

## Politicized Social Science and Human Nature's Disagreeable Aspects: An Example from the Anti-Catharsis Aggression Research

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### Abstract

The background and pitfalls of the original catharsis hypothesis with regard to human aggressive behavior are first presented, which is followed by an account of the research on the reformulated "cathartic effect" by V. J. Konečni and the development of his Anger-Aggression Bidirectional-Causation theoretical model (AABC). After analyzing the key findings and applications of this model, the article comments on the anti-catharsis studies by B. J. Bushman, which were carried out twenty five years later and published from the standpoint of neglect, or lack of awareness, of prior work. Such eyebrow-raising attitude to scholarship is discussed in terms of its broad socio-cultural and political backdrop – a *Zeitgeist* of antagonism to research that does not support a blanket "aggression breeds aggression" view and is too preoccupied with politicized quasi-sociological preferences to bother with the subtle findings and provisos of the AABC model.

**Keywords:** Social science; Social psychology; Sociology; Aggression; Catharsis; Cathartic effect; Konečni's AABC model; Bushman's anti-catharsis articles; Social science practice

### Introduction: Catharsis

The notion of *catharsis* as a kind of emotional "purgation" can be traced to Aristotle's ideas about tragedy in his *Poetics* [1], and to its substantial 20th-century transformations by way of psychoanalytic theory [2], ethology [3], and the frustration-aggression propositions [4] into the "hydraulic" [5] or "boiling pot" theoretical models of human aggressive behavior. A considerable amount of laboratory research with adults and children, as well as field studies, was carried out in the 1950s and 1960s. Many of these experiments suffered from inadequate conceptualization and methodology. For example, authors tended to regard, without a solid empirical grounding, the infliction of physical injury, play aggression, observation of aggressive acts, and fantasy aggression as functionally equivalent for a "cathartic release." In addition, inadequate control conditions were used, especially with regard to the emotional state of the participant prior to aggressing, specifically the degree of anger (if any) due to a provocation (if any).

In 1973, Bandura [6] proposed a "moratorium" on the catharsis hypothesis. He did this in part on the basis of his own "Bobo doll" social-learning studies, although they themselves suffered from conceptual and methodological shortcomings. While Bandura justifiably criticized the view of endogenously generated "negative energy" that builds up (even without provocations) and must be released, he himself relied on experimentally administered frustration in order to obtain the findings of allegedly "mere" imitative aggression in children. The idea of the spontaneously accumulating negative energy, coupled with the alleged benefits of "venting," has found a place, in the past forty or so years, in pop/folk psychology and a variety of unsophisticated self-help manuals.

### The AABC Project on the "cathartic effect"

In my doctoral dissertation in 1973 [7], and a number of related articles [8-14], I identified the essential components of what I called the "cathartic effect" and outlined the three-stage experimental paradigm that was required to test it: 1. the provocation stage; 2. the expression-of-aggression ("revenge") stage; and finally, 3. the measurement of additional (or "residual") aggression stage. Exceptional care was devoted in this research program to control conditions at each stage,

many of which were themselves of theoretical interest. As one example, I investigated the effects of the nature of interpolated activity in the second stage (for example, the nonaggressive alternatives to retaliation) on the amount of subsequent aggression [10]. There were numerous findings of substantive interest, which could all be parsimoniously accommodated by my Anger-Aggression Bidirectional-Causation theoretical model [15]. The model was supported by a variety of additional studies and there were some interesting extensions – to vicarious aggression, substitute targets, alcohol intake, music choice, and violence in the theater, among others – the results of most of which could not have been predicted by any other theory [16-21].

For the purpose of this article, two related findings in the AABC program are the most pertinent. One is that when a person is provoked by another (for example, unjustifiably verbally insulted), the opportunity to injure the insulting person physically, for example, by (fictitious) electric shocks or blasts of noise, significantly *decreases* the amount of *immediate subsequent aggression* against the initial annoyer – Plato expressed an analogous thought about this short-term consequence [22]. In comparison to other interpolated activities, aggression against the insulting person is the most effective means of decreasing both residual aggression and anger [10], and this effect almost certainly depends on physiological arousal being reduced from the aversively high level that had originally been caused by the provocation [16]. However, quite obviously, not a single research contributor to the AABC program has advocated expression of aggression in the form of physical revenge as an individually or socially good thing. Instead, alternatives must be found – and soothing music is perhaps one, even if not as effective as aggressive actions or always readily available; I have

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demonstrated this in a 1979 experiment by means of a complex, 2 × 7, factorial design [23; experiment II.B., pp. 159-167]. But a scientific discovery of even a disagreeable fact about human behavior or nature – made by a sound methodology and in good faith, and published in first-tier journals – does not entitle another scientist to pretend, ostrich-like or capriciously, that the demonstrated fact does not exist.

