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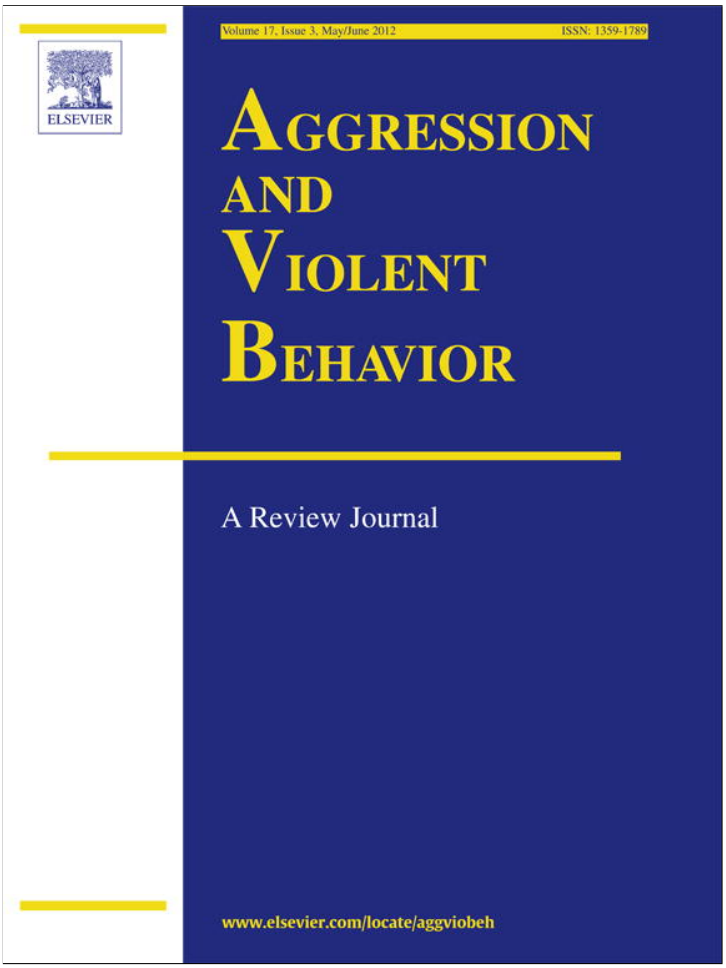

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Aggression and Violent Behavior

Paradigm change in aggression research: The time has come to retire the General Aggression Model

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ABSTRACT

As Kuhn (1970) has noted, most scientific fields go through processes of paradigm change, painful periods in which old theories no longer fit available data and are placed by new theories. Such periods typically create strife and debate as ideological differences emerged between proponents of old and new theoretical approaches. In the current paper, we argue that such a period has been reached within the field of aggression research. Over the past half-century, social cognitive and social learning paradigms of aggression, exemplified in the General Aggression Model (GAM) have retained dominance, particularly in areas such as media violence. We contend that data to support the GAM and social cognitive approaches to aggression have never been conclusive, and newer evidence increasingly suggests that the GAM and social cognitive theories of aggression more generally are not adequate to explain aggressive phenomena. We discuss weaknesses and problematic, sometimes hidden assumptions of the GAM and how these reduce the utility of this paradigm. Current evidence suggests that the GAM and the social cognitive paradigm of aggression should be retired, and approaches which focus on diathesis-stress hold greater promise.

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1. Introduction

Aggression, defined either in terms of the willful attempt to harm another who wishes to avoid that harm (Baron & Richardson, 1994) or perhaps less moralistically as the intent to increase one's own position in a dominance hierarchy at the expense of another (Ferguson & Beaver, 2009) is a central behavior of the human species, present

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across cultures and historical epochs (McCall & Shields, 2008). The word *aggression* has roots in the Latin *ad* "to" and *gradus* which simply is "a step." Aggression is typically framed in negative terms, as an unwanted facet of human behavior (Hawley & Vaughn, 2003; Smith, 2007) so much so that an entirely new word, *assertiveness,* has been created for aggression in which we tacitly approve. Thus, rather than understanding aggression as a continuum from criminal violence to assertiveness, terms such as aggression, assertiveness, and violence have been falsely encapsulated (Ferguson & Beaver, 2009). Given that aggression appears to be both innate to the human condition and so often couched in moralistic negative terms, it is probably not surprising that aggression research has historically been so controversial and fraught with ideology and hyperbole (Gauntlett, 2005; Grimes, Anderson, & Bergen, 2008; Kutner & Olson, 2008). Considerable pressure has been put on scholars to provide society with an understanding of the origins of human aggression and how its excesses might be prevented. The current essay concerns itself with one particular theoretical paradigm for understanding aggression, the social cognitive model as exemplified by the General Aggression Model (GAM; also sometimes referred by the expanded version, General Learning Model). The origins of the GAM and its rise to dominate thinking in media violence research in particular will be detailed, as will the gulf between the ideological and dogmatic rigidity of the model with the available data on aggression.

2. The origins of the GAM

2.1. A brief history of social cognitive theories of aggression

The basic notion that individuals who view aggression, whether in real life or in fictional media might imitate the behaviors they witnessed and thus become more prone to aggression themselves is not a remotely new idea (see Ferguson, 2010a for a history) stretching back at least to the Greeks. This basic premise has been at the root of media based moral panics regarding media as diverse as dime novels, movies, comic books and video games (Ferguson, 2010a; Kutner & Olson, 2008). Similarly this basic imitation model has been applied to the intergenerational transmission of family violence (e.g. Jaffe, Sudermann, & Reitzel, 1992; Wareham, Boots, & Chavez, 2009).

