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COMMENTARY

INTEGRATIVE MODEL OR FRACTURING FRAMEWORK: BETTER WE HEDGE OUR BETS

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Weatherly and Dixon proposed a behavior analytic account of gambling. There were many excellent points made in the paper, and we were in agreement with many of them. Certainly, any conceptualization of gambling that ignores establishing operations and the contribution of verbal behavior will be incomplete. Here we address three main issues related to their proposed framework. First, it was not clear whether the framework encompasses gambling, 'pathological' gambling or both. Furthermore, definitions of these terms were not provided, an omission that invites conceptual confusion.

The second issue concerns the treatment of delay discounting. A preponderance of evidence suggests a relationship between steep delay discounting and addiction-type pathologies (alcoholism, nicotine dependence, pathological gambling, etc.). Weatherly and Dixon assert, however, that steep delay discounting is not only associated with pathological gambling but actually causes it. Further, they contend that factors known to increase or decrease the likelihood of pathological gambling exert their effects through their effect on delay discounting. Several difficulties with this aspect of their framework arise. First, it is not clear how *temporal* discounting relates directly to gambling behavior. Gamblers do not choose between small, immediate and large, delayed reinforcers but between small, certain and large, *probabilistic* reinforcers. If one conceives of gambling in this way, problem gamblers exhibit *shallow* probability discounting functions rather than steep delay discounting functions. Second, to argue a causal role for a hypothetical gradient that has been inferred from other behavior is circular reasoning.

The third issue concerns the degree of integration and inclusiveness of the framework. The framework excludes or minimizes integral pieces to the puzzle, such as genetics, neurophysiology, Pavlovian conditioning and the role of nonhuman research. The potential contributions of these areas on understanding individual differences should be reason enough for their inclusion. The remainder of the commentary will focus on the role of nonhuman research. The position of Weatherly and Dixon that "…an animal model of gambling will always be somewhat lacking in external validity" unnecessarily minimizes the potential contributions of nonhuman research to the understanding of gambling. Whenever nonhuman models are used to explore a human problem, there exists a trade-off between experimental control and external validity. That non-humans lack verbal behavior is not the only area where external validity may be

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lacking, but it is also far from a fatal blow to their potential usefulness in exploring the conditions that may be necessary and sufficient to produce gambling. Nonhuman research confers many advantages. First, while the authors admit that one of the pitfalls with a "reinforcement history" approach to human pathological gambling is that "the history of the human gambler…may never be exactly known," this is not a problem for a nonhuman model. Second, experimental designs (rather than correlational ones) can be implemented without having to get around ethical and legal problems. Finally, problems with interpreting cause-effect sequences (as with the relationship between marital status and problem gambling) can be minimized. These advantages can more than outweigh the cost of sacrificing external validity. Not too long ago drug abuse was considered a uniquely human phenomenon. This view changed when it was demonstrated that drugs could function as positive reinforcers for nonhumans; the resulting contribution to our understanding of drug abuse has been enormous. To discourage forays into modeling gambling behavior in nonhumans would seem imprudent.

Fortunately a nonhuman animal analogue of some aspects of pathological gambling-like behavior is quite possible. Consider a situation in which a rat is required to complete a large fixed ratio (e.g., FR 100) that results in a choice between pressing two levers. Pressing one lever results in the delivery of one food pellet with a probability of 1; Pressing the other lever results in the delivery of two food pellets with a probability of p. When $p < .5$, the rational long-term strategy is to exclusively select the smaller but certain option. A rat that continues to select the probabilistic option at lower values of p will lose more reinforcers than it gains, mimicking the situation of human pathological gamblers who continue to gamble despite mounting losses. Data from our lab show that rats experiencing this situation do, in fact, continue to "gamble" at values of p as low as .125. In fact, the rats will sometimes complete 40 FR 100's while receiving as few as 4 pellets for their trouble. Kendall's (1987, 1989) research also should be mentioned because it represents the first serious attempt to model gambling in nonhumans. He was critical of gambling analogues that lacked face validity (e.g., a simple variable-ratio schedule) and developed procedures with more similar features of human gambling.

That verbal behavior probably plays a causal role in the genesis and maintenance of gambling in certain cases is not disputed. The research by Dixon, Hayes, and Aban (2000) showing that instructions can result in persistence of gambling despite incurred loss is intriguing and could help explain many of the characteristics of 'pathological' gambling. That verbal behavior necessarily mediates all gambling however, goes beyond the data and diminishes/eliminates the role of nonhuman models of gambling. Also, the inclusion of verbal behavior does not solve the problem of individual differences. Indeed, the inclusion of verbal behavior will likely reveal another level of individual differences that have to be explained (such as why someone is more or less likely to follow a rule). Finally it should be recognized that explanations based upon verbal behavior can be just as 'nebulous' as those based upon reinforcement history, and one must still account for the reinforcement history that resulted in rule-following.

In conclusion, Weatherly and Dixon's proposed framework is an approximation of a comprehensive account of gambling because it neglects several critical elements such as genetics, neurophysiology, Pavlovian conditioning and nonhuman animal models that are all necessary to understand the phenomenon. To be truly comprehensive, we should hedge our bets and cast our nets much wider.

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