Intentionality, Social Play, and Definition

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ABSTRACT: Social play is naturally characterized in intentional terms. An evolutionary account of social play could help scientists to understand the evolution of cognition and intentionality. Alexander Rosenberg (1990) has argued that if play is characterized intentionally or functionally, it is not a behavioral phenotype suitable for evolutionary explanation. If he is right, his arguments would threaten many projects in cognitive ethology. We argue that Rosenberg's arguments are unsound and that intentionally and functionally characterized phenotypes are a proper domain for ethological investigation.

KEY WORDS: Ethology, cognitive ethology, play, intentionality, evolution, definition.

INTRODUCTION

Many behavioral biologists consider play an important behavioral phenotype. They have a hard time, however, coming up with a consensus definition of play. Most biologists who have observed mammals in the field can give examples of behaviors they consider to be playful, and while there may not be consensus about a definition, there is considerable consensus about cases – biologists agree that many mammals and some birds engage in play, especially during the early years of their lives. Sometimes play is reported in other classes of organisms, such as reptiles, but there is less consensus about these cases.

Putting aside, for the moment, the question of how to define play, and accepting biologists' intuitive classifications of some behaviors as playful, we can illustrate the importance of understanding play by identifying some of the questions that have been asked about it. Such questions include: why has play evolved in some species but not in others, how do ecological variables influence the expression of play, why are there species differences in the structure or form of play and for when play is "scheduled" during the life of the animal, how does

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the development of play in individuals affect the later behavior of those individuals, and how is play related to learning, socialization, and cognition. Although it is easy to speculate about these questions, they are not easily answered empirically, due to the difficulties of conducting the long-term field observations that would be required to answer them.

Social play – i.e., play involving interaction between two or more individuals – is especially interesting because of the degree of communication and cooperation that is required between the participants. For example, social play in canids (wolves, dogs, coyotes) frequently involves behaviors such as growling, and biting accompanied by shaking the head from side-to-side, actions that would normally preface or accompany aggression. (Other behaviors seen in play include those associated with courtship and mating.) Similar types of social encounters are observed in many primates and other mammals. Participants in such play interactions must have some way of controlling aggressive responses (or sexual responses) to such behaviors. Indeed, in canid species several stereotyped signals, such as the "play bow" have evolved apparently for this purpose. From one perspective, play signals seem to be about other signals. For example, play bows can be glossed as indicating that subsequent signals, such as growls, do not have their normal meaning because "what follows is play". This has led some play researchers to describe play signals as "metacommunicative" – communication about communication.

Researchers are also interested in studying the cognitive dimensions of human social play. For example, Flavell et al. (1987) consider pretend play to be an important step in the cognitive development of human infants. Whether or not participants in such play must possess the concept of pretense is discussed below, but insofar as non-human play is similar to human play, clearly it is tempting to think of non-human play and play signals in both cognitive and intentional terms. Given the present state of research and knowledge about play, we don't think this temptation should be resisted. If we are right, then an evolutionary account of play could provide insights into the evolution of cognition and intentionality. The study of play is potentially important because it offers an opportunity to expand cognitive ethology into an area that includes species other than the usual primates and dolphins (and a lone parrot) that are targeted for comparative research in animal cognition. However, such expansion is not without its critics (Bekoff and Allen 1994). Alexander Rosenberg (1990) thinks that if one succumbs to the temptation to characterize play intentionally then one can no longer have an evolutionary account of play. We shall argue that he is mistaken.