Despite a long history of applying the premise of imitation to aggression, the origins of imitation as the dominant paradigm for aggression in the late 20th century can probably be traced to Bandura's bo-bo doll experiments (e.g., Bandura, 1965) in which children modeled aggressive behaviors toward an inanimate bo-bo doll seen in a video recording. The bo-bo doll experiments have received some considerable critiques, including whether they are aggression studies at all, or whether the child participants simply understood the videos as instructions on what to do next (Ferguson, 2010a; Gauntlett, 2005; Tedeschi & Quigley, 1996) and thus sought to please the experimenters. Nonetheless, despite these concerns, it is probably difficult to overestimate the degree to which the bo-bo doll studies have been reified, particularly within the discipline of psychology, and the notion of modeling and social learning become a foundation for the understanding (or misunderstanding) of aggression.

The basic modeling view of social learning theory was elaborated in social cognitive theories of aggression (Anderson & Huesmann, 2003). Such models often incorporate elements of priming, in which a stimulus activates cognitive networks of related concepts, excitation and arousal and the imitation of specific behaviors (Huesmann & Taylor, 2006). The imitation element is typically worded such as to imply a rigid, passive (that is, irrespective of the viewer's intent) and mechanistic process. For example Huesmann and Taylor (2006) say of modeling of aggression, "human and primate young have an innate tendency to imitate *whomever* they observe" (p. 402, italics added for emphasis) and "As children observe violent behavior, they are prone to imitate it" (p. 402).

Under the social cognitive theoretical paradigm, long-term aggression effects are influenced by the development and activation of cognitive schemata. Social scripts are acquired through the witnessing of violence, and these social scripts are used later to interpret and respond to future events. The social cognitive paradigm argues that aggression is largely a process of cognition, in which cognitive schemas and scripts are learned through observation and applied with automaticity to new environmental antecedents. Social cognitive models also suggest that repeated exposure to violence decreased normal affective aversion to violence through a process of desensitization (Huesmann & Taylor, 2006).

Social cognitive models of aggression portray the cognitive and affective processes of aggression as largely automatic, mechanistic and unconscious. As Huesmann and Taylor state (2006, p. 403) "One of the insidious facts about socialization by the mass media is that much of the socialization process happens without children being aware of what is happening." Social cognitive models are often stated in terms of absolute certitude such as the use of the term "facts" in the above quote, and it is not uncommon to see claims that social cognitive models of aggression are essentially beyond debate (e.g., Bushman & Anderson, 2001a; Huesmann & Taylor, 2006). Although social cognitive models of aggression may tacitly allow for some biological or personological influences, they are seldom detailed at length, and the social cognitive view remains largely a "learning only" view, at least in its elaboration (Sherry, 2004). This likely explains why the American Psychological Association, which has largely adopted the social cognitive model of aggression, continues to say of violence (2011) "Violence is a learned behavior" despite much evidence for genetic, evolutionary and biological influences on violence (Ferguson & Beaver, 2009).

The General Aggression Model (or General Learning Model as it is also called with ramifications beyond aggression) is largely an extension of social cognitive theories of aggression, incorporating the basic cognitive and affective components of script theory and desensitization. The intent of the GAM was to integrate positive elements from other theoretical models such as the basic social learning and social cognitive script approaches, along with excitation transfer (Zillmann, 1983), social interaction theory (Tedeschi & Felson, 1994) and other related approaches. Thus the GAM is intended to be a comprehensive model of aggression, although mainly from within a social learning or social cognitive paradigm.

2.2. Main inputs and outputs of the GAM

The theoretical structure of the GAM has been discussed by its primary creators, Craig Anderson and Brad Bushman, along with colleagues at various times and in various venues (e.g., Bushman & Anderson, 2002; Carnagey & Anderson, 2003; DeWall, Anderson, & Bushman, 2011). As noted by the authors themselves, the GAM, despite an ostensible attempt to be an overarching theory, remains largely a social cognitive script theory (Bushman & Anderson, 2002, p. 1680) "According to this model, aggression is largely based on the activation and application of aggression-related knowledge structures stored in memory (e.g., scripts, schemas)."

It is important to note that the GAM does allow for the input of "personological" variables, particularly prior trait aggression or arousal. However discussions of the GAM seldom elaborate on these personological variables in great detail. When factors such as biology and genetics are mentioned at all, they are simply tied back to social cognition and not elaborated. For instance Anderson and Bushman, in a rare mention of biological inputs state "space limitations preclude detailed discussion of how biological factors operate within the GAM. Briefly, we believe that genetic and other biological factors operate via influences on learning, decision-making, arousal, and affective

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processes" (p. 35). The most recent theoretical discussion of the GAM (DeWall et al., 2011), in which the authors argue for its extension beyond aggression, to real-life violence, warfare (such as the Israeli-Palestinian crisis) global warming effects and suicide, reiterates the argument that the GAM incorporates biology and personality development, without explicitly elucidating upon this claim. Within the GAM, personality itself is construed simply as an accumulation of scripts and schemas (Gilbert & Daffern, 2011), thus the inclusion of personality related variables represents little distinction from script theory. Biology and personality as input variables thus may represent little more than a "fig leaf" to protect the GAM from "tabula rasa" criticisms (see Pinker, 2002) despite that the GAM remains, in effect, largely a tabula rasa theory.

Thus the GAM primarily focuses on external learning-based inputs and the development of cognitive scripts and affective related desensitization as well as arousal. The GAM holds that social knowledge structures develop over time due to exposure and learning. Thus each new experience or exposure to violence, whether in the real world or in media, represents a learning trial (Bushman & Anderson, 2002) which can accumulate over time solidifying aggression related scripts. Repeated exposure to violence (again whether real-life or fictional; relatively little language related to the GAM differentiates the two) solidifies those knowledge structures, potentially creating aggressive personalities. Thus personality development, as noted earlier, is largely a cognitive learning process, rather than one which develops through biological maturation. Short-term exposures to violence, such as in media violence, may prime these aggressive scripts, and they may also create a more general hostile-attribution bias in which individuals perceive ambiguous stimuli as hostile and respond accordingly. Thus, the GAM attempts to explain not only specific aggressive behaviors but the development of aggressive and antisocial personalities and, has been noted earlier, is potentially being extended to real-life violence, warfare, and suicide.

