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Temperature, Aggression, and the Negative Affect Escape Model

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This article addresses Bell's (1992) comments on Anderson's (1989) review of the temperature–aggression literature. At a global level, all agree that geographic region studies and most time period studies do not cleanly address the question of the functional shape relating temperature to aggression. In addition, all agree that the negative affect escape model warrants additional empirical investigation. At a more specific level, however, numerous inaccuracies and misinterpretations are noted and corrected. The conclusions of the original review are confirmed. Suggestions for new research are offered.

A recent review (Anderson, 1989) examined extant studies on the temperature–aggression relation, with the explicit goal of addressing three main issues. The issues concerned whether a nonartifactual relation exists between temperature and aggression, the shape of this relation, and the fit of past work to five potentially relevant theories. Bell's (1992) comment addresses only one small portion of the review: the relation of extant evidence to the negative affect escape (NAE) model. We agree on many points. We also welcome the opportunity to clarify misinterpretations and to specify the characteristics necessary for improved research on NAE.

Three features characterize the comment. First, several "criticisms" are merely restatements of points made repeatedly by the review. Second, the comment responds to a review that was not written. Third, specific defenses of NAE are weak and potentially misleading. This reply addresses these three features, presents the characteristics needed in future research on NAE, and notes the value of prepublication scholarly communications.

Restatements of Points Made in the Review

The review and our current position agree with the following major points of the comment: (a) Because of methodological complexities, the shape issue (and hence the specific-theories issue) has not been resolved, (b) better laboratory and field experiments on the shape and specific-theories issues are needed, and (c) NAE warrants additional work.

Methodological Complexities and the Shape Issue

Quotes throughout the review indicate our agreement that the shape issue has not been resolved. The review notes several possible shape functions and states that "distinguishing among these functions is quite difficult, particularly in field studies" (Anderson, 1989, p. 75). This includes the NAE prediction of a downturn in aggression at high temperatures. A host of reasons for this difficulty were discussed several times in the review and the comment and thus are not repeated here. The review summary of the geographic region results clearly stated that the shape issue "was not resolved by these data. Indeed, all one can say is that these studies provided an opportunity for disconfirmation of the linear, J-shaped, and U-shaped functions and that disconfirmation did not occur. Similarly, the specific theories . . . could not be tested" (Anderson, 1989, p. 80).

Though the review did not go beyond this statement, a broad view of theory testing in science allows more. Theories are tested by examining data that allow falsification. When a theory survives a potential falsification, confidence in the theory grows. Because NAE can be made consistent with several patterns of results in the region studies, it is practically unfalsifiable. Subsequently, NAE cannot gain support from these studies. On the other hand, theories that are more easily falsified by region data can gain strength by successfully passing such tests. Specifically, theories predicting that hotter temperatures continue to increase aggression throughout the normal temperature range could have received disconfirmation and therefore gained support when falsification did not occur.

The time period studies (hotness of years, quarters, months, days) can be viewed in much the same way. The NAE inverted U is hard to distinguish from other shapes when the unit of analysis is anything greater than a day. Comparable reasons for this are listed in both the review and the comment. However, all the time period studies do present the possibility of falsifying several shape functions (e.g., linear, J-shaped) and thus can (and do) lend some confidence to theories predicting those shapes. But the NAE inverted U is not falsifiable by the time periods except for day, and the review discussed the shape issue only for the day studies. Thus, the review agrees with the comment that most time period studies do not contradict NAE. Of course, the flip side of this is also true: Because these studies do not allow falsification of NAE, they cannot support it. The issue of daily time period studies is somewhat different and is discussed shortly.

Better Laboratory and Field Studies Needed

The review and comment agree that thorough testing of theories of the temperature–aggression relation requires better

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methods in both the field and the lab. The main points are discussed throughout the review (e.g., see suggestions in the "Future Work" section, p. 94, Anderson, 1989). We later detail additional necessary features.

NAE Warrants Additional Work

The review notes problems with the laboratory studies and discusses the need for research to further test various theories. The review also notes that NAE receives relatively little support from existing studies. The studies in which NAE had some chance of being falsified produced results that did, on the whole, falsify the theory (e.g., Baron, 1972; Baron & Bell, 1976, Study 2; Bell, 1980; Bell & Baron, 1976, 1977; Palamarek & Rule, 1979). But methodological and theoretical caveats very similar to those in the comment were discussed in the review. The inference to be drawn is that future work should include better tests of NAE, as well as of Zillmann's (1983) excitation transfer model and of Berkowitz's (1984) cognitive neoassociation model.

