statistical concerns of a reviewer of an early draft of this work, we did not include the 2 race variables (percentage Black population, percentage Spanish population) in this initial set of 10 social variables. Finally, we added the 2 race variables, producing a total of 14 competitors. The first line of Table 1 presents the results of this first set of destructive tests.

As one can see, the temperature effect survived all three tests, with partial correlations ranging from .21 to .37 (all  $ps < .01$ ). It is important to note that SCVI by itself accounted for about 32% of the variance in vcrime. The 14 competitors (2 SCVI and 12 social variables) as a set accounted for about 66% of the variance in vcrime. Therefore, the survival of the temperature effect cannot be attributed to weaknesses in the competitor variables. Rather, the results suggest that the temperature effect is fairly robust.

*Destructive testing by degrading vcrime.* Our second set of destructive tests paralleled the first, with the additional stress of degrading the vcrime measure into its component parts. In essence, this is like taking a 3-item scale and testing to see if the target effect (here, the heat effect) occurs for each item separately. As can be seen in Table 1, the heat effect is considerably weakened by these additional stresses. Nonetheless, the heat effect still survived all 12 tests ( $ps \leq .05$ ).

*Destructive testing by degrading vcrime and hotscale.* For our third set of destructive tests, we degraded both the main dependent (vcrime) and independent (hotscale) variables, by

breaking them into their component parts. Then we examined the heat effect after further stressing it by partialing out the SCVI and the social competitors. Table 2 presents the results of these tests, again in the form of partial correlations. As one can see, we finally succeeded in breaking the temperature effect in 3 of the 12 tests on murder, in 4 of the 12 tests on rape, and in 4 of the 12 tests on assault. Even here, though, all of the partial correlations were in the theoretically expected direction, and several of the successful "breaks" barely missed the .05 significance level.

*Destructive testing of the heat effect: Summary.* We confess to being surprised at how strong the heat effect was in the face of these various destructive tests. Part of our surprise arises from our knowledge of the high correlations between the measures of temperature and those of SCVT. Such high correlations among competing predictors mean that much of the zero-order correlation between the predictor of interest (temperature) and the criterion (violent crime) is likely to be shared (Area A of Figure 2) and thus gets removed in the partialing process.

Of course, it is possible that different combinations of competitors might successfully break the heat effect at some earlier point in the destructive testing process. The possible combinations of 14 competitors taken 2 at a time, 3 at a time, and so on, is quite large. Adding interactions as potential competitors would further exacerbate the problem. We thought that a further search for ways of breaking the heat effect would inappropriately capitalize on chance patterns in the data rather than inform us of additional "truths" about the robustness of the heat effect. The bottom line, from our perspective, is that the heat effect required us to go to considerable lengths to break it. It even withstood the theoretically important competitor variable of southernness. Thus, these results strongly contradict Model A in Figure 3, which states that the relatively high violent crime rate in the Old South is solely due to SCVT.

We next turned our attention to a fuller examination of the linear SCVI effect. Because it has a newly developed theoretical and empirical basis (e.g., Nisbett, 1993), we thought an empirical examination of it from the destructive testing approach would yield interesting insights.

*Destructive testing of the southernness effect.* SCVT predicts relatively higher levels of aggression in southern cities. The first step in testing this is to see whether such an effect actually exists in the present data. As noted earlier and shown in Figure 4, both the linear and the quadratic effects of SCVI on vcrime were significant. The partial correlation relating linear SCVI to vcrime was .56, after the quadratic component was partialled out. This linear component supports SCVT, but the slight bend at the high end of SCVI contradicts it.