2.3. The use of the GAM

As a psychological model, the use of the GAM has generally been specific to psychology, with some exceptions. Use of the GAM in criminology and criminal justice appears to be minimal. A PsycINFO search for "General Aggression Model" AND "crimin*" (the latter as a journal name search) turned up no hits. A search of the Criminal Justice Periodicals Index on ProQuest for "General Aggression Model" turned up no hits from journals specific to criminology or criminal justice, and thirteen hits from interdisciplinary journals that often draw from psychology.

Within psychology itself, the primary application of the GAM has been within the field of media violence. This is not to say such a limitation is intentional on the part of the creators and proponents of the GAM (again, see DeWall et al., 2011 for arguments in favor of widespread application of the GAM), mainly that proponents of the GAM have generally been investigators heavily interested in media violence. Because the GAM does not distinguish the influence of exposure to real-world violence such as might occur during child abuse with exposure to fictional violence (indeed proponents of the GAM routinely argue that the influence of fictional violence is on par with public health concerns such as smoking and lung cancer and other medical effects, see Bushman & Anderson, 2001a), the GAM is particularly effective for those who wish to argue for the importance of media effects.

Among proponents of the GAM, the degree of absolute certitude with which it is claimed evidence supports the GAM is difficult to exaggerate. As noted, proponents of the GAM routinely compare the effects of expected outcomes as on par with highly important medical effects, both in terms of magnitude of effect and potential public health implications (Bushman & Anderson, 2001a; Huesmann, 2007). As claimed by DeWall et al. (2011, p. 446) "GAM has received consistent support as a general model of aggression." That both these claims, regarding comparison with medical and public health outcomes and regarding the "consistent" nature of support for the GAM, have been discredited by other scholars will be discussed later in this paper. For now it is important simply to note the vehemence and certitude with which proponents of the GAM make their claims as well as its influence, for good or bad, on the psychological science community's understanding of aggression.

3. Assumptions of the GAM

Within a given paradigm, assumptions are presuppositions (whether explicitly stated or not) which are required to be "true" for the theory itself to retain cohesion. Particularly when such assumptions are unstated they may be treated as if true, irrespective of the availability of evidence to support them. The collapse of psychoanalysis as an accepted theory in academic psychology largely rested on the observation that psychoanalytic theory depended upon a set of assumptions that could not be empirically tested, or which had been empirically tested as false. We argue here that the same state of affairs has been reached for the GAM, wherein a number of required assumptions necessary for the cohesion of the GAM can be demonstrated as false or which are difficult to demonstrate empirically.

3.1. Aggression is always bad

As noted earlier, one of the underlying assumptions of the GAM, and perhaps wide swaths of aggression research more broadly is that aggressive behavior is universally harmful and undesirable and maladaptive. This can be seen, for instance, in the recent case in which a California law banning the sale of violent video games to minors was struck down by the U.S. Supreme Court (Brown v EMA). The premise of the California law (largely endorsed by proponents of the GAM including Anderson, Bushman, Gentile, Heusmann and others, who signed an amicus brief supporting the California law) was that video games increased aggression in children (although this premise was debated by an opposing amicus brief of scholars) and thus constituted "harm" to these minors. In other words, increased aggression is harmful. DeWall et al. (2011, p. 245) clearly state this harm view in introducing the GAM by stating:

"In the distant past, aggression often was an adaptive behavior for our ancient ancestors who lived in small groups. Aggression and related threat displays played an important role in mate selection, protection of offspring and other kin, and survival of the group. As humans became more social and developed culture, however, aggression became less adaptive, especially at the group level. Although one can reasonably argue that even today, minor forms of aggression play an adaptive role in socialization and social control (e.g., Tedeschi & Felson, 1994), more serious forms of aggression are more maladaptive than adaptive. Aggression breeds aggression, and it seems to cause more problems than it solves. Even when it works in the short run, aggression frequently fails in the long run."

We submit that this view presents a moralistic, not empirical view of aggression. In fact the authors cite no empirical evidence upon which they base their claims of the maladaptive nature of aggression. From this view aggression, in basically all forms (aside from some undefined "minor" forms of aggression[1]), is maladaptive in the modern world. It is implied that the vision of an idyllic, fully cooperative, non-competing social world forms an ideal. We argue the GAM becomes a pseudoscientific edifice (or perhaps better put as *cargo cult science*, see below for discussion) for this social engineering project rather than a true falsifiable theory. However other scholars have disputed the GAM's view of aggression (e.g., Ferguson & Beaver, 2009; Hawley & Vaughn, 2003; Smith, 2007) suggesting that aggression is better viewed on a continuum from adaptive behaviors to those

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which are maladaptive. This is not a minor issue for the GAM, particularly where the effect sizes seen in research supporting the GAM (even if one takes it at face value and ignores that various methodological criticisms of this body of literature) are very small, ranging on the order from $r = .00$ through $r = .20$. In other words, the phenomenon which the GAM predicts only results in a maximal 4% change in aggression ('aggression' as measured within the studies, see below). Furthermore, samples used in GAM studies are most often college students or normal non-clinically aggressive (i.e., Conduct Disorder, Oppositional Defiant Disorder) youth, in which regression to the mean effects could explain such small shifts. A shift of 4% would typically represent not more than a point on most aggression measures (depending on the scale), whether clinically validated or not. If such shifts are within the range of normative, adaptive aggression, they are not worrisome (and may in fact be beneficial when samples are so often taken from among low-aggression groups). Thus, studies supportive of the GAM, even taken on face value, are unable to document shifts from normative, adaptive aggression to aggression which is maladaptive, given that the aggression measures used in such studies are simply not validated for this purpose. Nonetheless, proponents of the GAM (e.g., DeWall et al., 2011) often generalize the GAM to important real world violent events. In this sense GAM supportive literature seems too often to be overly satisfied with "statistical significance" without a critical examination of effect sizes. The small effect sizes seen in this literature do not warrant its generalization to serious aggression.