Biologists and psychologists alike have found the task of defining behavioral phenotypes vexing, and they have found the task of defining play particularly vexing. Wilson (1975), Fagen (1981), Bekoff and Byers (1981), Martin and Caro (1985), Mitchell (1990), and Bekoff (1993) all explicitly refer to the difficulties of defining social play. As Wilson puts it, "No behavioral concept has proved more ill-defined, elusive, controversial, and even unfashionable" (1975, p. 164). Some of these authors even express skepticism about the merits of providing a definition of play (Bekoff and Byers 1981; Martin and Caro 1985) yet they go ahead and attempt to give one. The emphasis on providing definitions seems to be a hangover from the influence of behaviorist psychology, which saw precise definition of terms as necessary for empirical rigor. Even though behaviorism is no longer the force it was, the common view persists that definitions are needed, else how can we know what we are studying? This common view skirts dangerously close to the paradox of the *Meno*, where Socrates refers to the "trick argument that a man cannot try to discover either what he knows or what he does not know" which he expands by saying "he would not seek what he knows, for since he knows it there is no need of the inquiry, nor what he does not know, for in that case he does not even know what he is to look for" (Plato 23rd C BP: 1961, 80e). The point here is that requiring a rigorous definition prior to empirical research may unreasonably require possession of knowledge that must first be gained by empirical research.

Nonetheless, Rosenberg (1990) uses the problem of defining play to motivate his claim that there will not be an evolutionary theory of play, ordinarily understood. He argues that play is a category that is unlikely to have an evolutionary explanation because it must be intentionally and functionally characterized. Rosenberg thinks that, at best, biology can provide a theory of "play-behavior", which must be defined in such a way that the resulting concept will be "only metaphorically connected to our ordinary conception" of play (Rosenberg 1990, p. 187). In this paper we question the soundness of Rosenberg's arguments.

Although Rosenberg's arguments are directed specifically against play, parallel arguments could be formulated for many other behavioral phenotypes, threatening many projects in ethology. The attack on intentionally characterized phenomena is especially worrisome for cognitive ethology. Our critique of Rosenberg's arguments should be seen as a general defense of certain explanatory projects in ethology, cognitive ethology, and behavioral biology, against a class of attacks exemplified by his concerns.

THE ARGUMENTS

Rosenberg uses two arguments to support his claim against an evolutionary theory of play. Schematically presented, they are:

A (1) Play is a (high)-order intentional activity.
(2) There can be no evolutionary biology of intentionality.
Hence (3) There can be no evolutionary biology of play.

B (1) Our common conception of play is a functional characterization.
(2) Because play is functionally characterized, actual cases of play have heterogeneous causes and effects.

Hence (3) There can be no evolutionary biology of play.

Argument *A* has, we shall argue, one dubious and one false premise. In argument *B* we have no complaints with the premises, but we do not think it is valid. Our reason for doubting its validity is closely related to the reason for disagreeing with premise *A*(2). Rosenberg presents these arguments in the reverse order that we have them here. For expository reasons we treat *A* first.

ARGUMENT A

Premise *A*(1): The term "intentional" in this premise (and the next) is used in its philosophical sense, meaning having representational content. Ordinary intentions to act are intentional in the philosophical sense, but so are beliefs, desires, and a whole host of other mental states (see Allen 1993 for an explanation of this terminology).

Rosenberg argues for *A*(1) first by considering E.O. Wilson's (1975) characterization of our intuitive notion of play as "a set of pleasurable activities ... that imitate the serious activities of life without consummating serious goals" (Wilson 1975, p. 164 as quoted by Rosenberg 1990, p. 183). Rosenberg (but not Wilson) calls this characterization of play a definition and draws attention to the notion of imitation that it contains. He states that imitation requires "a representation of the behavior to be imitated, together with the recognition that [one's behavior] will be produced as a pretence" (pp. 183–184) and a little later he claims that imitation entails an organism "bringing [its behavior] under the description of a pretense" (p. 184). Accepting this characterization of play would commit us to the intentionality of play (a degree of intentionality which Rosenberg thinks we should be unwilling to attribute to animals – but see below for a criticism of this). Rosenberg then tries to establish the high-order intentionality of social play. He argues that when one organism plays with another we must be prepared to attribute at least third order intentionality in Grice's (1957) sense. According to this scheme, simple beliefs (or other propositional attitudes) about material states of affairs display first-order intentionality, beliefs about beliefs are second-order, beliefs about beliefs about beliefs are third-order, and so on (see Dennett 1983 for an application of Gricean orders of intentionality to experimental design and interpretation in ethology). On Rosenberg's view, for animal *a* truly to be playing with *b*, it must be that "*a* does *d* [the playful act] with the intention of *b*'s recognizing that *a* is doing *d* not seriously but playfully ... So, *a* wants *b* to believe that *a* wants to do *d* not seriously but with other goals or aims" (Rosenberg 1990, p. 184). This is third-order because *a* **desires** that *b* **believes** something about *a*'s **desires.**