We performed the same destructive testing procedure as just reported for the heat effect on the linear SCVI effect. Specifically, we first added the theoretically most interesting competitor, our hotscale index of heat measures. Next we added the 10 nonrace social variables. Finally, we conducted destructive tests with all competitors in the model.<sup>3</sup>

<sup>3</sup> Note that many theorists in the SCVT tradition have focused primarily on murder. Rape is not seen as relevant to the defense of one's honor, which presumably underlies the SCVT as conceptualized by some (e.g., Nisbett). Assault also is sometimes seen as less relevant than murder, but defending one's honor would seem to include assault as well

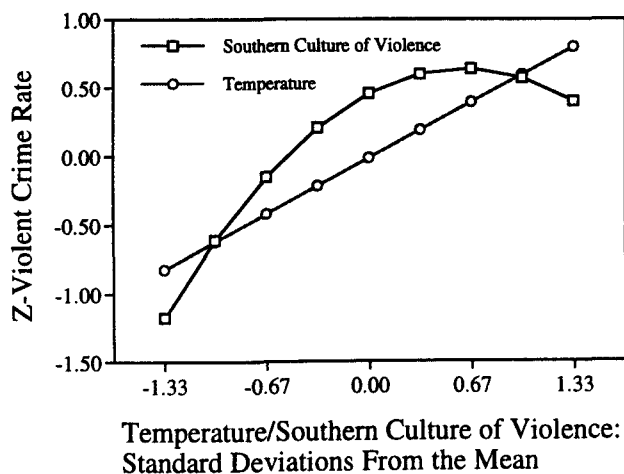


Figure 4. The relations among southern culture of violence, temperature, and standardized violent crime rate.

Table 1  
*Correlations Between Temperature (HotScale) and Rates of Violent Crime, Murder, Rape, and Assault: Destructive Testing by Degrading the Dependent Variable (Vcrime) and Partialing Out Competitors*

Type of crime	Variables partialled from the temperature-violent crime effect			
	None	SCVI and SCVI <sup>2</sup>	SCVI, SCVI <sup>2</sup> , and 10 social variables	SCVI, SCVI <sup>2</sup> , and 12 social variables
Vcrime (n = 253)	.61***	.37***	.28***	.21**
Murder (n = 253)	.61***	.32***	.27***	.19**
Rape (n = 260)	.48***	.29***	.20**	.13*
Assault (n = 260)	.48***	.28***	.20**	.16*

*Note.* The 10 social variables are unemployment rate, per capita income, poverty rate, mobility (percentage living in a different home in 1975), high school education (percentage of the  $\geq 25$ -year-old population that had graduated), college education (percentage of the  $\geq 25$ -year-old population that had attended 4 or more years), population size, percentage of population less than 18 years old, percentage of population 18-64 years old, and median age. Two race variables were added to the 12-social-variable model: Percentage Black and percentage Spanish (census bureau designations). Vcrime = violent crime index; SCVI = southern culture of violence index.

\*  $p \leq .05$ . \*\*  $p < .01$ . \*\*\*  $p < .001$ .

Table 3 presents the results of these various tests of the SCVI effect. As can be seen, the SCVI effect was broken relatively soon in the destructive testing process. Indeed, because it was broken so soon, there was no need to further stress it by degrading the SCVI into its component parts.

For the composite measure vcrime, as well as for rape and assault, partialing out hotScale alone broke the SCVI effect. The SCVI effect on murder was stronger than on vcrime, in support of some SCVT positions. However, the SCVI effect on murder rate broke down when all competitors were added to the model.

*Destructive testing of the SCVI effect: Summary.* The destructive testing results on the linear SCVI effect provide only weak support for the hypothesis that there exists an SCVT that independently contributes to aggression. The support is most convincing for murder, for which the SCVI effect survived until the race variables were added to the model. It also is clear that the SCVT hypothesis did not withstand the destructive testing nearly as well as did the temperature hypothesis. Indeed, for the vcrime composite and for rape and assault, the linear SCVI effect broke as soon as the temperature variable was added. However, just as the destructive testing of the heat effect produced results inconsistent with Model A of Figure 3, the SCVI effect on murder suggests that Model B (which gives no role to southernness) is somewhat implausible.

*Additional statistical concerns.* Statisticians have been concerned for some time with the nonrobust characteristics of ordinary least squares procedures (e.g., Tukey, 1977; Wilcox, 1994a, 1994b). Violations of various assumptions can produce major shifts in parameter estimates and in Type I and Type II

error rates. In our view the most important concern with the present archival data set (and with similar types of data) is the possibility that a few extreme scores on one or more variables may substantially distort the picture that emerges from standard regression procedures. What is needed is a procedure that is more robust against (i.e., less influenced by) a few extreme scores or outliers. Although such procedures are beginning to emerge from the statistics literature, they have not yet found their way into standard statistical packages (e.g., SAS), and (as far as we can tell) have not been fully developed for the kind of complex multivariate case at hand.

