

# Continuing Commentary

**Commentary on Daniel C. Dennett (1983) *Intentional systems in cognitive ethology: The “Panglossian paradigm” defended*. BBS 6:343–390.**

**Abstract of the original article:** Ethologists and others studying animal behavior in a “cognitive” spirit are in need of a descriptive language and method that are neither anachronistically bound by behaviorist scruples nor prematurely committed to *particular* “information-processing models.” Just such an interim descriptive method can be found in *intentional system theory*. The use of intentional system theory is illustrated with the case of the apparently communicative behavior of vervet monkeys. A way of using the theory to generate data – including usable, testable “anecdotal” data – is sketched. The underlying assumptions of this approach can be seen to ally it directly with “adaptationist” theorizing in evolutionary biology, which has recently come under attack from Stephen Gould and Richard Lewontin, who castigate it as the “Panglossian paradigm.” Their arguments, which are strongly analogous to B. F. Skinner’s arguments against “mentalism,” point to certain pitfalls that attend the careless exercise of such “Panglossian” thinking (and rival varieties of thinking as well), but do not constitute a fundamental objection to either adaptationist theorizing or its cousin, intentional system theory.

## Doctor Dennett and Doctor Pangloss: Perfection and selection in biology and psychology

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Daniel Dennett wrote a paper on vocal signaling in vervet monkeys. In it he commended the “intentional stance” to behavioral ecologists. An anonymous referee claimed that Dennett’s paper exemplified evolutionary adaptationism, which, according to the critic, had been shown to be “bankrupt” by Gould and Lewontin (1979). These evolutionary theorists had referred to adaptationism as the “Panglossian paradigm.” In response to this critique, Dennett added a discussion of adaptationism to the vervet monkey paper. He likewise expanded the title, which became “Intentional systems in cognitive ethology: The ‘Panglossian paradigm’ defended” (Dennett 1983). I have no direct knowledge of Dennett’s thoughts in revising the paper. But, taking the “intentional stance” with regard to the episode, and having some understanding of the beliefs and desires probably involved, one may construct the following scenario: Dennett is a man of civility and culture, but he was unhappy with this dismissal. He wishes to give a strong response. So he did the most offensive thing he could think of under the constraints of civility. He compared Gould & Lewontin to B. F. Skinner. [See also BBS special issue on the work of B. F. Skinner BBS 7(4) 1985.]

**1. Adaptationism as mentalism.** There are reasons not to take the analogy between Gould & Lewontin and Skinner too seriously. Most details address argumentative style rather than theoretical substance. Richard Dawkins, Dennett’s strongest biological supporter, calls the parallel “mischievous” (Dawkins 1983a, p. 360). And Dennett himself, responding to a reply from Lewontin, says that Lewontin just “can’t take a joke” (Dennett 1983, p. 388). Jocularly aside, I believe the issue to be an important one. The debates surrounding mentalism in psychology show deep similarities to those surrounding adaptationism in evolutionary theory. Dennett has the parallel precisely backwards, however. This commentary will present an analogy between psychological and evolutionary theory with the op-

posite polarity to Dennett’s. It will explore the implications of Dennett’s alignment of adaptationism with mentalism, and argue in conclusion that Dennett’s defense of adaptationism ought not to be welcomed by adaptationists.

The more rhetorical of Dennett’s comparisons between Gould & Lewontin and Skinner point out, for example, that each party claims its opponents’ explanations to be too easy to invent, and that each exploits especially poor examples of opponents’ thinking. It would be hard to find a serious debate in which these techniques were unused; exaggerating ones opponent’s flaws hardly makes one a Skinnerian. In a much more substantive critique, Dennett says that Gould & Lewontin, like Skinner, prefer historical causes to *post hoc* “rationalizations,” and for this reason reject the assumptions of optimality which are shared by mentalism and adaptationism. Mentalism assumes optimal rationality in the psychological subject (for reasons to be discussed later). Biological adaptationism assumes that organisms are optimally adapted to their environments. Skinner’s opposition to mentalism can be traced to its optimality assumptions; likewise Gould & Lewontin’s opposition to adaptationism. But the problem, according to Dennett, is not that adaptationism and mentalism are *falsely* accused of being irrefutable and tautologous. In fact, the critics are right in this judgement. The critics’ mistake is their assumption that what is irrefutable must lack scientific value. Mentalism and adaptationism are not themselves *theories*, to be judged by their fit to the data. They are rather “stances or strategies that serve to organize data, explain interrelations, and generate questions to ask Nature” (Dennett 1983, p. 353). Stances and strategies have scientific value independent of their falsifiability. Gould & Lewontin’s criticism of adaptationism and Skinner’s of mentalism are mistaken for the same reasons. Adaptationism stands to its critics as mentalism stands to behaviorism.