The Review That Wasn't Written

The comment appears to respond to a review that was never written. First, it assumes that the major focus of the review was on NAE. Thus, every review statement made about the temperature–aggression hypothesis is interpreted as if it directly referred to NAE. The comment apparently missed the distinction drawn early in the review between the issue of whether there exists a nonartifactual relation between temperature and aggression and the issue of what is the shape of that relation. The bulk of the review addressed the former issue. Statements about the "consistency" of the field/archival studies explicitly refer to the "existence" issue, but the comment misinterprets many such statements as if they referred to the shape/theory issues. Such misinterpretation results in the juxtaposition of quotes from very different sections of the review, quotes that have quite different referents. For example, the comment compares quotes from pages 92 and 93 with a quote on page 80. The former quotes point out how the field studies yielded consistent findings, in reference to the existence issue. These are compared with a quote on the inconclusiveness of the field studies concerning shape. Contrary to the comment, there is no contradiction. The field studies are consistent with respect to the existence question and inconclusive with respect to the shape question. The comment's inclusion of the odor–aggression study as support for NAE further reveals the mistaken assumption that the review was about NAE. The review was about the temperature–aggression relation; the odor study is totally irrelevant.

The comment also assumes that our research group has as vested an interest in debunking NAE as others have in defending it. This is not true. Shortly after publication of the review, we began work on a grant proposal in which over one third of the studies were designed to demonstrate conditions under which NAE might be true and which attempted to integrate NAE with Berkowitz's (1984) cognitive-neoassociation model. Some of those studies have recently been completed; we have been doing the computer programming for several others. If we really thought that NAE was without merit, we would not be investing so heavily in it. Indeed, we hope that Bell and others will join us in a resurgence of interest in this type of aggression research.

Specific Defenses of NAE

A careful reading of the comment reveals how weak the evidence for NAE actually is. The defense attempts to show that (a) the field studies are more inconsistent than the review indicated, (b) the field studies are consistent with the inverted-U shaped function predicted by NAE, and (c) the lab studies are more consistently supportive of NAE than indicated in the review.

We next examine all three attempts from a meta-analytic perspective. The philosophy behind any literature review is basically a meta-analytic one, even if the review does not use the specific techniques that now reside under that methodological title. The reviewer must delimit a problem and classify studies as relevant or irrelevant and as being of one type or another on the basis of prior theory and the characteristics of the studies. Then the reviewer must try to summarize what the bulk of the data show. The temperature–aggression review took exactly this approach and combined the data of similar studies whenever it was possible. In this way, one gets a more accurate overall view of the true relation between variables of interest.

Are the Field Studies Inconsistent?

Geographic region. The review and the comment agree that the region studies cannot resolve the shape issue. The review examined region studies primarily from the standpoint of the existence question. Consistency thus requires that reliable temperature-related region effects occur across a variety of types of spontaneous aggression with as many potentially confounding factors controlled as possible. The review reports an amazingly consistent set of findings across cultures, periods of history, and types of spontaneous aggression. The only exceptions (e.g., Schwartz, 1968) had methodological problems that made them irrelevant. The Schwartz (1968) study discussed in the comment is an excellent example. One reason for including it in the review was to show how seemingly relevant studies can in fact be irrelevant. The review noted (on p. 79) that one fatal problem with the Schwartz study is that the aggression measure used is of a type that theoretically is not susceptible to temperature effects. Specifically, the aggression measure (acts of political violence) is best classified as instrumental aggression, rather than as spontaneous affect-based aggression. The review also noted that instrumental aggression is not relevant to the temperature–aggression domain. Other fatal flaws were discussed as well.

This point allows clarification of an issue that will resurface concerning laboratory studies. The issue concerns the difference between a priori theoretical distinctions versus post hoc convenient distinctions. Distinguishing among studies on an a priori theoretical basis is an accepted way to do science. The instrumental versus spontaneous aggression distinction is precisely of this type. Post hoc distinctions of convenience, which are based on searches for any differences between those studies

that "work" and those that don't, are unacceptable. This is especially true when there are no theoretical reasons for expecting the discovered distinction to matter.

The comment also claims that two other region studies (deFronzo, 1984; Rotton, 1986) yielded equivocal findings. In fact, deFronzo provides strong support for a temperature-based region effect. Specifically, violent crimes were positively correlated with temperature variables across 142 standard metropolitan statistical areas (SMSAs). The ambiguity referred to by the comment concerns the fact that when a North/South dummy variable was partialed out (along with other sociodemographic factors), the temperature variables were no longer consistently significant. Of course, the North/South variable is highly correlated with temperatures of cities; partialing out its effects should remove most of the variance in aggression accounted for by temperature. This is neither surprising nor inconsistent. Note also that a methodologically stronger version of this same type of study (Anderson, 1987) also showed that SMSAs with more hot days had higher violent crime rates, even after partialing out a number of social variables.