Using Tukey's box plot procedure (Tukey, 1977), we found that many of our variables had at least some outliers. In an attempt to address the robustness problem, we first Winsorized the data for all the main variables at 10% (see, e.g., Wilcox, 1994a, 1994b). This involves setting the highest 10% of scores on a given variable equal to the 90th percentile score and setting the lowest 10% of scores equal to the 10th percentile score. This procedure eliminated outliers for all but two variables—population and percentage Spanish population. In both cases there were many outliers still remaining (32 and 42, respectively), so we decided to drop both from the robust analyses. We then recomputed the main regression analyses on this "robust" data set. The relations between temperature (hotScale) and all of the violent crime measures were practically identical to those displayed in Table 1. In some cases there was a slight increase; in others a slight decrease. All were significant at the .05 level.

A similar reanalysis of the linear SCVI effect on violent crime yielded two modest changes from our original analysis in Table 3. First, the linear SCVI effect on vcrime became nonsignificant when hotScale was in the model. Second, the linear SCVI effect on murder rate became nonsignificant when social variables were in the model. Thus, though we urge caution regarding the specific magnitudes of our basic regression results, we are confident that the main results of our destructive testing analyses are not due to a few extreme data points. The Appendix contains both the temperature effects and the linear SCVI effects from these robust analyses.

as murder. On the other hand, one could argue that even rape is often an expression of anger or hostility directed at women as a consequence of perceived threats to honor or self-esteem. Our intent is not to present or defend a particular position on the most frequent motivations underlying murder, rape, and assault. Rather, we merely point out that: (a) there are reasons to expect SCVI to relate less strongly to rape and assault than to murder and (b) the reasons are themselves debatable.

Table 2  
*Correlations Between Temperature and Rates of Murder, Rape, and Assault: Destructive Testing by Degrading the Dependent Variable (Vcrime), the Independent Variable (Hot/cold), and Partialing Out Competitors*

Temperature measure	Variables partialled from the temperature-violent crime effect			
	None	SCVI and SCVI <sup>2</sup>	SCVI, SCVI <sup>2</sup> , and 10 social variables	SCVI, SCVI <sup>2</sup> , and 12 social variables
Murder				
Cooling <sup>a</sup>	.56***	.27***	.27***	.20**
Hot	.53***	.22***	.23***	.17**
Heating	-.59***	-.30***	-.17**	-.10
Cold	-.55***	-.24***	-.12	-.06
Rape				
Cooling	.37***	.17**	.16*	.09
Hot	.36***	.15*	.11	.04
Heating	-.52***	-.34***	-.18**	-.14*
Cold	-.48***	-.28***	-.14*	-.10
Assault				
Cooling	.42***	.20***	.15*	.10
Hot	.37***	.12*	.08	.04
Heating	-.52***	-.33***	-.22***	-.20**
Cold	-.46***	-.24***	-.15*	-.12

Note. Vcrime = violent crime index; SCVI = southern culture of violence index.

<sup>a</sup> Cooling = cooling degree days; Hot = number of hot days; Heating = heating degree days; Cold = number of cold days.

\*  $p \leq .05$ . \*\*  $p < .01$ . \*\*\*  $p < .001$ .

### Study 2: White Violent Crime Arrests in 1980

It has been proposed (e.g., Nisbett, 1993) that the SCVT may be particularly relevant to the White population. We conducted Study 2 to test this notion. We attempted to collect similar violent crime rates for 1980 (as in Study 1) for White perpetrators. We were unable to get such data but were able to get the number of White arrests in 1980 for murder, rape, and assault for most of the 260 cities from a computer data tape of the *Uniform Crime Reports* (U.S. Department of Justice, 1981b). We calculated White violent crime arrest rates by dividing the number of White arrests for each crime for each city by the White population of each city, obtained from the U.S. Bureau of the Census (U.S. Department of Commerce, 1983a). We regressed the data for the 12 social variables, temperature, and SCVI used in Study 1 onto the White arrest rates using the destructive testing approach.

