Commentary

Expanding on Barrett: The value of valleys

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ARTICLE INFO

Keywords:
Food psychology
Cultural psychology
Evolutionary psychology
Adaptive value

Not surprisingly given the many citations by Barrett (Barrett, 2020) of my paper on social psychology and science, I am extremely positive about Barrett's paper, and agree with all of his fundamental claims. This "response" is a supplement to Barrett's paper, rather than a comment. It expands on some of his points. To illustrate some of these points, I am copying, with minor modifications, an allegory I published in a chapter from 1981, on how to start studying a new area (human food choice). It was in one of those edited volumes that no one reads (Rozin, 1981).

1. Allegory

"The Martian Institute or Foundation for Furthering Science (MIFFS), Earth Sport Section (MIFFESS) was convening for its tenth year. Up to this time, the Research Program had been entirely devoted to a thorough study of one simple earth sport, that earthlings call "tennis". Progress was rapid and many laboratories were engaged in the enterprise. Thus, it was quite a shock when a few scientists at the ninth MIFFESS meeting suggested that MIFFESS support research on the uninvestigated sport of football, at some expense to the tennis program. The tennis researchers pointed out, with some justice, that they had made great progress and now understood the scoring, physics, and other aspects of the sport. Yet there were still many problems to be tackled in the microanalysis of the game. There was, for example, the well-known "yellow ball problem." A yellow ball was used on only some occasions, and no one could predict this distinct occurrence. Pigment analyses of the yellow ball were just beginning. "Why," asked the tennis researchers, "commit money to the murky enterprise of football when such good problems remain with tennis?" Nonetheless, in year nine, a small amount of Martian money was budgeted for the following year for the investigation of football. And now, the Committee had to evaluate the proposals.

The Committee was faced immediately with some fundamental disagreements among the applicants. Some claimed that the essential elements of the sport were some six creatures with black and white striped costumes. They were the only participants who appeared to be on the field at all times. Others focused on the more than 70 creatures, some sitting, some running, each with their own number. Some claimed that the ellipsoidal object noted in some observations should be the focus of study, but others pointed out that this object was rarely visible, and probably didn't matter. In the end, the Committee agreed that the numbered creatures might be the best bet for study. Since they had numbers, they could form the basis for precise quantification. For unnumbered participants, one might have to make up arbitrary numbers.

One proposal suggested correlating two measurable variables: the number of the creature, an incontrovertible datum, and the percent fat, of known biological importance. Other proposals suggested electrical rather than biochemical analyses. One group suggested use of the standard electroencephalogram (EEG) technique. Each creature would be wired up, and the total set of generated potentials for all of the creatures would be measured with a computer. The investigators worried about tripping on wires, but came up with the clever idea of using a blimp over the stadium, from which all wires could be suspended. Yet more clever proposals did away with the individual EEG, and proposed a total integrated reading, taken from the ellipsoidal extremes of the stadium itself. Using newer technology, and a mega magnet, it would be possible to get an fMRI of the whole stadium. They would see if the front of the stadium, the frontal area, was most active. Another set of investigators proposed to set up animal models of football.

One group encouraged the search for invariances, and impressed the committee with the preliminary finding that the summed numbers of the players on each of the two differently colored groups on the field remained roughly constant through the game, although players came in...
and out. Economists proposed to model these events. They assumed it was a mathematical puzzle type of game, and the purpose was to maintain the same sum of numbers as constant as possible throughout the game. The group with the lower variance would win the game.

In the search for order, a number of proposals pointed out that they should begin with what was apparently the most structured aspect of the game: the grouping of players in a circle, in fixed order, every minute or so. This was followed by another less ordered formation, and then by an apparently disordered set of movements, probably the players “letting off steam.” Positions of plot in the circle against players' number seemed reliable and a good point of departure. It was proposed that these observations be followed by detailed analysis of foot and hand positions of the players in the circle and after, in order to build up the elements of the game.