The Rotton (1986) study, which examined homicide rates in 41 countries, also provided some support for the temperature–aggression hypothesis, though the small sample size is a problem. More important, the temperature measures made the study largely irrelevant. As noted in the review, Rotton "gathered 30-year average temperatures in January and July in the capital cities of the countries" (Anderson, 1989, p. 79). The review further noted (p. 79) that what is needed is a climate measure that is more representative of the country than is the capital city and that is taken in the same year as the homicide measures. For these reasons, the review did not dwell on this study.

Time period. Most of the points made in the comment agree with the review. Specifically, most of the time period studies (all those except the daily temperature studies) do not cleanly address the shape issue. The comment incorrectly claims considerable inconsistency, however.

First consider an article listed in the comment as contradictory. Atlas (1984) reports a study of aggression in prisons. Though this article was potentially enlightening, its data characteristics, data analyses, and reporting made it difficult to interpret. Some of the prisons had air conditioning in some places. The frequency of aggressive behaviors varied greatly from institution to institution. The number of hot, moderate, and cool days at each institution is not known. The article was excluded from the review for these reasons, not because it contradicts other time period results. In fact, the most straightforward reanalysis reveals additional support. We computed the average number of assaults per day by month on the total number of assaults (from Atlas, 1984, Table 1) and performed an analysis of variance by quarter of year (with three replications per quarter). The quarters differed reliably in assaults, $F(3, 8) = 9.35$, $p < .01$. The hottest quarter had the largest mean, replicating the pattern that appears repeatedly in the review.

A second claim of inconsistency in time period studies revolves around a pair of reanalyses by Bell and Fusco (1986, 1989) that discovered higher variance in aggression scores at hotter temperatures in Cotton's (1986) study of violent crime and Harries and Stadler's (1988) study of assault. Bell and Fusco (1986) reported several findings from their reanalysis of Cotton's second study. A quadratic trend was significant; the direction was opposite to the NAE prediction. A quartic term also was significant. It too indicated that "at higher temperatures violent crime is still increasing" (Bell & Fusco, 1986, p. 805). The fact that the variance also increased at higher temperatures is interesting, suggesting that some data transformation may be necessary for statistical reasons. But the increased variance is irrelevant to the main issue of whether the hottest temperatures produce increases or decreases in aggression. The Cotton data still contradict NAE and are consistent with other studies of this type. Bell and Fusco (1989) made similar arguments about the Harries and Stadler (1988) assault data. But the regression analyses found significant linear effects and no significant quadratic effects. Aggression did not decrease at the hottest temperatures.

In summary, the field studies are amazingly consistent with each other in showing that temperature is related to aggression. Furthermore, the daily time period studies consistently show an increase in aggression as daily temperatures increase and fail to show the NAE-predicted downturn in aggression at high temperatures. The review's summaries on these points still stand.

Are the Field Studies Consistent With NAE?

As already noted, all agree that the region studies and the extended time period studies do not provide adequate tests of the shape issue. However, the review maintains that at least some of the daily time period studies do allow falsification of the NAE-based inverted U as well as shapes predicted by other models. Studies in which escape from an aversive situation is impossible are not conclusive, but most of the daily studies do not suffer from this, as noted in the review, "because in most cases of assault, murder, and rape, the perpetrator can remove himself or herself from the situation" (Anderson, 1989, p. 89). Across the range of time period studies, with several dependent variables (e.g., assaults, riots, family disturbances, rapes), aggression consistently was found to increase with increasingly hot temperatures. The inverted U was never found. Bell's (1992) comment implies that these data are consistent with NAE, but it is hard to see how this could be so.

Another potential problem with the daily studies is a lack of sufficiently high temperatures. To a great extent this depends on where NAE predicts aggression to begin its decrease as temperatures further increase. All agree that at extremely high temperatures aggression must decline as heat stroke and other physiological problems (e.g., death) preclude such behavior. Such an effect is uninteresting. All agree that the interesting aspect of NAE is the prediction of a downturn in aggression well within normal temperature ranges. Estimates from various reports by NAE proponents suggest somewhere in the mid-80s (°F). Although two of the daily time period studies (Cotton, 1986, Study 1; Rotton & Frey, 1985) may have had too few extremely hot days to permit a fair test of NAE, the remaining studies did not. For example, Houston, the focus of Anderson and Anderson (1984), experiences very many hot days. It is also the case that temperatures in urban centers can be 10–20 °F hotter than in surrounding areas (Bell, Fisher, Baum, & Greene, 1990) and

that many weather stations are in cooler areas (airports). Reported temperature distributions therefore frequently underestimate actual temperatures.