Note that although White arrest rates may be better conceptual representations of SCVT-relevant violence, they are poorer measures than violent crime rates in several ways. First, arrests may occur in a different year than the crime. Second, more than one arrest, or no arrests, may be made for each crime. Finally, the frequency of arrests is lower than the frequency of crimes, resulting in less stable estimates of violence rates, much as a test with fewer items is generally less reliable than one with more. All of these problems should add error variance that would make it more difficult to detect true SCVI and temperature effects. In essence, use of White arrest rates rather than overall crime rates can be regarded as a destructive testing procedure.

We therefore expected the heat and SCVI effects to break more easily in Study 2.

### Results and Discussion

*Destructive testing of White violent crime and hot/cold.* The zero-order correlations between temperature and the White arrest rates for the violent crimes supported the temperature hypothesis (see Table 4). The inclusion of linear and quadratic SCVI terms as competitors did not break the temperature effect. Furthermore, the heat effect survived the addition of the 10 nonrace social variables to the model. In fact, the only clean break occurred when all 14 competitors were in the model testing the heat effect on rape. Thus, the temperature hypothesis received considerable additional support. As in Study 1, adding additional stresses (i.e., degrading the temperature variable into its component parts) provided further breaks in the heat effect but produced no new insights, so detailed results are not presented.

*Destructive testing of the SCVI effect on White violent crime.* We subjected the White arrest data to destructive tests involving the same sets of competitors used in the tests of the linear SCVI effect in Study 1. Table 5 presents the results of these tests. The partial correlations between linear SCVI and the violent crime arrest rates were substantially smaller than those in Study 1. The partial correlation for rape was not even significantly greater than 0. Merely degrading the dependent variable was sufficient to break the SCVI effect. Furthermore, when the temperature competitor was in the model, the SCVI effect reversed

Table 3  
*Correlations Between Southernness (SCVI) and Rates of Violent Crime, Murder, Rape, and Assault: Destructive Testing by Degrading Variable (Vcrime) and Partialing Out Competitors*

Type of crime	Variables partialled from the SCVI-violent crime effect			
	SCVI <sup>2</sup>	SCVI <sup>2</sup> and hotscale	SCVI <sup>2</sup> , hotscale, and 10 social variables	SCVI <sup>2</sup> , hotscale, and 12 social variables
Vcrime ( <i>n</i> = 253)	.56***	.13*	.12	-.01
Murder ( <i>n</i> = 253)	.57***	.18**	.15*	.07
Rape ( <i>n</i> = 260)	.44***	.07	.07	-.06
Assault ( <i>n</i> = 260)	.44***	.07	.07	-.01

Note. SCVI = southern culture of violence index; Vcrime = violent crime index.  
 \*  $p \leq .05$ . \*\*  $p < .01$ . \*\*\*  $p < .001$ .

direction in all cases but one and was significantly negative in nine cases. In other words, in contradiction to SCVT and the claim that it is particularly relevant for the White population, when temperature was controlled (i.e., in the model) the more southern cities had lower White violent crime arrest rates than the less southern cities. We do not want to overinterpret this finding, but it clearly represents a major breakdown of SCVT in our destructive testing scheme.

We also applied the Winsorization and outlier elimination techniques used in Study 1 to both the heat effect and the linear SCVI effect on White arrest rates. The basic findings reported in Tables 4 and 5 were replicated in these robust analyses, so they are not further presented here.

### Summary and Conclusions

#### *The Present Findings in Context*

The utility of using geographic region studies of the heat hypothesis depends on a straightforward but multilayered set of translations and assumptions. Such translations and assumptions are necessary to create testable empirical realizations of any hypothesis, but they also partially insulate the hypothesis from disconfirmation. When multiple studies using multiple methods test the same conceptual hypothesis and produce the same empirical answer, our confidence in the validity of the various assumptions and translations increases, as does our confidence in the conceptual hypothesis. Geographic region studies of the heat hypothesis

are useful because they use unique assumptions and translations and thus help in the triangulation process. They are not without weaknesses, however, so care must be taken to identify and factor them into judgments about the success or failure of the heat hypothesis.