**2. Adaptationism as behaviorism.** We will return to Dennett’s analogy, but first I will introduce a rival version. My version rests on similarities in the structure of explanations in psychology and evolutionary theory. The first similarity has been independently recognized by many authors. Among them are two of the campers Dennett has so gleefully assigned to the same tent – Richard Lewontin and B. F. Skinner. A third is an even stranger bedfellow, and one whom I expose with a great pretence of reluctance – Daniel Dennett himself (Lewontin 1982; Skinner

1981; Dennett 1975). Each of these writers has pointed out the similarity between natural selection and the Law of Effect. Natural selection explains phylogenetic adaptation by positing a nondirected ("random") source of heritable variation within the lineage, together with selection by the environment of those variants which happen by chance to be better suited to survival and reproduction. The Law of Effect (with its cognates trial-and-error learning and operant conditioning) explains the ontogenetic adaptation of individual behavior (what we call "learning") by positing nondirected ("spontaneous") variations in behavior, together with some form of selection by the environment which allows successful behavior to persist and the unsuccessful to die out. In each case, the adaptations are cumulative, and so indefinitely fine tuning is possible in theory. The ontogenetic behavioral adaptation of the learning organism can be explained by a selective mechanism virtually identical in its structure to that used to explain the phylogenetic adaptation of the evolving species.

The important point is not just that the parallel exists. It is that the selective mechanism possesses a sort of singularity. It is arguably the *only* explanation of complex (i.e. nonaccidental) adaptedness which does not beg the question. It produces a naturalistic explanation, perhaps the *only* such explanation, of the phenomena which have given comfort to teleologists throughout the ages.<sup>1</sup> The selectionist mode of explanation is surely one of the most intellectually satisfying achievements of the 19th century.<sup>2</sup> We can now give a naturalistic answer to the question of how adaptations come about. But a related question is not settled with the acceptance of selection. It is this: Which characters of organisms *are* adaptations? In biology this is by no means a simple issue. The debates about adaptationism are not about whether natural selection is the appropriate explanation of *adaptations* – virtually everyone agrees that it is. The issue is how adaptations are identified, not how they are explained.

Accepting selection as the sole mechanism of adaptation can create semantic problems. An example is the conflict between new and old criteria for use of the term "adaptation." We are commonsensically tempted to say that a character is "well adapted" (or "an adaptation") to a task just in case it performs the task well. In this sense, human eyebrows must be an adaptation for keeping perspiration out of the eyes. But if we accept that all adaptations are explained by selection, then we run the risk of concluding that eyebrows were selected for because of their drainage properties, based *simply* on the fact that their drainage properties are useful. Now, it may well be true that eyebrows evolved in this way. But we must be careful not to reach this conclusion by the wrong route. Our belief that selection explains adaptation should not lead us to believe that selection explains *everything good in the world*. If selection explains all adaptation, then whether something is an adaptation depends not only on whether it serves a purpose, but whether it had a certain sort of causal origin – a selective origin. After all, things with different causal origins may equally well serve current needs. Let us resolve to call a character an "adaptation" only when we mean to assert that the character evolved selectively to serve a specific purpose. This is consistent with reflective biological practice.

If this stipulation seems odd, consider a parallel bit of semantics from psychology. Thirty years ago psychologists talked about the "learning" of language. Now the topic is the "acquisition" of language. Today we expect quite different things from a course entitled "Learning" and one entitled "Cognition." To the lay individual there is nothing especially theory-laden about the term "learning." As far as the individual in the street knows, a course in "learning theory" might well discuss mental images and the language of thought. But within the profession these terms have taken on proprietary meanings. Whether a particular ontogenetic change exhibits "learning" or "cognition" depends, as we sometimes say, on which theory you believe. (Actually, it depends on which theory is true.) To call a change in

behavior "learning" is to assert that it arose as a result of a certain kind of causal process. This was a semantic decision, made at quite an early stage by linguists and cognitive psychologists. They could have launched a crusade to rescue the term "learning" from the behaviorists, but they tactically surrendered the terminology. The behaviorist credo was accepted – all learning *was* conditioning. The important question remained; which features of behavioral change are cases of learning?

