

Re: "It's uncertain whether intelligence has any long term

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There is ample evidence that "intelligence" has had considerable

> > *long-term evolutionary advantage.*

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> *HT: For those species who possess it, intelligence (given an*

> *appropriate*

> *definition) has probably been a selective advantage.*

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> *GS: You might want to think about whether or not it makes sense to say*

> *that "intelligence" is a thing that can be possessed. You might also*

> *want to think about how it is that one has license to define*

> *colloquial terms as one wishes.*

>

HT: Words are your servants – not your masters.

GS: Tell it to Wittgenstein.

HT: In technical discourse, it

is routine to stipulate definitions for words. For instance, Einstein defined 'time' as what clocks measure and 'distance' as what measuring rods measure.

GS: Curiously, or not, so did colloquial speakers, although they probably had other things to say as well. The point is – and I might as well get right to it – that with colloquial mental terms like "intelligence" we must discover their "referents." If we simply state what "they" are (and might we not find that they are not things at all?) then we are merely superimposing our assumptions on what should be treated as something to be discovered. In psychology, and related disciplines, this has been disastrous. The notion that any concept is OK if it can be given an operational definition is the single reason that mainstream psychology is such a joke, and "evolutionary psychology" is just a handmaiden to cognitive psychology (which IS mainstream psychology now).

HT: What he concluded about time and distance relates only

to the definitions he stipulated, but he wasn't asserting that other definitions are not possible or valid.

GS: Yes. But these were straightforward definitions, and they were ones that were forced upon him and anyone before him that tried to build a clock. The definitions that psychologists, and others concerned with behavior, give for terms simply reflects their philosophical assumptions and these are, when it comes to behavior, a product of theism and animism.

HT: Without making it clear what your terms mean in a particular context, it is impossible to evaluate the correctness of statements like "intelligence has had considerable long-term evolutionary advantage" or "intelligence is a thing that can be possessed".

GS: Being clear is one thing, superimposing one's pre-scientific assumptions upon the subject matter is another. When we explicate ALL the usages of a term, we are beginning to analyze the term, and doing so forces one to be sensitive to the range of phenomena "designated" by the term.

HT: Of course, as a practical consideration, you should define your terms so that they are as close to their everyday intuitive meanings as possible so that people aren't misled about the applicability of your conclusions.

Philosophers who argue about what the 'correct' meaning of a word is, are completely wasting their time in my opinion.

GS: Well we are in agreement here, but I believe what you say here is incompatible with what you said earlier.

[snip]

>

> > *That is, it is evident if one has*
> > *some clue as to what is being called "intelligence." If one views it*
> > *as some "thing" that is measured as "G" and so forth, then one can*
> > *make the argument that there is no evidence. If, however, one views*
> > *"intelligence" as a murky, nonscientific, simplistic description of a*
> > *person's (or non-human animal's) behavioral repertoire, then it is*
> > *obvious that there is such evidence. In what sense? If one views the*
> > *behavioral repertoires of humans and non-human animals as products of*
> > *processes that are shared by a great many animals ? especially operant*
> > *conditioning ? the argument disappears. Operant conditioning (and*
> > *other common behavioral processes) CAN account for the complex*
> > *behavioral repertoires of humans (see Skinner's "Science and Human*
> > *Behavior" and "Verbal Behavior," and, no, Chomsky didn't) and the*
> > *process is obviously widespread in the animal kingdom. This is because*
> > *animals that can acquire new responses are frequently in a better*
> > *position than those that don't since the environment is always in some*

> > *flux.*

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> *HT: You've raised a lot of issues here.*

>

> *Chomsky's review of "Verbal Behavior" is a pretty thoroughgoing
> demolition of Skinner's position and was one of the most important
> events triggering the cognitive revolution against the old-school
> Behaviourism.*

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> *GS: No, it is widely regarded as "a pretty thoroughgoing demolition
> of Skinner's position," that does not make it so. It is correct,
> though, that it "was one of the most important events triggering the
> cognitive revolution against the old-school Behaviourism."
> Unfortunately, Skinner had already begun dismantling "old-school
> Behaviourism" 20 years earlier. The version of behaviorism that
> Chomsky attacked is not the version that Skinner advocated, despite
> Chomsky's numerous quotes. Anyone who has read both Verbal Behavior
> and Chomsky's "review" can see easily that Chomsky never read the
> book, and he admitted as much to Searle.*

>

HT: Something doesn't sound quite right about what you've said here especially when we put these two comments alongside one another: "Chomsky never read the book", "despite Chomsky's numerous quotes".

GS: Are you saying you can't quote from a book without actually reading it for comprehension? Don't be ridiculous. Simply making "textual" and "transcription" responses is not "reading."

[snip]

> *HT: Your comments about general learning mechanisms that you say we
> share*

> *with other animals run up against a critical problem – we are capable
> of things that other animals are not (language being the obvious
> example). This leads inevitably to the conclusion that humans possess
> different 'learning' devices.*

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> *GS: I don't think so. Natural selection propels some species along a
> path in which more and more of their behavior is not elicited like
> reflexes or "fixed-action patterns" etc. In primates, this trajectory
> has been followed to the extreme and when the vocal musculature became
> sensitive to its consequences, "language" arose. Put simply, the
> argument is that there is a continuum of, let's call it, "percent of
> repertoire that is not elicited" (i.e., a matter of degree) and that
> the extraordinary flexibility and utility of sounds produced by the
> vocal musculature made the emergence of "language" inevitable. Once
> "language" and culture emerge, much of what is extraordinary about
> humans follows, even though we are just a little "smarter" than other
> mammals, and the mechanisms are the same.*

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HT: Language is language whether it is spoken, written, signed or whatever.