Examining crime rates in U.S. cities from the heat hypothesis perspective entails several risks. The theory is necessarily causal, whereas the data are correlational. The theory at its most abstract level is concerned with aggressive motivation in individuals, whereas the data are aggregated crime rates. Temperature differences among cities are known to correlate with many competitor variables that may play a causal role in aggression. In spite of these problems, this approach to the study of the heat hypothesis presents an interesting opportunity for falsification.

In the present analyses we adopted a destructive testing approach. Initial analyses of the heat effect showed it to be strong. Destructive tests involving an index of southern culture based on SCVT (both linear and quadratic terms) and 12 additional competitors produced evidence of a surprisingly strong heat effect for the violent crime rates in Study 1. The heat effect was also quite strong when aggression was indexed by White arrest rates in Study 2. The fact that SCVI did not break the heat effect in either study adds further support to the heat hypothesis.

Finally, the present studies yielded mixed results for the notions that a southern culture of violence exists and contributes to differences in violent crime rates among cities. The strong version of SCVT—one endorsed by Nisbett and colleagues (e.g., D. Cohen

Table 4  
*Correlations Between Temperature (Hotscale) and White Arrest Rates for Violent Crime, Murder, Rape, and Assault: Destructive Testing by Degrading the Dependent Variable (Wvcrime) and Partialing Out Competitors*

Type of crime	Variables partialled from the temperature-violent crime effect			
	None	SCVI and SCVI <sup>2</sup>	SCVI, SCVI <sup>2</sup> , and 10 social variables	SCVI, SCVI <sup>2</sup> , and 12 social variables
Wvcrime ( <i>n</i> = 249)	.44***	.42***	.23***	.19**
Wmurder ( <i>n</i> = 249)	.49***	.35***	.16*	.13*
Wrape ( <i>n</i> = 256)	.26***	.31***	.14*	.11
Wassault ( <i>n</i> = 257)	.32***	.33***	.22***	.18**

Note. W = White; vcrime = violent crime index; SCVI = southern culture of violence index.  
 \*  $p \leq .05$ . \*\*  $p < .01$ . \*\*\*  $p < .001$ .

Table 5  
*Correlations Between Southernness (SCVI) and White Arrest Rates for Violent Crime, Murder, Rape, and Assault: Destructive Testing by Degrading the Dependent Variable (Wvcrime) and Partialing Out Competitors*

Type of crime	Variables partialled from the SCVI-violent crime effect			
	SCVI <sup>2</sup>	SCVI <sup>2</sup> and hotscale	SCVI <sup>2</sup> , hotscale, and 10 social variables	SCVI <sup>2</sup> , hotscale, and 12 social variables
Wvcrime ( <i>n</i> = 249)	.25***	-.19**	-.28***	-.17**
Wmurder ( <i>n</i> = 249)	.39***	-.03	-.11	.07
Wrape ( <i>n</i> = 256)	.09	-.19**	-.26***	-.20**
Wassault ( <i>n</i> = 257)	.16*	-.18**	-.26**	-.20**

Note. W = White; vcrime = violent crime index; SCVI = southern culture of violence index.

\*  $p \leq .05$ . \*\*  $p < .01$ . \*\*\*  $p < .001$ .