Part of the issue is the conceptual *squishiness* with which terms such as aggression and violence are used. Most participants in experimental studies of the GAM, for instance, believe they are delivering stimuli such as non-painful noise bursts to a consenting competitor, not actually harming anyone (as they are themselves exposed to the same noise bursts). We argue that the GAM exists in a nebulous realm between poorly operationally defined aggressiveness and real-life violence, where studies of the GAM focus on the former and are quickly generalized to the latter (as in the Brown v EMA case). This conceptual squishiness provides the GAM with an inappropriate level of conceptual fluidity in which proponents of the GAM can deflect criticisms that the GAM does not adequately predict real-world violence (see, for example Olson, 2004) by shifting to the position the GAM is about aggression rather than violence. Although we accept that the overgeneralization of the GAM is sometimes the product of the news media or the general scientific community we express that it is the responsibility of a theory's proponents to correct improper overuse of a theory. Nonetheless, GAM proponents themselves do this extension of the GAM to real-world violence (e.g., DeWall et al., 2011) without providing adequate empirical support for the utility of the GAM in such scenarios. Thus we have the curious circumstance in which it is well known that the predictions of the GAM are in direct opposition with data on real-life violence such as for youth violence, bullying or adult crimes (see Ferguson, 2010a for a discussion), yet proponents of the GAM attempt to further extend the GAM into new areas of real-world violence. Stimuli which should promote aggression according to the GAM such as media violence, demonstrates, in fact, an inverse societal relationship with real-life violence (Ferguson, 2010a; Olson, 2004). Proponents of the GAM have largely prevaricated on this issue, suggesting on one hand that societal rates of violence do or should respond to increases or decreases in media violence (Barlett & Anderson, 2009; Bushman & Anderson, 2001a; Huesmann & Taylor, 2006). Then, when the inverse relationship between societal violence and media violence is demonstrated, claim there is no expectation that societal and media violence rates should correspond. The GAM seems either to make no clear predictions about real-life violence, or such arguments can be used to shield the GAM from falsification.

It appears that the GAM is used to support a *moral* position on aggression in which aggression is universally bad, rather than a scientific position. As it emerged in the Brown v EMA case, much of the evidence used by the State of California (and in support of the GAM) to support the "harm" view of aggression and violent video games had been funded by moralistic advocacy/lobbying groups such as the National Institute of Media and Family and Center for Successful Parenting (which despite its innocuous name appears wholly devoted to the video game violence issue). This intrusion of moral advocacy into the scientific process is of great concern.

3.2. The human brain does not distinguish reality from fiction

The GAM does not substantially distinguish from exposure to violence which occurs in the real world (child abuse, witnessing domestic violence) and that which occurs in fictional media (action movies, Tom and Jerry Cartoons, First-person-shooters, etc.) This is probably not surprising given both the above assumption that aggression is always bad as well as the GAM's primary usage in media violence research. Furthermore, advocates of the GAM have routinely claimed that the effects of exposure to violence effect all viewers, not just some with a priori susceptibilities (Anderson, quoted in Oxford University Press, 2007). Thus, the GAM posits that exposure to any violence, even fictional violence, has widespread effects with maladaptive consequences, to which the human brain has few defenses. Claims that the human brain responds identically to cartoon violence in *Looney Tunes*, more graphic violence in video games or violence in real life was one source of skepticism among the U.S. Supreme Court in the Brown v EMA case.

There is considerable research to refute this assumption, however. First, considerable research documents that the human brain learns to distinguish between reality and fiction at an early age. Of course, this is not a process that works like a light switch, but progresses through stages of greater sophistication. For instance, even children as young as 3 to 5 begin to use the context of a message (i.e., whether it is told as part of a fictional story or true information) in order to judge the truthfulness of information (Woolley & Van Reet, 2006). Other research has demonstrated that young children can identify obvious fantasy elements of fictional stories in order to evaluate the truthfulness of information presented within (Corriveau, Kim, Schwalen, & Harris, 2009). Still other research has suggested that children may look for and evaluate evidence to support the existence of information (Boerger, Tullos, & Woolley, 2009). At young ages, children may be prone to accept information and testimony from authority figures such as parents as evidence for information, although as they age they demonstrate greater sophistication in evaluating even false information coming from authority figures. This can be seen in the case of Santa Claus. Children's belief in Santa Claus (in the West) is predicated upon the arguments from authority (i.e., parents and society) that Santa Claus does, in fact, exist. Indeed, children are presented with evidence for his existence in the form of Christmas Eve visits with presents, and his availability to listen to wish lists at the local mall. Nonetheless, despite what is in effect a massive if well-intentioned conspiracy between parents and society to lie to young children about the existence of a fictional character and present evidence for his existence, children are able to reason for themselves that Santa does not exist by the mid to late elementary years. This well known phenomenon documents both the development and considerable power of the brains of young children to distinguish reality from fiction. To claim that this powerful ability plays no role in the evaluation of information which may or may not be "modeled" is absurd.