It might be difficult for generative linguists to explain to lay individuals just why they were reluctant to say that little Johnny is *learning* English. Generativists would have to explain that the term "learning" is associated with a particular psychological theory – in their opinion, the wrong one. They might then describe the alternative theory, and why they prefer the non-selectionist explanation of language development. The debates about adaptationism are similar. Presented only with adaptationism and its denial, the nonspecialist is hard put to imagine what the debate is about. What is the alternative to adaptationism? Dennett gives very little help in this regard. He names a few alternatives mentioned by Gould & Lewontin (genetic drift, material compensation, "phyletic inertia," and architectural constraints; Dennett 1983, p. 352), but with no particular regard for their nature. Usually he refers only to the "historical-architectural" preferences of Gould and Lewontin.

Just as linguists have alternatives to behaviorism which underwrite their rejection of "learning theory," there are substantive nonadaptational evolutionary explanations which underwrite the criticisms of adaptationism. I will try to show that at least some of these alternatives are to adaptationism as cognitive and nativist psychological theories are to behaviorist psychology. The basic formula is this: On the one hand are theories which explain existing characters as environmentally selected results of randomly generated variation. On the other hand are theories which explain existing characters as environmentally influenced variations on highly conservative structures, and explain change by reference to complex internal mediating mechanisms. On this second sort of view, the environment may *shape* the expressed outcome of development, but the shaping effects are significantly constrained by internal structure. Theories of the first sort (let us call them "environmentalist") include behaviorist learning theories and selectionist/adaptationist biology. Theories of the second sort ("structuralist") include nativist and cognitive psychology, and evolutionary theories which stress, for example, morphological and embryological constraints on evolution (see Maynard Smith et al. 1985, for a recent discussion).

**3. How adaptationists defend adaptationism.** Research programs are based on central factual beliefs. The term "adaptationism" is used both to refer to an evolutionary research program and to its central factual belief. The belief is that the great majority of characters of organisms are recognizable adaptations to environments, formed in their detail by natural selection. The research program of the same name uses the belief as a guide to further research – one assumes that a complex structure is an adaptation, and attempts to devise a selective explanation consistent with the known (or the plausibly hypothesized) facts of natural history.

*Qua* research program, adaptationism is indeed unfalsifiable, but benignly so. After all, research programs are "instruments" of research generation. Instrumentalism is true of research programs, and Dennett is right to defend them on this ground. The story is different when "adaptationism" refers to the factual belief on which the research program is based. If adaptationists systematically fail to consider nonadaptive explanations of characters, as Gould & Lewontin claim they do, then their factual belief in the ubiquity of adaptation is immunized from challenge. An unfalsifiable belief is worse than an unfalsifiable research program.

Most biological defenses of adaptationism adopt a strategy different from Dennett's. They do not defend the unfalsifiability

of adaptationism-the-program. They rather argue that individual adaptive explanations *are* falsifiable – that adaptationism-the-belief is subject to test whether or not adaptationism-the-program is (Dawkins 1983a). Adaptationists can and do refute particular adaptive explanations (often replacing them with adaptationist alternatives). Gould and Lewontin may have overstated the problem of falsification, as even some supporters acknowledge (e.g. Kitcher 1985, p. 232). They must not insist (nor, on my reading, did they insist) that the entire adaptationist research program and/or the central belief in the ubiquity of adaptation, should stand or fall with the tested success of each individual explanation. On the other hand, critics of adaptationism have a perfect right to expect appropriate nonadaptive explanations to be at least *sometimes* entertained by adaptationists. Competing *types* of explanation must be considered, not just competing tokens of the same type. Let us consider how adaptationists deny individual adaptive scenarios, and what alternatives to adaptive scenarios might exist.