The second key finding in the AABC project, which I discussed already in 1975 [10], is equally important, because it concerns *preemptive* aggression. Precisely because an annoyed person's retaliatory act often leads to a fast reduction of arousal and anger, this may have particularly deleterious long-term consequences. Especially in long-term dyadic relationships, even as the real or imagined provocations by the original culprit diminish, the amount of retaliation may increase. And even when the instigation disappears altogether, the aggression – no longer vengeful but preemptive – may continue unabated or increase [10,23].

### The anti-catharsis campaign and the neglect of the AABC project

Such conclusions, from what is objectively one of the most painstaking projects on the catharsis hypothesis, bring me to the examination of important issues that would seem to belong in the broad domain of sociology of science. Can the practice of social science on a somewhat controversial topic be free of irrelevant or self-serving pressures and distortions? As we shall see, apparently not. I am referring here to the academic behavior of two much later arrivals on the catharsis or, rather, anti-catharsis scene. In the period 1999-2005, Dr. Brad J. Bushman published, in first-tier journals, at least four articles (three with colleagues) that (negatively) addressed aspects of catharsis; in fact, three of these four articles had "catharsis" in the title [24-27]. And in 2008, Dr. Kevin M. Carlsmith published, with two colleagues, an article on revenge that dealt with issues close to the heart of the cathartic effect and to the work done within the AABC model [28].

It is therefore astonishing that the large body of work that had been done on the cathartic effect, within the AABC model [8,10-14, 19,21, 23], was completely ignored by these social psychologists [in 24-28], with regard to both its empirical and theoretical contribution. Such overt neglect of prior work that is exceptionally close to the topic of one's article is hardly imaginable in the more mature sciences. But social psychology and sociology have had well over a century to mature, so it is of interest to examine various aspects of the situation. One: Bushman was an Associate Editor of the journal *Aggressive Behavior* (an old journal published by the International Society for Aggression) from 1994 to 2004. Two: Most of Bushman's and Carlsmith's coauthors were (and are) accomplished social psychologists, so that one has here a number of mature scientists' common stance that was fully shared with the first authors. Three: One must not forget the journal editors and reviewers in question who thought little of a substantial body of work being ignored.

Other issues are more substantive. Four: In a number of instances, both Bushman and Carlsmith could have made more accurate predictions in their own experiments had they acknowledged and relied on ideas and findings within the AABC model. However, this might have made some of their results appear less "counterintuitive" (arguably the most sought after quality in contemporary cognitive social psychology). Five: In three of the five articles in question, Bushman [26,27] and Carlsmith [28] cited a single article of mine [9, 1974]. This is a three-page article that deals with only a small part of a broader and more detailed theoretical and empirical account,

published in a far more prestigious journal the following year [10, twenty-six pages long]. But citing a comparatively minor article on a narrow issue in effect concealed from view the existence of a large body of relevant work that had resulted in conclusions different from those they wished to reach – work that would have to be confronted and discussed. Six: Not mentioning the AABC procedures and findings served several useful functions. Just three of them are: Bushman's work appeared more novel than it was; he was allowed to attack straw versions of the catharsis hypothesis with impunity; and he did not have to explain to readers (and reviewers and editors) why it was that his experiments required literally ten to fifteen times more participants per condition than was customary in aggression research for a significant result to be obtained. One reason for the small effects was presumably the weakness of Bushman's operational definitions and manipulations; and one consequence of such unwieldy numbers of participants per condition was the occasional absence of the necessary control groups.

### Conclusion: Socio-cultural anti-science

A reasonable conjecture, however, is that there is a far broader political and cultural backdrop to the rejection or neglect of the AABC model [15]. Since the Vietnam war and the violent crime wave in the period 1980-1994, coupled with the spread of "political correctness," the *Zeitgeist* has been one of hostility to complex messages about violence in individual behavior (if not in international relations!). In the context of aggression research, this meant that an acceptable result was only a disconfirmation of catharsis, for which purpose any straw version of the hypothesis did nicely. It was necessary to show, without any subtle provisos, that "aggression breeds aggression." This implied a general anti-catharsis view, including the rejection of the first main AABC finding of a short-term aggression-reducing effect of provoked people's aggression. Unfortunately, the blanket rejection also included the second principal AABC finding – that in the long term, precisely because of the mechanism described in the first finding, interpersonal aggression (especially in dyads) was indeed extremely objectionable, because it was likely to be used preemptively, in a callous manner.

A more detailed treatment of many of the issues raised here is available in an extensive new article [29].

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