

Re: Behavioral Genetics: A pseudo science or real scientific discipline

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- *From:* "Glen M. Sizemore" <gmsizemore2@xxxxxxxxxx>
 - *Date:* Sun, 4 Dec 2005 13:55:27 -0500 (EST)
-

<whitesickle@xxxxxxxx> wrote in message

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>

>>>From my past engagements with you I perceived you as something of a

> Skinnerian behaviorist, as someone who placed much more emphasis on

> operant conditioning and the environment. I think that is reflected

> here. The article I posted stated:

>

> During the late summer

> of 1999, a Princeton molecular biologist published the results of

> impressive research in which he enhanced the ability of mice to learn

> by inserting a gene that codes for a protein in brain cells known to be

>

> associated with memory. Because the experimental animals performed

> better than controls on a series of traditional tests of learning, the

> press dubbed this gene "the smart gene" and the "IQ gene," as if

> improved memory were the central, or even sole, criterion for defining

> intelligence.

>

>

> You replied, "There are a few mistakes here: first, and foremost,

> glossing over the

> subtleties of conditioning procedures as "memory" illustrates the

> fundamental conceptual muddle of mainstream psychology. It is not that

> the

> term "memory" would not fit here, it is that "memory" applies to

> virtually

> all psychological phenomena. This is why the "kinds" of memory have

> proliferated. This might not be too bad, but mainstream psychology has

> no

> interest, really, in the variables that are responsible for the few

> generalities they obtain in "memory research." So-called short-term

> memory

> is treated as sort of pre-existing characteristic, but there is good

> reason

> to believe that STM is a loose collection of response classes that are

> acquired when animals, human and otherwise, are exposed to delays

Re: Behavioral Genetics: A pseudo science or real scientific discipline

- > between
- > events correlated with other significant events and those significant
- > events. Animals learn to behave in extremely subtle ways that are
- > dependent
- > upon the particular setting and these responses, then, serve to
- > discriminatively control behavior when the delay is over and the
- > "opportunity to act" arises. Thus, STM is a product of more basic
- > conditioning processes and specific ontogenic circumstances are
- > necessary
- > for it to exist. The point here, which cannot be emphasized enough, is
- > that
- > "behavioral genetics" is applied within the context of a conceptual
- > muddle
- > that renders mainstream psychology virtually worthless."
- >
- > I have not researched the study but are you suggesting there was no
- > enhanced ability of mice to learn
- > by inserting a gene that codes for a protein in brain cells known to be
- >
- > associated with memory?

No, I am suggesting that the sentiment expressed by "as if improved memory were the central, or even sole, criterion for defining intelligence" is revealing. It reveals the conceptual muddle that characterizes virtually any discussion of topics that have anything to do with the behavior of animals, human or otherwise. First, to equate the effect of the gene manipulation as affecting "memory" is simple-minded. Second, thinking that a reasonable goal is "defining intelligence" is simple-minded. Even referring to "enhanced learning" reveals a conceptual muddle. The issue is "What is it that has been altered?" It isn't "memory" because "memory" is, essentially, a name given to a myriad of different complex processes. It ain't "intelligence" because "intelligence" is even less a "thing" than "memory." And the same goes for "learning." Even if we make a distinction between what used to be called "associative variables" and motivational processes, "learning" still embraces a large variety of interacting processes; there is the "frequency-increasing" effect of consequences, and the stimulus control exerted by the stimuli that are present when the response has particular consequences. Any time one has such contingencies, one also has dependencies between stimuli, and it is such dependencies that produce Pavlovian conditioning. And, of course, all the stimuli are being presented repeatedly, and this alone is a manipulation that produces habituation or sensitization. Which are affected? And, of course, motivational variables can't really be left out, especially given that "motivational operations" are far broader than food or water restriction. Schedules of food delivery, themselves alter the reinforcing efficacy of other consequences (or other responses, as a modern view suggests that it is advantageous to consider "consequences" as access to the opportunity to engage in behavior – i.e., access to eating reinforces lever-pressing, etc.) many of which are automatically embedded in any "test environment." Grooming, corner sniffing, rearing, etc. in rats, for example, all undergo temporally dynamic changes

Re: Behavioral Genetics: A pseudo science or real scientific discipline

and these processes, if altered, would likely impact the measured behavior. What is it that is altered by gene manipulation? Similarly, when a "genetic basis" is uncovered, which of the myriad of processes that are going on are really different? The issues here go, of course, far beyond "behavioral genetics" per se, but I have some more specific criticisms of that particular field that I haven't even gotten to yet. Anyway – to put the main point differently – behavior itself is so conceptualized so badly that "we" don't even know what we are trying to explain. This problem is everywhere.

- > Are you saying the STM of the mice is a product
- > of more basic conditioning processes and specific ontogenetic
- > circumstances necessary to exist and that the "IQ gene" merely
- > augmented that?
- >
- > You write, "This reveals a most fundamental problem. For most of
- > psychology
- > "behavioral measurement" is, ironically, not what it appears. Indeed,
- > the
- > "measurement of behavior" is viewed as a way to "operationalize"
- > unobservable entities ("personality traits") that are, then, viewed as
- > the
- > "real subject matter." It is sometimes difficult to tell what has been
- > actually measured and manipulated since authors more often refer to the
- >
- > alleged unobserved entities as the independent and dependent variables.
- >
- > Behavioral genetics is just a set of experimental and statistical
- > procedures
- > that, as I said, are embedded in this conceptual morass."
- >
- > Yup, I don't consider such behavioral measurement to be much different
- > from phrenology. You write regarding intelligence, "Or, perhaps, is
- > "intelligence" the phlogiston of psychology? No – that's
- > too much of a slur on phlogiston. Perhaps it would be better to ask if
- > "intelligence" is the vis anima of psychology." You are being very
- > sarcastic here but I think it is warranted. I still like Stephen
- > Hawking's quote, "It is uncertain whether intelligence has any long
- > term survival value. Bacteria do quite well without it."

I don't know exactly what position you are endorsing here. Obviously I would say that Hawking is a victim of the same misconceptions that I just described. "Intelligence" is not a "thing" that can be possessed. What exists are behavioral processes. There's no question that the behavioral processes that characterize bacteria are much simpler than those that characterize mice, monkeys, or man, but that doesn't mean that

Re: Behavioral Genetics: A pseudo science or real scientific discipline

"intelligence" is a useful scientific concept. This also doesn't mean, though, that the observed behavior said to require "intelligence" is unimportant or unaffected by natural selection. Behavior is fundamental to what animals are.

• **References:**

◆ **[Re: Behavioral Genetics: A pseudo science or real scientific discipline](#)**

◇ From: whitesickle@xxxxxxx

• Prev by Date: **[Re: evolutionary success of humans](#)**

• Next by Date: **[Re: Article: Precision Extinction – Eradicating a species when you want to isn't that easy](#)**

• Previous by thread: **[Re: Behavioral Genetics: A pseudo science or real scientific discipline](#)**

• Next by thread: **[Re: Behavioral Genetics: A pseudo science or real scientific discipline](#)**

• Index(es):

◆ **[Date](#)**

◆ **[Thread](#)**