Current adaptationists are able to reject individual adaptive explanations because they operate under substantive Darwinian constraints. To be sure, a few can find it in their hearts to praise pre-Darwinian adaptationists – even William Paley.<sup>3</sup> But the basic commitment is not to adaptation itself, but to the power of natural selection to create it. This can be seen in the disputes surrounding group selection. Were the *real* Dr. Pangloss alive today, he would point to characters which exist “for the good of the species.” But most modern adaptationists reject these. Why? Because careful analysis seems to show that natural selection could give rise to such characters only under very rare circumstances. Adaptationism gives way under the pressure of selectionism. Other adaptationist refutations of adaptive explanations are similar. A plausible scenario of selective history is a necessary corollary to an adaptationist explanation. To be sure, there are some comparative studies which focus on extant groups.<sup>4</sup> This may give the appearance of *unencumbered* adaptationism – the sort which asserts adaptedness without any commitment to its cause. But whatever the outcome of a comparative study, it will be subject to selectionist evaluation. In the words of a critic of Gould & Lewontin, adaptationism “is a method of generating hypotheses *based on our knowledge of the processes and limitations of natural selection*.” (Brown 1982, p. 884, emphasis added.) I submit that *any* proposed adaptationist explanation which cannot be supported by a plausible selectionist scenario will be, for that reason alone, rejected by modern adaptationists. Today’s adaptationist, like William Paley, expects the world to be rich in adaptations. Unlike Paley, his faith is in selection. A character, no matter how useful or cleverly related to an organism’s needs, will not be seen as an adaptation unless it can be explained as an outcome of particular mechanisms of natural selection.

There are many productive lines of research in biology which give alternatives to adaptationist explanations.<sup>5</sup> None of the alternatives denies that natural selection will produce adaptation. Rather, it is contended (1) that other evolutionary forces may give rise to patterns of complex characters best accounted for by nonselective explanations, or (2) that there are reasons to doubt that the conditions necessary for the operation of natural selection actually obtained in the evolutionary history of a purported adaptation.

Recall that the operation of natural selection requires copious, heritable, nondirected variation. This is the source from which the environment selects reproductively successful variants. But suppose that there is some internal, nonenvironmental mechanism which constrains the variation of a certain character within a phylogenetic lineage. The discovery of this constraint *contributes to the explanation* of why organisms in that lineage exhibit the characters they do. In doing so, it reduces the scope of any selectionist/adaptationist explanation of the same facts. Suppose that the variations occur preferentially in a certain direction. This also reduces the scope of the

selectionist explanation. The absence of environmental competition from non-character-holders erodes the force of selectionism yet again.

My present purpose is not to argue that such constraints, biases, and non-competitive environments actually existed in the history of life on earth. These are questions of fact. The point is this: *Adaptationism hangs on these questions of fact*. Consider one example. Many respectable biologists believe that terrestrial vertebrates have never had variation for limb number. Without variation, there is nothing for the environment to select. Unlike the insects, the deep developmental processes which have been constant in land vertebrates strongly constrain limb number. Our ancestors the lobe-finned fishes had four, and we inherited their developmental program. You and I and dogs and bats do vary in the morphology of our limbs, and today’s great diversity must result largely from adaptive selection. But even here it is argued that variation is biased by embryological mechanisms, for example, that digits can be added and lost only in certain orders (Shubin & Alberch 1986). In amongst the diversity lurk universals; selection and adaptation probably explain diversity, but structural constraints may explain universals.

This should sound familiar to students of psychology and linguistics. Postbehaviorist linguists propose linguistic universals, and internal constraints on language acquisition. Surely *some* of language is “learned” in an environmentalist way, involving reinforcement and whatnot. But the explanatory force of environmental selection is eroded by the nonrandomness of the child’s responses, and the evidence for complex internal processes which mediate the child/environment interaction. It is often claimed that the child’s linguistic environment contains too little behavioral training (i.e. selection) to account for the complex development observed. Something else, innate and internal, must account for the development. If these claims are true, then the environmentalist/selectionist theory – Skinner’s theory – is weakened. This is one of many parallels between the environmentalist-versus-structuralist debates in biology and those in psychology. I will spare the reader further details, but repeat one reminder – at issue are factual matters, not to be settled by methodology-slinging alone. The issues are similar in biology and psychology, but not in the way Dennett suggests.

**4. Dennett’s defense of adaptationism.** Up to this point my argument may sound like false dichotomy. After all, perhaps Dennett and I are both right – adaptationism is in some ways like mentalism and in others like behaviorism. (Dennett himself has recently taken this approach; Dennett 1987.) But, as it happens, adaptationism is not like mentalism in the ways Dennett says. Notice that he and I have chosen different aspects of antibehaviorist psychology to emphasize. I cite the causal explanatory force of postulated internal structures, while he cites the assumption of optimal rationality needed to support intentional interpretations. My focus is syntactic; his is semantic. To see the significance of this, recall that Dennett’s notion of the “intentional stance” involves a special and somewhat controversial view on the relations between mentalistic psychological ascriptions and the causal insides of the psychological subject. He holds that mentalistic ascriptions of beliefs and desires have a sort of explanatory value *even if* no genuine, causal, internal mental structures exist to be identified as those beliefs and desires. The value of mentalistic talk lies not in its reference to an internal, causally active reality, but in its instrumental usefulness in making predictions and inspiring experiments. (This is not a majority view among philosophers. Many believe that mentalism has explanatory force only if it describes actual, internal causes. There are disagreements, of course, on whether this condition is satisfied.)