Much of the frightening messages which surround the notion of media effects as proposed by the GAM, suggests that brain changes can be documented via neuroimaging studies. These studies were one cornerstone of the Brown v EMA case, for instance, however during the Brown v EMA case, it emerged that many of the imaging studies had been funded by anti-media lobbying groups such as the Center for Successful Parenting. Other recent research has largely

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refuted the claims of these studies (Regenbogen, Herrmann, & Fehr, 2010). The entire enterprise of using brain imagery to make elaborate links with specific behaviors has also come under criticism (Vul, Harris, Winkielman, & Pashler, 2009). Many studies may overgeneralize findings quickly to desensitization and reduced impulse control prematurely when more parsimonious conclusions appear more suitable. For instance, decreased activation of the frontal lobes may simply indicate boredom, whereas some scholars may prematurely conclude such effects represent disinhibition of aggression or desensitization. The results of brain imaging studies thus may be akin somewhat to Rorschach cards with scholars seeing in them what they wish to see. This is one weakness of physiological measures, either increased activity (i.e., arousal) or decreased activity (i.e., desensitization) may be interpreted negatively in accordance with the theory, thus reducing opportunities for falsifiability.

3.3. Aggression is mainly learned

The view that aggressive behavior is primarily a learned behavior is an appealing view as it holds the promise that aggression may easily be prevented or reversed. In fairness, the proponents of the GAM do acknowledge that aggression can be difficult to change (Bushman & Anderson, 2002), but nonetheless the GAM remains focused on learning structures and its proponents do appear to promise society-wide changes were exposure to violence to decrease (e.g., Barlett & Anderson, 2009). As noted, this view that aggression is primarily learned can be traced back at least the Bandura's bo-bo doll studies, and in less systematic form, probably back to the Hellenic Greeks. As noted earlier, the GAM does tacitly acknowledge biological and personological inputs, but even these are often couched in the language of learning. Thus, at its core, the GAM remains largely a tabula rasa learning model.

The notion that aggression is primarily learned remains an article of faith for many scholars as evidenced by the APA statements on youth violence. However this view ignores considerable evidence regarding the genetic (Ferguson, 2010b; Rhee & Waldman, 2002), neurobiological (Kumari et al., 2006), neuroendocrine (Carré, McCormick, & Hariri, 2011) and other biological elements that contribute to aggression (Beaver, 2010). So too, although it is clear that environment and environmental strain can increase aggressiveness (Ferguson et al., 2008) it is less clear if learning is the primary mechanism through which the environment influences aggression. For instance, there is considerable evidence that *stress* from the environment rather than learning, is a key variable (Barash & Lipton, 2011). This is not to say that learning of aggression does not happen; only that it appears to be a relatively weak variable compared to other inputs. Thus, a model which focuses on learning primarily is unlikely to be adequate in describing aggressive phenomena. In their review of the factors involved in youth violence, the U.S. Surgeon General (U.S. Department of Health and Human Services, 2001) concluded that traditional "learning" inputs, such as child abuse or exposure to media violence, were relatively weak predictors of youth violence. Even where learning inputs are considered, they are not "all have won and must have prizes" as the effects for family violence appear to be more salient than those for media violence (Ferguson, San Miguel, & Hartley, 2009; Ferguson et al., 2008). This conflicts with the GAM's implicit treatment of learning based inputs as identical. It may be that exposure to violence in the family has greater impact due to the stress associated with this phenomenon. By contrast, use of media violence appears to relieve stress (Barnett, Coulson, & Foreman, 2008; Ferguson & Rueda, 2009) which may explain why media violence effects are historically negligible (Freedman, 2002; Savage, 2004).

Part of the issue goes back to the bo-bo doll experiments themselves and the degree to which children responding to demand characteristics may have been mistaken for "learning" of aggression. It may be time to seriously reevaluate the entire program of learning-based aggression research given the potential infusion of demand characteristics in many of the experimental studies.

3.4. Aggression is mainly cognitive

Although the GAM incorporates elements of affective arousal and desensitization, core elements of the GAM remain devoted to the notion that cognitive scripts play a key role in the development and production of aggressive behavior. For instance, as Anderson and Dill (2000, p. 788) "Thus, the danger in exposure to violent video games seems to be in the ideas they teach and not primarily in the emotions they incite in the player." Taking into account that in the quoted study the aggressive ideas taught to the participants were indeed taught by the researchers - namely giving blasts of white noise to the opponent - and the differences in the length of these on which the above is based is from Ms = 6.65 to 6.81 (Anderson & Dill, 2000, p. 788) and intensity of noise blasts did not differ, there is little evidence to support this position, and good reasons to suspect that, although cognitions may play some role in aggression, they may not be as central as implied by the GAM.

Most of the evidence for the cognitive components of the GAM come from research which finds that exposure to violence, particularly violent media, primes aggression related thoughts. To be clear, this does not mean that such exposures prime aggressive *intents*, rather that the availability of words related to aggression tend to be more highly available to individuals who have just been exposed to violent media. Measures used often involving examining reaction times for responding to aggressive words, filling in the missing letters of words (such that finishing kn__e as "knife" rather than "knave" would be aggressive), or completing the ending of stories. Where participants exposed to violent media or other forms of violence demonstrate greater accessibility to aggression related cognitions, this is considered evidence for the development of aggressive scripts and thus support for the GAM.

The problems with this approach are probably self-evident to many. One could expose participants to almost any theme or topic stimulus and naturally they will be thinking of that thing immediately afterward, much more so than other individuals not so exposed. This is not evidence that long-term cognitive scripts have been formed that will later be used to direct behavior. For instance, were experimenters to have some participants watch a movie with homosexuality themes such as *Brokeback Mountain* and another group of participants watch a heterosexually themed film, undoubtedly we would find that those who watched *Brokeback Mountain* had greater accessibility to homosexuality related cognitions immediately afterward. However the notion that this would be evidence for the creation of long-term cognitive scripts that would actually increase the likelihood of those participants adopting a homosexual lifestyle later on quite clearly has no merit. These measures are explicitly among those rejected by the U.S. Supreme Court in Brown v EMA for this very reason. There is no evidence that these measures are able to demonstrate that these cognitive processes predict aggression in the real world (Freedman, 2002; Gauntlett, 2005; Savage, 2004). We suspect that demonstrating the existence or non-existence of cognitive "scripts" for aggression is not possible with these types of measures and may, in fact, not be possible at all. Put another way, such a gulf exists between the construct of interest and the measures used to tap into that construct that the latter does not truly represent the former despite attempts to squash square pegs of data into round theoretical holes.