& Nisbett, 1994; Nisbett, 1993)—is that the “culture of honor” (their version of SCVT) accounts for regional differences in violent crime rates and that temperature does not play a role—a version of Model A in Figure 3. Our data strongly contradict this version of SCVT. Certainly we agree that recent data suggest the possibility of attitudinal differences between the South and other regions and that these attitudinal differences may coincide with notions of honor and use of violence to protect or restore it (D. Cohen & Nisbett, 1994). We further agree that other research, described by Nisbett (1993), suggests some interesting SCVT differences. Furthermore, the fact that the linear SCVI effect survived destructive testing best when the dependent variable was murder supports SCVT to some extent. However, that model also predicted that focusing on violence committed by Whites would strengthen the SCVI effect; in fact, the White arrest rate results were particularly problematic for SCVT. Finally, we think it is important to note that many of the data claimed to “explain” the relatively high violent crime rates in the South in terms of SCVT are only tangentially relevant. As yet there are no data showing that populations of cities with higher violent crime rates are more supportive of SCVT-relevant attitudes or values than populations of less violent cities, once one controls for temperature. In short, it is hard to see how the overall data, contained in our studies as well as in others’ works, can be interpreted as supportive of an exclusive SCVT interpretation of regional differences in violent crime.

At this point, the accumulated empirical evidence suggests that either Model C or Model E in Figure 3 may be correct, at least concerning the most violent of crimes—murder. That is, we believe that both heat and culture contribute to high Southern murder rates, though support for SCVT is weak at best. However, it also is possible that a violent southern culture emerged solely (or largely) as a function of hot temperatures. It is even possible that a culture of honor developed to justify aggressive behaviors that were (a) partially caused by discomfort due to heat and (b) were committed by the powerful leading citizens of the Old South. Indeed, recent cross-cultural work by Pennebaker, Rimé, and Blankenship (1996) suggests that emotionality in general may be increased by hot climates.

Our particular methods, which are quite different from those used by Nisbett and colleagues, may partially explain the weakness of support for an independent SCVI effect in the present studies. Further explorations of potential culture effects are certainly warranted, but the most productive approach for that

work is likely to be very different than our present one. We find the Nisbett et al. (1995) approach especially promising.

### *Destructive Testing and the Nomological Network*

We had no doubt that the heat effect in our geographic region studies would eventually break. A basic assumption of destructive testing is that the item to be tested (in this case both the heat and the SCVI effects) can be broken if enough stress is placed on it. Additional destructive testing may prove interesting, but we caution others (and remind ourselves) to pay careful attention to the theoretical status and empirical distinctiveness of additional competitors.

We also emphasize the importance of putting any particular finding in the context of the broader nomological network. The heat hypothesis currently summarizes a vast number of findings from quite varied methods. Within the geographic region type of studies alone there is evidence from several different countries that hotter regions have more aggressive behaviors (Anderson, 1989). Other types of studies add to the impressive array. Some involve the same region but across different (temperature-related) periods of time (Anderson, 1989; Anderson & Anderson, in press; Anderson & Groom, 1995). Some involve aggression measures assessed at the same time as the temperature (e.g., Kenrick & MacFarlane, 1984). Our own recent laboratory work (Anderson et al., in press; Anderson, Deuser, & DeNeve, 1995) has shown that hot temperatures increase feelings of hostility and endorsement of aggression-related beliefs and attitudes. It seems safe to predict that no single alternative explanation can come close to accounting for this broad array of findings, but the heat hypothesis handles them easily. For instance, SCVT may eventually succeed in claiming some of the geographic region heat effect if SCVT is assessed differently, yet it cannot begin to account for increases in wife battering during summer months (Michael & Zumpe, 1986), or hot day increases in aggressive horn-honking (Kenrick & MacFarlane, 1984), or batters hit by pitched balls (Reifman et al., 1991), or assaults in New York City (Dexter, 1899).

It is possible, of course, that different alternative explanations will emerge for at least some of the correlational studies. At this point in time, however, the search for competitors seems likely to generate more heat than light. For this reason our research team has focused on developing and testing a more complete

theoretical model of aggression that incorporates temperature as merely one factor. We are focusing on experimental laboratory studies because they allow more precise tests of the processes intervening between the experience of uncomfortable temperatures and performance of aggressive behavior. In addition, we link this work to the larger body of research on affective aggression by means of a more general theoretical model (Anderson et al., in press). Instead of the traditional call for "more research" on geographic region effects on aggression, we end with a suggestion that the most fruitful avenues may well lie in other directions. Specifically, what are the routes through which various situational factors such as temperature, pain, and frustration influence a person's tendency to aggress against others? For us, this question leads back from whence we came, the experimental laboratory.