Dennett has recommended the intentional stance to cognitive ethologists. It has not been generally noticed that this suggestion has *two distinct* instrumentalist implications. First and uncontroversially (as noted above) the stance is a research

program, an instrument for generating research. Second, Dennett's view on mentality gives an instrumental interpretation to *each* program-inspired mentalistic explanation. So not only the program, but each of its products, is an instrument for prediction. The ethologist would be mistaken who expected the intentional stance to yield objectively accurate causal descriptions of vervet monkeys' cognitive insides.

Dennett applies his two-pronged instrumentalism to adaptationism in a manner exactly consistent with his analogy. Not only adaptationism-the-program, but also individual adaptive explanations lack reference to specific causal processes. Even intentionality, the mentalist's Holy Grail, is given an adaptationist analog. "The indirect bearing of the Eiffel Tower on my thought about the Eiffel Tower, like the indirect bearing of the toxicity of those ancestral bee corpses that weren't removed from their hives on the current behavior vis-à-vis corpses of today's bees, is not the sort of relation that can be illuminated by a mechanistic, nonintentional account, however voluminous." Dennett sees this unilluminating, mechanistic, nonintentional account as *including* selection mechanisms; it is Gould & Lewontin (not adaptationists) who foolishly advocate "unadorned mechanical history of actual selection" (Dennett 1985, p. 764).

Dennett points out, correctly, that the truth of a nonadaptationist story (e.g., a Laplacean microphysical account) does not require the falsehood of all adaptationist stories of the same feature. He then attributes the *denial* of this belief to adaptationism's critics. As evidence that anti-adaptationists have physical reductionist tendencies, Dennett quotes a case – the only specific case mentioned in his adaptationism discussion. The case is the one discussed above – quadrupedality in terrestrial vertebrates. The nonadaptationist explanation is that the ancestral species had four limbs. This structural account asserts that ancestry, rather than adaptation, explains quadrupedality. Dennett analyzes the case as follows: He attributes the nonadaptationist conclusion to reductionism – the false belief that adaptationist explanations are in competition with causal ones.<sup>6</sup> Adaptationists would be expected to develop an optimizing rationale for quadrupedality, unrestrained by the physicalist/structuralist alternative.

Is this how adaptationists think? Is the affection for "mechanical histories of actual selection" a mistake of *anti*-adaptationists? Does the relation between an adaptation of today and a selective mechanism of yesteryear involve the deep problem of "aboutness"? Are particular adaptive explanations actually tools for prediction, independent of physical history? Here are the words of adaptationists, quoted from passages specifically responding to antiadaptationist criticism. Richard Dawkins reports that, "the adaptationist controversy . . . is concerned with whether, given that we are dealing with a phenotypic effect big enough to see and ask questions about, we should assume that it is the product of natural selection." (Dawkins 1982, p. 33) Later, "The neo-Darwinian adaptationist . . . insists upon knowing the exact nature of the selective process that has led to the evolution of the putative adaptation." (ibid., p. 51) These passages do not sound as if mechanistic histories of selection were something the adaptationist could just as well do without. And would an adaptationist float an optimizing rationale over the ancestry explanation of quadrupedality? Verne Grant, even while "striking back" (his expression) against critics of adaptationism, *specifically accepts* the ancestry explanation of quadrupedality. He does not accuse its proponents of reductionism, and does not propose a free floating adaptive rationale (Grant 1983, p. 155). If adaptationism were decoupled from the mechanisms of selection in the way Dennett says, we should be able to find adaptationists who doubted whether a particular adaptation were indeed the causal result of a specific physical history of selection. There have been many such thinkers, of course. But then along came Darwin.