Is the concept of pretense or third-order intentionality required for play? Take the concept of pretense first. Flavell et al. (1987) report that the tendency for human children to engage in pretend play (e.g., manipulating an object as if it is something else) is biologically preprogrammed and first appears at around 12 months. The ability to answer questions accurately about the pretend-real distinction comes much later, but before 3 years old. Of course, verbalization is not the only test of concept possession, but between 1 and 3 years old, parents play a significant role in teaching children to distinguish between play-based imaginings and reality. Initially, pretend play by children may be imitative of adult behaviors without the child being able to reflect on the distinction between pretense and reality.

The Gricean analysis of two-party play seems similarly fanciful when applied to infants. Following Millikan (1984, chapter 3), who questions the applicability of a Gricean account to linguistic meaning in adult humans, we will consider the attribution of intentional states significant only if they correspond to actual changes in the organism's cognitive machinery (i.e., its nervous system) and these changes are causally responsible for the behavior in question. Given this understanding of intentional state attributions, it seems to us unlikely that play with inanimate objects should be analyzed in terms of third-order intentional states since it seems unlikely that anything corresponding to such states is causally responsible for infant play. At *most* play with an inanimate object might involve a second order belief about one's own goals or motives, and there is no reason why play with another organism could not be initiated on the same terms, i.e. without reference to any model of the internal representations of the other organism. However, even second order intentionality (representation of one's own goals or motives) seems implausible in the case of 12 month old children who play. Hence one is faced either with denying that very young children play (in the ordinary sense) or with denying that play really requires high-order intentionality. If Rosenberg accepts the first horn of this dilemma, we suggest he is the one who is not dealing with the ordinary conception of play.

Premise *A*(2) does not specifically mention high order intentionality so perhaps premise *A*(1) does not require high order intentionality to support the conclusion *A*(3). We believe that play does manifest cognitive abilities in organisms, and that these cognitive abilities are intentional in the philosophers' sense (Bekoff and Allen 1992; Jamieson and Bekoff 1993). We are more than willing to concede that play is intentional. Even with this admission, however, Rosenberg's argument fails because of the falsity of its second premise.

Premise *A*(2): Our strategy in denying the second premise has two parts. First, we attempt to give a *reductio* (reduction to absurdity) of the premise. Second, we indicate some positive arguments for the view that evolutionary biology needs intentional notions (these arguments appear in greater detail in Allen 1992a, b).

Consider the following argument scheme:

A* (1) *X* is an intentional activity.
(2) There can be no evolutionary biology of intentionality
Hence (3) There can be no evolutionary biology of *X*.

Suppose that arguments having the form of A^* are valid and consider some possible values for the variable X. In particular, imagine running A^* on kin recognition or language speaking. Both of these seem to be intentional activities, yet it would be surprising if either lacks an evolutionary explanation. Of course, the surprisingness of a conclusion is no guarantee of its falsity, but evolutionary biologists do attempt to explain both kin recognition and language. So, unless the actual practice of behavioral biologists is profoundly misguided there is good reason to think that kin recognition and language are suitable subjects for evoutionary explanation. On the assumption of the validity of A^*, this constitutes a *reductio* of the second premise.

Whether or not A^* is valid we can both challenge the positive arguments often produced in favor of $A(2)$ and provide a positive argument for the view that intentional notions are essential to the practice of cognitive ethology.