There was one proposal that was easy to reject. It stood out as the one that failed to follow the basic scientific dicta of objectivity and quantification. The authors proposed to simply observe the general flow of the game, and to supplement and guide these observations with interviews of the players, in an attempt to find out what the game was about. They proposed to ask the players such open-ended questions as: What is the purpose of this game? Is the ball important? And Why do the players move toward one end of the field for a while, and then to the other? The Committee unanimously agreed that this approach was not quantifiable, and that it relied on verbal reports, which were of questionable scientific status. Why, for example, should one believe a player's claim that he (she) moved to the right to misdirect other players, or that the rarely visible ball was the center of activity?

There was another proposal asking to explore books in libraries on Earth, in the hope that some information on the game could be unearthed. The proposal was rejected because it did not involve the discovery of new facts through research. After all, the proposed findings were already in books, somewhere.

And so it was that a decade of studies of the arrangement of players in the football huddle was supported, along with an analysis of the biochemical and electrical correlates of this circular event “(Rozin, 1981; modified).

2. Phenomena and functional relationships are the basic foundation of science and can lead to big theory

As Barrett and I agree, we are too ready to look for mechanisms, do experiments, and quantify before we know what to look at. We need robust and often general phenomena and functional relations. These are often derisively referred to as “Just descriptions”.

Darwin, was initially “just” an “observer”, someone with informed curiosity, manifest over decades. All the relationships he uncovered would, by themselves, have trouble reaching publication in psychology journals; they lacked a mechanism, or a theory. With patience, look at where those observations led!

Probably the major theoretical event in biological science of the 20th century, was the discovery of the structure of DNA by Watson and Crick, in 1953. In their famous article in Nature, they cited four critical experiments. It can yield Nobel prizes in the sciences.

Phenomena and functional relations play a major role in physics and biology. One of the best understood (and most physics-like) areas in psychology is color vision. Two outstanding color vision theories emerged from physicists in the late 18th century, and were subsequently refined. Both have been challenged to explain a set of phenomena and functional relations; the luminosity curve (visibility to humans as a function of wave length), and the fact that you could mix all color experiences from three appropriately chosen wavelengths. The color mixing observations dictated that a theory would have to, in one way or another, postulate three and only three independent channels.

3. The problem of evolutionary psychology: Adaptive value is relatively easy to assign; evolutionary origin and history is much harder

In a sort of prevision of evolutionary psychology, Jonathan Schull and I wrote a general chapter entitled “The adaptive-evolutionary point of view in experimental psychology” in the Handbook of Experimental Psychology (Rozin & Schull, 1988). In it, we laid out a model of biopsychological explanation, which was an expansion of four questions that Niko Tinbergen (1963) presented for the study of animal behavior. Two types of explanation of special relevance to the evolutionary social sciences are adaptive and evolutionary. Showing that a behavior was adaptive in the human ancestral environment is an important accomplishment, and describes much of evolutionary psychology. But this is just the beginning of the evolutionary adaptive argument. It is then important to show that this adaptive function was actually the selection pressure that established the behavior, and to show how the behavior evolved. That is much harder than suggesting an adaptive value.

Evolutionary accounts have been constructed beautifully in some areas of evolutionary biology, greatly facilitated by the presence of fossil remains. Behavior does not generally leave fossils, so historical accounts of evolution are very difficult to construct. Of course, insight into adaptive value may put order into our understanding of certain events, and lead to important predictions, but it is an incomplete explanation.

In my field, the evolutionary and cultural psychology of food, there are many lovely demonstrations that culinary practices in various cultures have adaptive value, that is, they improve nutrition. This is not sufficient to account for the actual evolution of these behaviors, without further evidence. For example, the corn tortilla is central to the cuisine of MesoAmerica. The traditional preparation of corn for tortillas almost always involves cooking the corn with a base (often powdered shells high in calcium hydroxide, called “cal”). Katz, Hediger, and Valleroy (1974) demonstrated that this process substantially improved the nutritive value of the corn in three different ways. So, there is an adaptive value to this important cooking process. But how did it arise? Was the nutritive improvement perceptible?