**5. A caution to adaptationists.** My version of the psychology/biology analogy stresses similarities in the causes cited by Skinner and adaptationists, and in the sorts of causal constraints cited by their critics. Dennett cites optimality, a commitment said to be shared by mentalists and adaptationists, as the central feature of his version. To be sure, both mentalists and adaptationists make optimality assumptions. But do they do so for relevantly similar reasons? Evolutionary adaptationists regard optimality as an implication of a specific causal theory – Darwinian selection. If the conditions necessary for unconstrained selection are present, optimal adaptation is to be expected. But for mentalistic psychology, the story is more intricate.

Mentalists assume optimal rationality in a psychological subject because if they didn't there would be no criterion for correctness in the semantic interpretation of mental states. As Quine (1960, pp. 59, 69) showed in his discussion of "radical translation," any semantic interpretation is as good as any other when we suppose our subject to be irrational. Mentalists, by definition, *want to make* semantic interpretations. It seems that there is no way to do so, under our present state of knowledge, without presuming rationality. We rely on rationality by default – because we have nothing else to rely on.

The mentalist would be freed from his dependence on rationality if a *better* way of assigning semantic content were available. Imagine that a mentalistic Newton or Einstein were to come along with an acceptable, naturalistic, causal theory of intentional content assignment – perhaps a nice little neurological or behavioral semantics. We would no longer be forced to *assume* optimal rationality. It would be independently testable. We simply assign our semantic interpretations as the theory tells us, and then assess the subject's beliefs by our *now-independent* normative criteria of rationality. Today, sad to say, such a theory is not available. In its absence, mentalism does not (and cannot) *derive* rationality from a specific causal theory. Mentalism reluctantly assumes optimality for lack of anything better.

So the cornerstone of Dennett's analogy between adaptationism and mentalism is an epiphenomenon. Optimality is a concept which adaptationists anchor to a central causal theory, and which mentalists presuppose for lack of one.<sup>7</sup>

Given Dennett's views on mentalism, there is no reason for him to be embarrassed by the lack of causal warrant for the optimality assumption. In noncausal theories, calling something an "epiphenomenon" is no special curse. If belief-ascriptions are tools for prediction, and if optimality assumptions prove to be useful tool-sharpeners, then why worry about independent causal warrants?

The problem is that Dennett's disenchantment with causal theories has resulted in an insensitivity to the causal texture of evolutionary theory. He removes from adaptationism its commitment to selectionist causation and replaces it with the problem of intentionality. The one thing which *saves* modern adaptationism from starry-eyed, contentless, Leibnizian pseudo-optimism is its commitment (in principle at least) to Darwinian mechanisms and to the hard cold facts of natural history. Intentionality, on the other hand, is the most notorious unsolved problem of modern philosophy. The tradeoff is not a bargain for biologists. Keep in mind that the critics of adaptationism *do have* causal warrant. Adaptationism is to be defended not against people who find it instrumentally useless, as incapable of "generating questions to ask Nature." It must be defended against specific alternative *causal* explanations of evolutionary phenomena. If adaptationists accept Dennett's exchange, if they commit themselves to optimal adaptation on instrumental rather than on causal grounds, Pangloss may live but Darwin will die. Dennett's defense of adaptationism is a poisoned pill.

#### NOTES

1. Dennett (1975) points to the singularity of selection in explaining why the Law of Effect will not go away; Dawkins (1983b) argues that

Darwinian selection is, virtually *a priori*, the explanation of any adaptive evolutionary process anywhere in the universe; Skinner slightly more conservatively claims that selection (either ontogenetic or phylogenetic) accounts for all adaptiveness in *this* world, and an active contingent of “evolutionary epistemologists” agrees, some of them extending the domain of selection in amazing ways (see the readings in Plotkin 1982).

2. The first publication of a fully naturalistic selectionist explanation in psychology occurred in the same year as its introduction to biology; Darwin’s *Origin of Species* and Alexander Bain’s *The Emotions and the Will* were both published in 1859.

3. A. J. Cain is cited by Dennett (1983) praising Robert Hooke on insect antennae; Dawkins (1983b) describes adaptationists like himself as “neo-Paleyans.”

4. For example, two explanations of sexual dimorphism can be tested by their differing predictions of dimorphism in monogamous and harem-holding primates (Maynard Smith 1984, p. 301).

5. A few of the nonadaptationist explanations are those involving macroevolutionary theory, restrictions on optimality which come from the details of genetic mechanisms (especially in sexually reproducing species), the “neutralist” theories of molecular geneticists, and the developmental constraints discussed briefly below.