Rosenberg does not make clear why he thinks that an evolutionary biology of intentionality is unlikely to be forthcoming, but his reasons appear to be derived from arguments found in Dennett (1969) and Stich (1983). Rosenberg asks whether we could attribute the concept of mouse-catching to a cat, asking "Does it have the concept of mouse, *Mus musculus* in Linnaean terms?" (p. 184). Dennett and Stich use similar examples to argue that there can be no science of intentionality (partly) because the difficulty of specifying the contents of animal cognitive states shows that intentional descriptions cannot be made precise enough for scientific purposes. Allen (1992a) argues that these arguments are unsuccessful because they presuppose an improper account of content specification. Applied to Rosenberg's version of the argument, the objection is that if his question is asked of pre-Linnaean humans we should answer "no," but we should not infer from this that those humans had no concept of mouse. (Even if you think they didn't have the same concept as us, it does not follow that they have no concept at all. For a discussion of what kinds of evidence might support concept attribution in non-human animals see Allen and Hauser 1991.) Failing to share our conceptual scheme does not rule out the possession of some other conceptual scheme. Nor is it required that these other conceptual schemes be neatly describable in contemporary English. Neither Rosenberg's, nor Dennett's nor Stich's examples establish that attributing concepts (or other intentional states) to animals is inappropriate, so the examples do not support the stronger claim that science cannot make use of such attributions.

A more direct criticism of $A(2)$ can be derived from considerations that seem to require the use of intentional language in evolutionary biology. The selective advantage of cognitive states appears to be that they enable organisms to react appropriately to their environments *in a variety of circumstances* (Griffin 1992). If it were not for the functional capacity for *flexibility* in cognitive systems, there would be no advantage over simpler, hard-wired mechanisms. Allen (1992b) argues that attributing intentional content enables cognitive ethologists to describe the evolutionary significance of cognitive states. Evolutionary accounts explain the presence of certain phenotypes because those phenotypes are produced (in part) by present genotypes which are in turn descended from earlier genotypes which produced phenotypes with a selective advantage. No matter what the actual causal mechanisms linking genotype to phenotype, and phenotype to selective advantage, the selective advantage that results is what is important for explanations in evolutionary biology. We characterize the phenotypes in terms of the *functional* capabilities they provide. For example, light-sensing organs have appeared independently at several different times in the evolution of species on this planet. The evolutionary biologist believes this convergent evolution can be explained in terms of the similar benefits provided to individuals possessing such organs. Notice however, that to describe an organ as light-sensing is to understand its role in a functional analysis (Cummins 1975) of an organism's behavior, and to ignore the specific details of how such a functional capacity is implemented. Of course, under selective pressure, more efficient implementations will be selected over less efficient ones, all other things being equal. But if efficiency is not a significant factor, only functional capacity will be important to the fitness of the organism. (In most real cases there is a complex interplay between functional capacity and implementation-specific properties, such as energetics.) Insofar as an evolutionary account can be given of the convergent development of light-sensing organs, it is in virtue of the common selective advantage of the functional capacity for sensing light in organisms of different species.

Our discussion of the implementation-independent specification of phenotypes (i.e., those traits suitable for evolutionary explanation) may be usefully compared to Sober's (1984, 1993) discussions of the supervenience of fitness on the physical properties of organisms. Sober points out that biologists are concerned with formulating principles, such as Fisher's (1930) *fundamental theorem of natural selection* which relates rate of evolution to variance of fitness within a population. Such principles apply across species that vary widely in their physical characteristics so it is extremely unlikely that there will be a single physical explanation of the empirical applicability of the theorem. Sober extends this argument to the behavioral phenotype of predation. Biologists try to come up with theories to predict changes in the ratio of predators to prey that apply equally to, e.g., lion-antelope predation and plant-insect predation (Sober 1993) or bird-spider predation (Riechert 1993). Clearly the physical basis for predation in lions is much different than in Venus fly traps, but the equations describing the ratio of predators to prey are intended to apply equally to each case. Despite Rosenberg's misgivings about physically diverse, functionally characterized behavioral phenotypes such as play, evolutionary biology is rife with such phenotypes, which biologists theorize about and attempt to study empirically.