While doing research on chili pepper in a traditional Mexican village, I asked some women why they boiled their corn in cal. They had no idea, and correctly responded that they “had always done it that way.” When I continued to explore this with them, they suggested that it might make the ground cooked corn easier to roll out into a tortilla. I asked them to prepare some corn with or without cal and try to roll out tortillas. They were right: the cal softened the tough outer layer of the corn. So now we face an interesting question. What is the relation, in terms of the evolution of this technique, between the palpable tortilla roll-out effect and the important, but less apparent, nutritional consequences? This is not an easy problem.

The same issue arises with the substantial evidence that disgust motivates avoidance of pathogens, a clear adaptive value. But how was this originally discovered, and why did it produce a uniquely human emotion? Cooking provides yet another example. It is one of the major advances in human evolution. It kills pathogens, but it also changes the taste of food and makes it more chewable. How did these forces work
together in the establishment of cooking?

4. Cultural psychologists and evolutionary psychologists should be friends

Instead of battling over how much variance in humans, or what percent of human universals, can be accounted for by culture or evolution, it is much more fruitful for these fields to work together. The evolutionary psychologists have the great theory, but the cultural psychologists have the best data to test it (Richerson & Boyd, 2008; Rozin, 2010). It is a lucky break for the evolutionary social sciences that there are hundreds of cultures and countless cultural artifacts and behaviors which have evolved in accordance with roughly the same model as biological evolution. Cultural evolution happens much more quickly, and leaves a much more accessible record, in artifacts and writing.

Of course, there is purpose in cultural evolution: individuals or groups can imagine desired states, and create things to achieve these results. They can continue to work on a problem, overcoming failures, but profiting from them. The problem of local minima does not prevent progress. Although variation, transmission and selection operate in both cultural and biological evolution, variation can be intentionally created in cultures, and selection can include an eye to the future.

To me, it is ironic (Rozin, 2010) that the history of writing does not play a central role in cultural evolution scholarship. It is an extraordinarily powerful invention. There is much cultural variation, and of course, we have a written record of its evolution. It is a fascinating story.

5. What aspects of human life should we study?

A major issue for all academic fields is deciding what particular thing one should study. The actual determinants for this are often circumstantial and unpredictable: an inspiring undergraduate teacher, a graduate sponsor who can offer grant-based stipend. A rational approach would weigh the costs and benefits of a particular choice. One major dimension of choice is between a crowded and an empty area of research. There are reasons why an area might be crowded. On the whole, crowded fields are more promising, and it is easier to publish and get grants. Much of the technology for research has been developed.

An empty area may be empty for good reason.

Humans are faddish creatures, and this holds for research areas and techniques, along with everything else. We are quite reasonably influenced by others, and topics like mating are really important and interesting. Sometimes, fads are caused by a conceptual or technological breakthrough, for example, fMRI, or the discovery of the structure of DNA. The question is not whether this convergence of investigators is justifiable, but rather whether it results in an optimal distribution of research effort. Barrett documents clustering on certain substantive areas.

This is a matter I have discussed in the sequel to my 2001 paper that Barrett presents. I will give the title, because it encapsulates the idea: “Domain denigration and process preference in academic psychology” (Rozin, 2006). A first determinant of problem choice in psychology has been a greater interest in basic processes, such as perception, memory, and attention, dating back to William James. The table of contents of the Handbook of Evolutionary Psychology (one by Barrett and one by me), and 1 of 32 chapters in the second edition of the Handbook of Cultural Psychology. Only one of Barrett’s top 20 terms from EHB has anything to do with food (i.e., disgust). This minimal amount of attention to food probably does represent the amount of literature in the different areas, but it does not represent the importance of food for life.

6. Conclusion

A colleague recently noted to me that psychology wasn’t as easy as it used to be, because we had already plucked all the low hanging fruit (a food metaphor). That just isn’t true (Rozin, 2007). Every time we create a pile of work on the landscape of areas of human life, we leave a hole somewhere, and there are lots of holes to explore. It is fun to be in a hole; marginal progress is high, you don't have to read too much literature, and no one scoops you.

References