GS: Yes, but it was speech that allowed true culture to emerge. Indeed, it is telling that writing didn't emerge for a very long time.

HT: I wasn't making a comment about speech which you seem to be conflating with language. Various ape language experiments have been performed using sign language and a mode of communication which involves pointing to lexigram symbols on a board.

GS: Thanks for the heads-up on that one, Huck. As someone pushing the view that "language" is a product of processes that we share with non-human animals don't you think I might be familiar with the ape-language stuff?

HT: Neither task exceeds the perceptual/motor abilities of apes in the way that speech does. Nevertheless, there are still extremely severe limitations in the communication systems that develop in terms of vocabulary size and basic grammar.

GS: Yes, of course.

HT: Brain damage can also affect language without inhibiting other aspects of intelligence and vice versa. This suggests that they are processed in different parts of the brain although conceivably using similar kinds of circuitry.

GS: No, it suggests that some of the circuitry is different. Some of it may very well be the same. This is somewhat orthogonal to my argument, however.

HT: Language is also acquired without apparently making much use of negative feedback about what is and isn't grammatical. Infants are much more likely to be corrected when they say something that is untrue rather than something that is ungrammatical so the feedback is extremely noisy.

GS: Yes, but selection has a way of amplifying slight differences in fitness.

HT: Infants also spontaneously use structures universal to language that they haven't heard in the input (most notable in the phenomenon of creolisation).

GS: But to say this is antithetical to an operant description is based on an incorrect understanding of the notion of operant response class. This is why people say that over-regularization of verb tense shows that a behavioristic interpretation is wrong. But it doesn't. This

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kind of generalization is a normal part of operant conditioning.

HT: In short, there is ample evidence that the ability to acquire language is within the range of phenotypic expression of human genomes, but not the genomes of other species and for reasons to do with cognitive abilities rather than perceptual/motor abilities.

GS: And just what are "cognitive" abilities? Animals show some of the same processes we see in the verbal behavior of humans, and the verbal behavior of humans can be explained in terms of these processes where humans show quantitative differences. My impression is that it has to do with how fast responses generalize, and how fast this process accelerates with continued exposure to contingencies that produce generalized response classes.

HT: There is a small amount of evidence of continuity between ape–language abilities and that of humans, but not enough to draw serious conclusions about the evolution of language. Apes never acquire exotic properties like case systems, subject–verb agreement, phrasal movement and embedding, relative clauses and so on. The comparative method is therefore limited in what it can tell us about where these abilities came from.

GS: It should be clear that I agree to some extent. As I said, there are quantitative differences between species, but it is not clear that the basic processes are not the same. And the "comparative method" has already produced the only viable technology for establishing any kind of language in people that don't acquire it under "normal circumstances," i.e., autistics, and it could be argued that the "comparative method" was successful in establishing ape–language. What do rats pressing levers, pigeons pecking keys, and apes pressing "lexigrams" have to do with each other? Think about that. Then, ask yourself how much you know about what goes on in operant laboratories, and how much you know about behaviorism as presented by behaviorists rather than their critics.

- > *HT: On your last point, 'learning' is not always an advantage. If what*
- > *is*
- > *learned is always the same thing, then it would pay to be equipped*
- > *with this knowledge/ability at birth or as soon as it can be*
- > *exploited. Having to learn this thing would mean a delay that could be*
- > *costly.*
- >
- > *GS: And this is exactly why many things are "hard-wired." But learning*
- > *has obviously been an advantage, as is evidenced by its widespread*
- > *representation in the animal kingdom.*

HT: A property that is widespread is not necessarily adaptive. Most animals fall to the ground if you drop them, for instance.

GS: Hmmmmm. I shall waste no time in starting to think about this. How's that for tact?

HT: An ability to adjust behaviour according to environmental conditions is present to varying extents in the animal kingdom. I don't know if it's really appropriate to say that it's widespread. Maybe among mammals and birds, but not really among say, reptiles and fish.

GS: Nonsense. It is probably a matter proportion, but I assure you that operant condition can be demonstrated in all vertebrates, and has been demonstrated in bees (von Frisch), octopi (look it up), drosophila, etc. etc. etc. And, of course, there're also habituation and classical conditioning. However, my own take is that spontaneous behavior is very primitive, and it is the presence of spontaneous behavior that makes operant conditioning possible.

HT: Let me ask you a question. Can you draw a principled distinction between 'learning' and other types of phenotypic plasticity such as muscle development (a function of exercise) and callus formation (a function of pressure on, or repeated rubbing against, skin)? Both of these developments are adaptive responses of the body to environmental input. Would you call these 'learning'? If not, why would you be any more inclined to group all 'learning' processes under one label? Is there some reason to suppose that the same mechanisms are involved in all cases of learning?

GS: The answer to this question is simple. Taking operant conditioning, we say that operant conditioning (let's just take positive reinforcement) occurs when 1.) there is a contingency of reinforcement, 2.) there is a resulting increase in the frequency of the response that enters into the contingency and, 3.) that the increase is because of the contingency. And, finally, the reason that I say that the mechanisms should be lumped together is because 70 years of research suggests that the processes are reliable and general within and between species (see the history of the experimental analysis of behavior). The generalization is not complete, of course, but so what?

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