In summary, those field studies that did not allow falsification of NAE can be interpreted as consistent with NAE, but because of the lack of falsifiability, they do not add support. Those studies that did allow falsification consistently contradicted NAE.

Are the Lab Studies Consistently Supportive of NAE?

The final defense of NAE is the claim that lab studies produce consistent support for it, in contradiction to the review. This is simply not true. The most common pattern of supportive findings is a Temperature × Anger interaction, in which hot temperatures produce increases in aggression under nonangry conditions and decreases under angry conditions (e.g., Baron & Bell, 1975). However, this interaction sometimes fails to occur (Baron, 1972; Bell & Baron, 1977) and sometimes occurs in exactly the opposite fashion (Bell, 1980). Sometimes the predicted behavior occurs, but concomitant measures or manipulations of variables predicted by NAE to be mediating behavioral choices (e.g., escape motives) yield results that contradict NAE (Baron & Bell, 1976, Study 2; Bell & Baron, 1976; Palamarek & Rule, 1979). The specifics of these contradictions are detailed on pages 89–92 of the review.

The comment then compares the field study finding that temperature influences violent crime but not nonviolent crime with the claim that lab findings work when shock is used but not when verbal retaliation is the measure of aggression. The point seems to be if one kind of discrepancy is allowed, then why not the other? The reason is simple: The violent/nonviolent crime distinction is an a priori theoretical one. Violent crime is generally spontaneous, affect driven, and theoretically responsive to temperature variations, whereas nonviolent crimes have a large instrumental component to them. Differences in responsiveness to temperature variations are theoretically expected. On the other hand, the shock/verbal assault distinction is a post hoc convenient one. Theoretically, both are spontaneous, affect-driven behaviors with no instrumental component. Both should respond to temperature manipulations in the same way. Indeed, a recent meta-analysis (Carlson, Marcus-Newhall, & Miller, 1989) demonstrated considerable convergent validity between these types of measures. This appeal to different methods as an explanation of inconsistency falls short on other grounds as well. Of the 11 laboratory studies detailed in the review, only 1 (Bell, 1980) used verbal measures of aggression. The inconsistencies among the remaining shock studies (and the 1 white noise study) detailed earlier justify the review conclusion that the lab studies are inconsistent.

Critical Features to Test NAE

Although there are many negative aspects to such disagreements in academia, there are positive ones as well. We hope that better research and eventually a better understanding of spontaneous affect-based aggression will emerge. Toward that end, we offer a listing of features we see as necessary to improve lab testing of NAE in the temperature context.

There are four basic assumptions to NAE. Aggressive motivation is assumed to increase with increases in negative affect. Escape motivation is assumed to increase with negative affect. At low levels of negative affect, aggression is the dominant behavioral tendency. The slope relating negative affect to escape motivation is steeper than the corresponding slope for aggression. From these assumptions, we can derive the following minimal features. Temperature must be varied to produce a range of negative affect, from low to high (e.g., 75 °F to 95 °F). Subjects must be given an opportunity to aggress against a target. Aggression and escape motives must be pitted against each other; that is, subjects must believe that high levels of aggression preclude escape and that escape behavior precludes aggression. (Interestingly, none of the laboratory studies to date included procedures to induce or assess such beliefs.) The demand characteristics inherent in these procedures require one additional feature: Subject suspicion must be assessed. In addition to these minimal features, research should also assess proposed mediating variables, such as level of negative affect, desire for escape, and desire for aggression.

Importance of Scholarly Communication

Clearly, Bell (1992) did not read the review in the way it was intended to be read. The comment suggests a belief that the review was primarily directed at NAE; that aspect was intended to be a minor part. The comment appears to confuse the existence issue and the shape/theories issues; this is why many of the suggested disagreements are actually agreements. We hope that few readers interpret the review in the same way as Bell has, and to date we have received no other similar feedback. However, with hindsight it is easy to see how such misinterpretations could arise, particularly among those with a special interest in NAE. The point is to call for increased communication among scholars before publication. This has long been a practice in Anderson's lab. In the present case, an early draft of the review article was sent to Bell and 13 others in the field, with a request for comments and suggestions. Responses were received from Rotton, Berkowitz, Kenrick, and Michael. Their comments were incorporated into the final version, resulting in a much improved article. Bell's prepublication comments on a more recent paper from our lab have proven helpful; we have no doubt that timely comments on the review draft would have resulted in a review article less susceptible to misinterpretation.

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