Perhaps just as important, this view of aggression as espoused by the GAM is artificially narrow, simultaneously cognitive yet mechanistic. Most scholars consider aggression as existing in differing forms, particularly along the lines of hostile versus instrumental (King et al., 2009), although perhaps not surprisingly proponents of

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the GAM advocate against such distinction (Bushman & Anderson, 2001b) perhaps because the GAM makes no such distinction. Despite this, evidence for a distinction between hostile aggression, and a more calculated and premeditated instrumental aggression continues to mount (e.g., Baker, Raine, Liu, & Jacobson, 2008; Fite & Vitulano, 2011; Tuvblad, Raine, Zheng, & Baker, 2009; Bobadilla, Wampler, & Taylor, in press). Within this hostile/instrumental distinction it is difficult to find where the GAM fits best. Instrumental aggression would appear, on the surface, to be more cognitive, involving considerable premeditation, yet instrumental aggression seems to be precluded by the automaticity and irrationality of the aggressive responses proposed by the GAM[2]. Thus, the GAM may be a better fit for hostile aggression, involving as it does a combination of arousal, affect and automatic cognitions. Yet, this very type of aggression appears to be better supported by diathesis-stress theories of aggression, which focus on a combination of biological and personality influences combined with environmental strain. For instance, a diathesis-stress model of aggression, the *Catalyst Model* has been found to be superior in the prediction of aggressiveness both in laboratory settings and in real life in direct comparison to the GAM (Ferguson et al., 2008). And as noted before, the general evidence for genetic, brain injury, strain and general gene x environment effects supports the use of a diathesis-stress approach to understanding such phenomenon rather than a social cognitive approach.

3.5. Aggression is mainly automatic

The GAM appears to posit aggression as an automatic and mechanistic learning process over which the individual has little control. To the GAM, modeling of behavior is passive, something that individuals *must* do rather than something they can *choose* to do. Continuing on the example above, the alternative reasoning that watching *Brokeback Mountain* (or playing violent videogames) may simply create more knowledge on a topic which can in fact be assessed and put to use by the viewer in the way he/she wants instead of blindly following the script to become homosexual doesn't appear to have been considered in depth. By contrast, to the GAM, automatic scripts are accessed when ambiguous scenarios present themselves in real life and individuals may develop a hostile attribution bias which is used to evaluate those incidents. DeWall et al. (2011) suggest aggression itself is a largely functionless and maladaptive behavior, something created in individuals by external learning opportunities and which is applied behaviorally despite its maladaptive nature. Although we do agree that arousal through strain and environmental stress is a likely component of aggression, we see less evidence in favor of desensitization as an established process, except perhaps in extreme circumstances such as those exposed to brutal warfare. Many of the studies purporting to demonstrate desensitization employ artificial laboratory environments and improbably scripted scenarios likely to create demand characteristics. Typical of these is Bushman and Anderson (2009) in which a "fight" including rather dubious dialog breaks out in the research lab just outside the participants' door immediately after being exposed to violent media. Coupled with the average undergraduate's likely familiarity with "tricky" psychology experiments and the improbability of a fight breaking out in such a controlled university laboratory environment, the potential for demand characteristics is obvious. At present, civic involvement and volunteering is at an all-time high among youth, despite their exposure to violent entertainment (Girl Scout Research Institute, 2009; Lenhart et al., 2008) and exposure to violent video games, particularly when played alongside parents, appears related to increased prosocial and civic behaviors in the real world, not decreases in such activity (Ferguson & Garza, 2011). Thus, the only component of the GAM with much merit is the arousal component, which is probably better described through diathesis-stress mechanisms.

Further, many forms of aggression ranging from premeditated murder (Fontaine, 2007) to bullying behaviors (Ferguson, San Miguel, Kilburn, & Sanchez, 2007) involve considerable forethought and instrumentality. Indeed one of the concerns Ferguson et al., express regarding the general ineffectiveness of anti-bullying is the failure of such programs to note the instrumental motives of bullying behaviors themselves. It should be noted that the GAM does not specifically exclude conscious thought processes and decisions (e.g., Bushman & Anderson, 2001b) Rather we find that these are not well described or delineated in the GAM model. This is particularly true on the acquisition side of cognitive scripts. A model which focuses implicitly on automatic knowledge structures risks moving us further down an unfruitful path. Although many forms of aggression may indeed involve affective (rather than cognitive) automaticity (i.e., bursts of hostile rage), these may be better understood by understanding underlying genetic and biological structures and their interaction with environmental strain than focusing on automatic cognitions. Or put another way, thoughts may follow behavior rather than the other way around - a concept employed in behavioral activation in cognitive behavioral therapy.

Regularly studies supporting the GAM are criticized on two main grounds, the poor validity of the aggression measures used and the failure of the studies to consider wider systems of variables which are involved in aggression (Ferguson, 2010a; Freedman, 2002; Gauntlett, 2005; Savage, 2004). At best, the GAM describes only a piece of the puzzle which applies to only a subset of aggressive behaviors, but even here the evidence is generally lacking. Many of the studies which allegedly support the GAM studiously avoid the inclusion of other variables which might better explain aggression (Freedman, 2002; Kutner & Olson, 2008; Savage, 2004). When those other variables are considered, GAM related concepts drop to nonsignificance as predictors of aggression (e.g., Adachi & Willoughby, 2011; Ferguson et al., 2009; Ferguson et al., 2008; Przybylski, Rigby, & Ryan, 2010). Increasingly the GAM does not fit well with data from its own primary area of application (i.e. media violence) and is not able to functionally predict behavior in the real world. Indeed, the correlation between predicted inputs (i.e. media violence) and outputs (i.e. youth violence) are in exactly the opposite direction proposed by the GAM, strongly but inversely correlated at $r = -.95$ (Ferguson, 2010a, b). In the next section of the paper, we explore the issue of how the GAM has been allowed to survive despite poor data support.