Where does this leave intentionally characterized phenotypes? Well, organisms with *cognitive* capacities differ from those that are without them partly in virtue of the plasticity of their behavior in response to different environmental conditions. A reasonable hypothesis is that members of species with limited cognitive capacity are less able to survive in conditions which differ from those in which the species evolved or in which their neural systems were conditioned. If so, the proper functional characterization of cognitive systems is that they allow for an organism to represent and respond to local environmental conditions. All things being equal, it doesn't really matter just how this is done; what is important is *that* it is done. Just as an evolutionary account of light-sensing organs is committed to functional characterization of these organs, so too is an evolutionary account of cognition committed to functional characterization of cognitive abilities. But the functional characterization of cognitive abilities describes them as representational, and hence, in philosophers' jargon, intentional. So, it seems not just that there *can* be an evolutionary account of intentionality, but that there is good reason for thinking that evolutionary accounts of cognition must include intentionality. So, we conclude that Rosenberg's premise *A*(2) is false. Play can be characterized intentionally without jeopardizing its suitability for evolutionary explanation.

ARGUMENT B

It should now be obvious from our account of the premise *A*(2) why we think that the argument *B* is fallacious. It is worth, however, examining the reasons behind Rosenberg's view.

Rosenberg thinks that there can be no unified evolutionary account of functional concepts because there are too many different causes and effects which make some piece of behavior an instance of play. He draws an analogy between play and clocks, pointing out that because there are so many different mechanisms that constitute clocks there is no "single general explanatory theory that really explains what clocks do, how and why they do it" (p. 180).

The problem with this argument is that the kind of "single general explanatory theory" referred to is not (and should not be) the kind of thing evolutionary biology is necessarily concerned with. Such univocal explanations may rule out other important causal factors or delay progress in coming to terms with a multifactor process (Hilborn and Stearns 1982). While it is the concern of some branches of biology (particularly molecular and cellular) to explain *how* certain organs do what they do, evolutionary theory is generally concerned with *what* they do and *why* they do it. So while it would be foolish to expect a singular molecular or cellular account of light-sensing capabilities across species, it is not foolish to expect unity in some aspects of the evolutionary explanations of the development of such organs (although, of course, there will be differences in the evolutionary histories across different species). Indeed, if the arguments above are correct, evolutionary explanations *typically* deal with functionally characterized phenomena, abstracting away from specific mechanical details, except insofar as these details affect the fitness of organisms or give clues about evolutionary history.

Almost every topic behavioral scientists study is functionally characterized rather than mechanistically characterized. Sober's example of predation, discussed above, is a case in point. One more example should serve to convince. Consider the wide interest (since Darwin) in mate choice and its effect on sexual selection. Darwin himself supposed that a single account of sexual selection could be provided across all species (Darwin 1871), despite the obvious point that the specific causes and effects of sexual behavior vary enormously across species as do the secondary sexual characteristics. Mate choice in birds is influenced by very different physical properties (e.g., tail size and color) than mate choice in frogs ("rivitt ... rivitt"). And the behavioral effects of mate choice are also very different, unless one unifies them under a functional characterization such as "egg fertilization".

If Rosenberg is right that there can be no evolutionary theory of functionally characterized phenomena which are implemented by a wide variety of causal mechanisms, then there can be no evolutionary theory of sexual selection. Indeed, one wonders whether, given Rosenberg's view, there can be evolutionary theory at all, except as an historical account of the development of phenotypes.

DEFINING BEHAVIORAL PHENOTYPES

The practice of giving explicit definitions in the behavioral sciences has its roots in behaviorist criticisms of 19th century comparative psychology (especially Watson 1930). Comparative psychologists frequently deserved the charges of anthropomorphism leveled against them by behaviorists. (See Burghardt 1985 for an historical account of the rise of behaviorism particularly as it affected comparative psychology and ethology.) As a result of the criticism directed at the earlier excesses of comparative psychologists, the tendency among many students of behavior is to want to specify precisely what they are talking about before presenting empirical results. The lingering influence of behaviorism is beneficial insofar as definitions may facilitate the design and subsequent interpretation of experiments. Definitions may also facilitate communication between scientists. We think it is a mistake to treat definitions as anything more than working definitions – i.e. as rough guides to the phenomena under investigation – and it is therefore a mistake to try to discount whole areas of scientific research on the basis of definitional problems, as Rosenberg attempts to do for play. Although some of the points we make in this section about definition may seem to many philosophers to be too obvious to belabor, the substantial degree of effort that behavioral scientists put into definitional issues indicates the need for such labor.