Finally, we reemphasize the utility of a destructive testing approach in general. We believe that there are at least two advantages to adopting this perspective. First, it reduces the tendency to see the empirical world in strictly black and white terms. An effect that is eventually broken by the stress of competitor variables is not suddenly and erroneously dismissed as being nonsignificant. Instead, the destructive testing approach requires that careful thought be given to the conceptual and empirical status of the competitors needed to break the effect, the statistical properties of the measures used, and to the total stress that was required for breakage to occur. In essence, we do not automatically throw out the baby with the bath water. Second, the destructive testing approach increases the likelihood of discovering important theoretical links between variables that initially seemed unrelated. When a competitor variable plays a major role in breaking the target effect, our attention is automatically drawn to that competitor and its relation to the target effect. In the present case, the fact that heat proved to be an important competitor to the SCVI-violent crime effect leads to speculation about how heat may have contributed to the development of a southern culture of violence.

The destructive testing approach is implicitly present in much research. It is revealed every time researchers write that their predicted effects remain "even when the effects of X, Y, and Z were statistically removed." Such statements abound in correlational studies of various types, including the stress literature, the attributional style literature, and essentially every domain in which individual differences may play a role. As this discussion was initially being prepared, the first author was in the process of reading a recent issue of the *Journal of Personality and Social Psychology*. One particularly interesting article reported in its abstract that "those with greater perceptions of control were less depressed, *even when* physical functioning, marital satisfaction, and negative affectivity were controlled for" (Thompson, Sobolew-Shubin, Galbraith, Schwankovsky, & Cruzen, 1993, p. 293, italics added). The control-depression effect is impressive precisely because it survived several destructive testing attempts with competitors that are conceptually and empirically strong ones.

In short, we have found the destructive testing approach enlightening in our studies of the heat hypothesis. The logic of the approach is implicit in much current research. We hope that a more explicit recognition of this approach will also prove beneficial to others.

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(Appendix follows on next page)

## Appendix

Table 1A

Correlations Between Hotscale and Rates of Violent Crime, Murder, Rape, and Assault: Destructive Testing by Degrading the Dependent Variable (Vcrime) and Partialing Out Competitors, Based on 10% Winsorized Data With Population and Percentage Spanish Population Dropped From the Social Variables

Type of crime	Variables partialled from the temperature-violent crime effect			
	None	SCVI and SCVI <sup>2</sup>	SCVI, SCVI <sup>2</sup> , and 9 social variables	SCVI, SCVI <sup>2</sup> , and 10 social variables
Vcrime ( <i>n</i> = 253)	.62***	.40***	.21***	.22**
Murder ( <i>n</i> = 253)	.64***	.32***	.20**	.20**
Rape ( <i>n</i> = 260)	.49***	.34***	.20**	.20**
Assault ( <i>n</i> = 260)	.49***	.30***	.15*	.14*

Note. Vcrime = violent crime index; SCVI = southern culture of violence index.  
\**p* ≤ .05. \*\**p* < .01. \*\*\**p* < .001.

Table 1B

Correlations Between SCVI and Rates of Violent Crime, Murder, Rape, and Assault: Destructive Testing by Degrading the Dependent Variable (Vcrime) and Partialing Out Competitors, Based on 10% Winsorized Data With Population and Percentage Spanish Population Dropped From the Social Variables

Type of crime	Variables partialled from the SCVI-violent crime effect			
	SCVI <sup>2</sup>	SCVI <sup>2</sup> and hotscale	SCVI <sup>2</sup> , hotscale, and 9 social variables	SCVI <sup>2</sup> , hotscale, and 10 social variables
Vcrime ( <i>n</i> = 253)	.57***	.10	.06	-.10
Murder ( <i>n</i> = 253)	.62***	.22***	.12	.01
Rape ( <i>n</i> = 260)	.45***	.02	.03	-.10
Assault ( <i>n</i> = 260)	.43***	.05	.03	-.07

Note. Vcrime = violent crime index; SCVI = southern culture of violence index.  
\*\*\**p* < .001.

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