4. Why bad theories survive

4.1. Ideology and theories as "beloved" children

First, it must be understood that, despite claims to objectivity, science is a human endeavor and subject to human failings. Theory in science is necessary to guide science. However, once theories are proposed there is a risk that their proponents become emotionally attached to them and unable to consider them objectively. This is, in essence, the process Kuhn (1970) refers to in paradigm change, in which proponents of a preexisting theoretical paradigm defend the paradigm vigorously, even in the face of disconfirmatory data. Or put another way, scholars begin to invest their energy into *proving true* a particular theory rather than *falsifying* it, which would be the proper conduct of science. Once scholars have become invested (whether emotionally, financially or through their reputations) in a particular theory, they risk slipping into functioning as advocates for their position rather than as objective scientists. Particularly when some scientists have actually taken research funding from advocacy groups, this risk is considerable.

There is a well known guideline in science known as Occam's Razor which basically states that if two theories describe the data equally well, the one with the fewest assumptions is more likely to be correct. As we have discussed here, the GAM has assumptions which are demonstrably in error, and the data to support the GAM

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are weak (despite claims to the contrary from its proponents). Thus, it is unlikely to survive Occam's Razor, particular in comparison to the much better diathesis-stress models available.

We would like to add to Occam's Razor a second proposition, which we here call the *Advocacy Effect.* Stated simply, we suggest that the degree to which a theory in the social sciences is stated by its proponents with absolute conviction and claims of near universal support is inversely related to the quality of data available to actually support this theory. The more scholars make extreme claims in support of their theories, the more difficult to maintain an objective view once disconfirmatory information comes to light. One curious defense mechanism employed for the GAM has been the argument that the GAM cannot be overthrown by critics unless they supply an alternate theory (Gentile, Saleem, & Anderson, 2007). As noted alternate models already exist, although these seem to be dismissed on grounds that they are not the GAM. Nonetheless, this claim is false. For instance, as this article is written, a new faster-than-light particle has been discovered that, if independently confirmed, may overthrow General Relativity Theory. The presentation of disconfirmatory data is enough, no alternate theory is required. At this juncture, evidence of disconfirmatory evidence contradicting the GAM is ample. In defense of GAM, we acknowledge that this theory may, at best, describe how individuals may think about something they were recently exposed to for a short time. However, as these limits are very narrow (and which are too often ignored by its proponents), and the measured effect sizes are close to zero and better theories are available, the implicit argument "a theory that explains a bit is better than none" doesn't convince us.

Nonetheless, in a period of paradigm change we do not expect proponents of the GAM to cease their support of their own model. It is human nature to insist on the *truthiness* of one's own beliefs. Indeed, in their excellent book on the topic Grimes et al. (2008) discuss the deep roots of ideology in aggression research and how insistence on this rigid ideology has done much to damage our understanding of aggression. Arguably this stretches back to the bo-bo doll experiments themselves and the reification of these studies in academic psychology and the failure to consider their limitations.

4.2. Lack of criteria for falsification

Part of the issue for the survival of the GAM past its usefulness is endemic to social science itself. Put briefly, null-hypothesis-significance testing offers no road to falsification, particularly where non-significant results are simply dismissed as Type II error. This state of affairs has been long acknowledged (see Ferguson, 2009 for discussion) yet persists, in the main because few alternatives have been proposed. Perhaps the most likely alternative is to focus on effect sizes. If we do, we see that the effect sizes for various bodies of literature examining the game suggest the utility of the GAM has been very weak (Ferguson & Kilburn, 2009; Savage & Yancey, 2008; Sherry, 2007). Proponents of the GAM have typically responded by comparing their results favorably to important medical effects, although it is now known that these comparisons are based on miscalculations of the medical effects (Block & Crain, 2007; Ferguson, 2009). Without a clear road to falsification, a particular theory can "accumulate" a body of allegedly supportive work, although often weak in effect size, and simply dismiss any non-supportive null results as Type II error. Problems of publication bias exacerbate this phenomenon, already well documented for GAM related research (Ferguson & Kilburn, 2009). These issues can be exacerbated by scholars themselves. For instance, in a call for papers for a special issue on video games, one of the proponents for the GAM identified among the issues researchers "need to avoid" the issue of small samples stating "Behavioral effect sizes in this domain are often in $r = 0.18$ to 0.20 range, which require sample sizes of 230 or greater in order to be reliably detected. For variables that have larger effect sizes (e.g., aggressive thoughts) somewhat smaller sample sizes might have acceptable power" (Anderson, 2002). Although we dispute the reported effect sizes in this quote (recent meta-analyses have ranged from $r = .04$ through .15, Ferguson & Kilburn, 2009; Savage & Yancey, 2008), we do agree sample size is one issue for researchers to consider. Of course, with very large samples, almost any effect will become statistically significant, no matter whether trivial in practical application. However, more importantly, we express the concern that the wording of this call to papers communicates to potential authors not only sample size requirements, but what effects are expected to be seen. In other words, it appears to us the call is not only for large samples, but also for results which are statistically significant. Apparently, smaller samples are perfectly acceptable (i.e. aggressive thoughts) so long as purported effects may be "reliably detected." We find that, within the context of the GAM, there is a general push to support a priori beliefs about aggression rather than a systematic and objective process of evaluating the theory. Although our comments will undoubtedly be perceived as highly critical, we in fact view this largely as a function of human nature and believe the proponents of the GAM to have acted in good faith. We do suspect the peer review process is a part of the failure. In a field in which a small group of researchers become dominant it is possible they may be allowed to act as "gatekeepers", letting pass the work of their close colleagues, while quashing debate from other scholars. Indeed this is consistent with the process of paradigm change as illustrated by Kuhn (1970).

