Amundson's insightful and entertaining commentary on my defense of adaptationism closes with a warning to the adaptationist (and indirectly to the ethologist): The trade-off I propose to biologists is "not a bargain"; it is "a poisoned pill."

I concur with his warning against the view he describes as mine: It is not the view I have espoused, however. Amundson has reacted to some exaggerations and incautious remarks of mine with some exaggerations of his own, but since he is not alone in reading me this way, I accept the blame for the misdirection.

Amundson is certainly right when he urges adaptationists not to abandon their obligations to the causal histories of the features they hope to describe as adaptations. But what he describes as my "disenchantment with causal theories" is nothing of the kind. My point was rather that biologists who claim to rely abstemiously on nothing but unvarnished, uninterpreted causal histories of actual selection are kidding themselves; that is just as impossible as the Skinnerian dream of a psychology that does all its explaining and predicting by appeal to nothing but an unvarnished history of reinforcement. What I have advertised is not a "noncausal" theory of adaptations (or of beliefs and desires), but rather indirectly causal theories. Just as there has to be some intelligible causal path that grounds a claim such as "he didn't think the gun was loaded" (it can't be defended just because it fits so nicely with a particular story one likes to tell), so there has to be some intelligible causal path that grounds any claim about an adaptation. I wanted just to show how any such path becomes intelligible only on interpretation — from the intentional stance, with its inevitable assumption of optimality or rationality. (I hope this has become clearer in Dennett 1987 and 1988.) The "free-floating rationales" I have pointed to are not utterly unanchored — they are just not tied to independently identifiable representations of those rationales in the causal ancestry. Just as it is folly to look for the brain-writing to clinch "he didn't know the gun was loaded" — the (brain-)writing might exist, but its existence is not required for the truth of the claim — so it is folly to think that for every adaptation there must be something analogous to a specific change order in a design-and-manufacturing project. Unless one were to suppose — with zero plausibility — that the design process of natural selection can never "kill two birds with one stone," one should not expect to be able to "read off" the adaptation claims licensed by even total historical information about causation, birth, and death.

Amundson says, "The ethologist would be mistaken who expected the intentional stance to yield objectively accurate causal descriptions of vervet monkeys' cognitive insiders." This is true if we read it to assert that the intentional stance does not directly yield such causal descriptions; but of course the whole point of adopting the intentional stance is to work directly toward that very goal: objectively accurate causal descriptions of creatures' cognitive insiders. (see Dennett 1989)

Amundson points out that he and I emphasize different aspects of both behaviorism and mentalism, which is why when we line them both up with adaptationism and its critics, we get the opposite mapping. He sees Chomsky's structuralism as the central mentalist alternative to behaviorism, whereas I have always viewed that as a weirdly extremist doctrine ("nobody ever learns anything") held by a vocal minority of mentalists (or "cognitive scientists"). There is a mapping from these extremists of cognitive science onto extremists in evolutionary theory: those who hold that there really is only trivial selection — the constraining structures and principles are (nearly) totally constraining. I don't put Lewontin and Gould in that camp of critics, but rather into the puritanical camp of critics who think that interpretation that uses optimization is to be steadfastly avoided — an impossible ideal they themselves cannot live by. When I aligned adaptationism with mentalism, I was thinking of the mentalists who agree with the behaviorists in thinking that there is genuine learning (as opposed to mere "triggering"), but who disagree with them about how that is to be described and explained. As Amundson shrewdly observes, I was not just poking fun with my parallel. My advice to Skinnerians has always been: loosen up, and notice that you can have all the virtues of causal-historical science plus "mentalistic" interpretation as an indispensable lever; so my advice to Gould and Lewontin is the same: You can't do biology without optimality assumptions, so instead of condemning the game, learn the rules.
Continuing Commentary

References

(1983a) Adaptationism was always predictive and needed no defense. Behavioral and Brain Sciences 6:360–61. [RA]