6. Dennett 1983, p. 354. It is particularly bizarre in this passage that the people Dennett hints to be microphysical reductionists are macroevolutionary theorists. Most of these people don’t even believe that macroevolution can be reduced to population level processes, let alone to microphysical particles. Reductionism is simply *not* a motivating factor in the criticisms of adaptationism. Dennett has again superimposed a topic from the philosophy of mind onto evolutionary biology, and again it doesn’t fit.

7. Some, including Dennett, have used natural selection itself as a reason to assume optimal rationality. I take it that Stich (1985) has shown the failure of this line of reasoning. In this context Dennett could *accurately* be accused of Panglossianism.

## Author’s Response

### Dr. Pangloss knows best

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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 insides.” 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 insides. (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.

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Commentary on Daniel C. Dennett (1988) *Précis of The Intentional Stance*. BBS 11:495–546.

**Abstract of the original article:** The intentional stance is the strategy of prediction and explanation that attributes beliefs, desires, and other “intentional” states to systems – living and nonliving – and predicts future behavior from what it would be rational for an agent to do, given those beliefs and desires. Any system whose performance can be thus predicted and explained is an *intentional system*, whatever its innards. The strategy of treating parts of the world as intentional systems is the foundation of “folk psychology,” but is also exploited (and is virtually unavoidable) in artificial intelligence and cognitive science more generally, as well as in evolutionary theory. An analysis of the role of the intentional stance and its presuppositions supports a naturalistic theory of mental states and events, their *content* or *intentionality*, and the relation between “mentalistic” levels of explanation and neurophysiological or mechanistic levels of explanation. As such, the analysis of the intentional stance grounds a theory of the mind and its relation to the body.

## The matter of other minds

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Dennett’s (1987; 1988) *The Intentional Stance* (henceforth *Stance*) continues a long tradition of philosophers’ writing prescriptions for psychology. In the past, prescriptions were part of cures for uncertainties about what we can know (epistemology) and doubts about what we must do (ethics). The current philosophical discussion about psychology is often limited to the scientific status of the concepts of folk psychology. *Stance* is also devoted to such matters (but see Dennett 1984). In this commentary we examine the relevance to autism of the methodology advocated in *Stance*. Autism also raises questions related to the practical, ethical side of folk psychology not discussed here.

When it was first described by Kanner in 1943, autism was defined as an “innate inability to form the usual biologically provided affective contact with people.” Subsequent research has confirmed that autism is a matter of severe social unadaptedness which is only remotely related to the syntactic aspects of linguistic and intellectual skills. For example, linguistic impairments are specifically in the domain of semantic-pragmatic competence. Impairments in the basic skills needed for interaction with others lead in turn to difficulties autists have in (to use Dennett’s words) “making sense of themselves.” In the face of this diagnosis, one is tempted to use the notion of a “theory of mind.” Should we conclude that, unlike chimpanzees (Premack

& Woodruff 1978), autists do not have a theory of mind (Baron-Cohen et al. 1985), that they are specifically retarded in the development of a theory of mind (Baron-Cohen 1989), or alternatively, that autists might have only a theory of mind and lack an ability for ascribing mental states in natural interaction (de Gelder et al. 1988)? Such conclusions sound very much like answers from folk psychology. We understand them but we do not know what they mean. *Stance* promises to extricate us from the predicament we find ourselves in when for the best of scientific reasons we refuse to dump folk psychology. The trouble is that *Stance* speaks in many tongues. It defends surface intentionalism (SI), deep realism (DR) and it promotes a distinction between notions and beliefs (NBD). In this commentary I point out that each of these views taken *separately* suggests a perspective on autism. It appears difficult to combine and to elaborate these perspectives.

1. *Stance* confirms at length the SI standpoint Dennett is well known for: To be predictable by the intentional stance is all there is to having beliefs. On most occasions we can effortlessly predict autistic behavior by treating autists as if they had beliefs and desires. If so, we must conclude on the strength of SI that autists are real believers. This gets the intentionalist in conflict with the current diagnosis of autism as a specific deficit that manifests itself in interaction with other people but does not affect perceptual beliefs. The conflict is of a peculiar kind because SI is unhelpful in clarifying what the disagreement is about and how far it extends. Moreover, the student of autism searches for internal causes, mechanisms and processes to be blamed for the deficit. SI declares that when it comes to deciding about the intentionality of the system none of that