5. Where do we go from here?

The GAM is the logical end product of the social learning/social cognitive paradigm of aggression. Unfortunately, it is insufficient as an explanation of aggressive behavior; its predictions do not fit with real-world aggression and violence statistics (Ferguson, 2010a, b), it rests on assumptions that are problematic or demonstrably false, has largely failed to find supportive data in media violence, it's primary area of use (Ferguson & Kilburn, 2009; Freedman, 2002; Gauntlett, 2005; Savage & Yancey, 2008; Sherry, 2007) and has calcified into a rigid ideology which risks doing more damage to our understanding of aggression than aiding it. A decade ago the leading proponents of the GAM argued that it was time to "pull the plug" on the instrumental/hostile aggression dichotomy. Ten years later we find that support for the instrumental/hostile dichotomy remains strong, whereas support for the GAM has been weak. We are aware that proponents of the GAM have taken to claiming the existence of strong, consistent effects which, at times, crosses clearly into the phenomenon of *confirmation bias*. We further express the concern that the GAM has become an example of what Feynman (1974) referred to as a *cargo cult science;* that is, an endeavor that displays the trappings of a science outwardly, but inwardly is not dedicated to the falsification processes necessary for a true science. As Hall, Day, and Hall (2011a) have warned, in reference to the GAM and associated theories, continued insistence upon a failed theoretical model risks doing serious harm to the credibility of science. The level of rhetoric employed by proponents of the GAM, such as favorable comparisons with medical science (e.g., Bushman & Anderson, 2001a, b; Huesmann, 2007), or ad-hominem attacks on critics[3] (e.g., Gentile et al., 2007; Heusmann & Taylor, 2003), or claims of near absolute consistency in the research (e.g., Bushman & Anderson, 2001a, b; Huesmann, 2007) as well as the general defensive posture adopted by proponents of the GAM should have warned the scientific community that the field was straying from science into advocacy.

We reiterate that we believe proponents of the GAM have acted in good faith, although they may have mistaken their advocacy (often involving "protecting" children from media violence) for good science. Although our review of the GAM is critical, it is less our intent to be critical of individual scholars and more to acknowledge that a

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process of paradigm change is occurring within aggression research and media violence research in particular. The GAM is a final product of an old view, increasingly out of sorts with the data, and the time has come to pursue new directions. Even while hoping for a critical scientific discussion, we understand, in writing this essay, that proponents of the GAM are more likely to either react with great hostility or simply pretend as if this essay does not at all exist. We hope to be proven wrong and hope also the rest of the aggression research community will not follow suit and give serious introspection to our concerns.

As for the future, we find that theories which focus on diathesis-stress (Ferguson et al., 2008) and gene x environment interactions (Beaver, Rowland, Schwartz, & Nedelec, 2011) to have the most immediate promise. Further, for the immediate future, we expect the instrumental/hostile aggression dichotomy to hold. Naturally researchers, whatever paradigm they next adopt, must always be prepared to question their own beliefs and remain open to new data (Feynman, 1974). Otherwise, we will have forgotten the lessons of the social cognitive epoch and merely returned to a cargo cult science.

1) What the GAM proponents mean by "minor" aggression is not a trivial matter. For instance, many of the "aggression" measures used in laboratory studies, filling in missing letters of words, completing story stems, giving non-harmful noise bursts to an ostensibly consenting opponent in a reaction time game, would be "minor" aggression. Perhaps the authors here mean to distinguish between minor aggressive acts, such as those occurring in the lab, and violent acts in real life which we would agree could be maladaptive (although even then perhaps not always, such as soldiers doing their duty in war). However, if this is the case, we express the concern that the delineations between "minor" aggression and maladaptive aggression are not clarified by proponents of the GAM, who, to our read, quickly generalize "minor" aggressive acts such as those in laboratory studies to real-world violence as was done during the Brown v EMA case. Unless it is clearly identified which aggressive acts are "minor" and thus adaptive versus those which are maladaptive, it is too easy to adjust one's language to meet the circumstance, generalizing irresponsibly at one time, then quickly retreating to a differing position when criticized in order to deflect the criticism. We refer to this as the game of "Yes I said it, no I didn't".
2) In Bushman and Anderson (2001b), the authors do acknowledge the existence of premeditated and instrumental aggression, but there is little in the GAM to our eyes that predicts how, when or why such behaviors occur.
3) A recent example emerged as a byproduct of the Brown v EMA case. Two amicus briefs involving scholars had been written to the Supreme Court. One, with a statement on media effects written by proponents of the GAM (the Gruel Brief), supporting the notion video game violence is harmful, and a second group of scholars challenging such claims (the Millett Brief). Subsequently several GAM proponents who had signed the first brief published a paper which purported to demonstrate that they were the "true experts" (Sacks, Bushman, & Anderson, 2011). The conflict of interest in scholars anointing themselves as true experts, and besmirching their opponents in such a manner is probably obvious to most. Furthermore the paper consisted of little more than arguments to authority and ad hominem arguments. The methodological and theoretical limitations of this paper have already been addressed quite elegantly by scholars who were not signatories to either amicus brief (e.g. Hall, Day, & Hall, 2011b). Put simply, Sacks et al., simply didn't consider many of the journals in which their opponents had published. To give one example, Dr. Constance Steinkuelher, a signatory to the Millett brief, lists 49 peer reviewed publications on her CV, yet the methods of Sacks et al. would have identified only 9 (less than a 20% "hit" rate). Dr. Steinkuelher has gone on to become a leading expert on technology and video games for the U.S. White House, yet she would be listed a non-expert by Sacks et al. Perhaps those of us who were signatories to the brief critical of the California law regulating video games might add "expert" including airquotes to our job descriptions.

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