Scientific rigor does not come via precise *a priori* specification of the concepts used to pick out the examples, but in precise application of experimental techniques to compare and contrast putative examples with respect to observable characteristics. Examples tentatively identified by a working definition (or even just a hunch) should be examined rigorously for similarities and differences. If similarities are found that allow for interesting theoretical generalizations, then confidence in the usefulness of the category increases. On the basis of empirical work one refines the concept and the working definition.

Of course, in some cases the refined concept may end up quite different from what one starts with. Also, if one finds that there are no consistent similarities which support theoretical generalizations then one may give up on the idea that the things examined constitute a scientifically interesting natural category.

The question of whether or not it is useful to recognize a category of behavior such as social play is a question of theoretical usefulness: Are there useful generalizations about the behaviors lumped together in this way? The study of a topic like play ought to be approached like the study of any other (candidate) natural kind. To study play, one ought to start with examples of behaviors which superficially appear to form a single category – kittens chasing one another, colts romping in a field, lion cubs wrestling each other, dolphins dunking turtles, human children playing cops and robbers – and look for similarities among these examples. If similarities are found, *then* we can ask whether they provide the basis of useful generalizations, for example in terms of a common evolutionary explanation, or whether a common adaptive function is served (see Bekoff and Byers 1981, p. 315, for a comparative chart for play in eight different species).

In the unlikely event that science is ever complete, then it may be possible to give precise stipulative specifications of the theoretical terms used by science. In the interim, the best that we can hope for is to pick out phenomena by more or less rough criteria and use comparative methods to test hypotheses about which similarities are the theoretically useful ones. The importance of conceptual analytical work should not be underestimated in this process. Working definitions and other means of specifying the conceptual commitments of a given theoretical apparatus serve as important catalysts to the formulation of empirical questions (Bekoff and Allen 1992). However, such working definitions ultimately do not determine the extensions of the concepts they purport to define.

Given this conception of the proper role of definitions, we view Rosenberg's insistence that a piece of behavior is (really) play only if it is intentional to be premature. Whether play, or any other behavioral phenotype, is intentional is an empirical question. It is not something to be decided by definition. So, even if Rosenberg's premise $A(1)$ is true, it is not true *by definition* – it requires empirical support. Empirical investigation might support a common adaptive explanation for all the behaviors biologists lump together as play, or perhaps only for the subset of those behaviors that are intentional. It is also possible that empirical work would support the view that none of these behaviors form an interesting class from the point of view of evolutionary explanation. They might, for example, be evolutionarily insignificant side-effects of a selected feature such as having a nervous system of a certain complexity. In Sober's (1984) terminology, it may be that there was selection *for* other behavioral phenotypes resulting in selection *of* play. It might also turn out that some of the behaviors that we are initially inclined to consider play are not usefully put in the same category as others. The same is true of any behavioral category applied across species, e.g., communication, deceit, learning, or murder.

Likewise, if concerned about the intentionality of a particular type of behavior, one should look at the purported examples and decide whether they provide evidence of intentionality. For example, in two-organism play do specific signals serve to initiate or terminate the play (Bekoff 1975; Bekoff 1977)? If so, then this *may* provide evidence that animals are communicating in their intentions to each other (Bekoff 1975: Bekoff and Allen 1992). Of course, to determine that this is evidence for intentionality requires a theory of what counts as evidence for intentionality. Whether or not the category of intentional behaviors is a useful category is itself an empirical question. In other words, are there useful generalizations that can be formulated by regarding some behaviors as belonging to the class of intentional behaviors? We believe that there are theoretical reasons for recognizing such a category. Whatever the case, we think the task of theory building is ultimately more interesting than that of picking over definitions.

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