Bees, Brains and Behaviour:

A Philosophical Essay In Theoretical Biology

by

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A thesis submitted to the Faculty of Graduate Studies in partial
Fulfilment of the requirements for the degree of

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Abstract:

In the introduction to the second edition of Karl von Frisch's popular monograph [1950] on honeybee behaviour, Donald Griffin concludes:

> a reluctance to become embroiled in metaphysics should not anesthetize our perceptions. Heretical as it may seem to many behavioral scientists, I am willing to entertain the thought that perhaps the bees know what they are doing [Griffin 1971, p. xiii].

In this thesis I will not be daunted by metaphysics. Rather, I will attempt to establish whether honeybee behaviour is best described by Griffin's cognitive theory, and whether this implies that it is reasonable to think "the bees know what they are doing".

Griffin has argued extensively that it is possible to learn whether non-human animals think consciously. He has founded and defended the recent and controversial scientific discipline of cognitive ethology, which attempts to make a scientific "analysis of the cognitive processes of non-human animals" [Griffin 1992, p. vii]. Cognitive Ethology seeks to combine and apply theory and method from biology, neurophysiology, and cognitive psychology to animal ethology. My analysis of bee behaviour will be made within the framework of cognitive ethology.

I will conclude that the dance language of the honeybees is intentional. I reject Griffin's strategy of conflating cognition with consciousness and self-consciousness. Therefore, although I think the best explanation of honeybee behaviour is a cognitive explanation, I do not conclude that honeybees are conscious or self-conscious.

Introduction

Introductory Remarks

In the introduction to the second edition of Karl von Frisch's popular monograph [1950] on honeybee behaviour, Donald Griffin concludes:

> a reluctance to become embroiled in metaphysics should not anesthetize our perceptions. Heretical as it may seem to many behavioral scientists, I am willing to entertain the thought that perhaps the bees know what they are doing [Griffin 1971, p. xiii].

In this thesis I will not be daunted by metaphysics. Rather, this thesis will be an attempt to establish whether honeybee behaviour is best described by Griffin's cognitive theory, and whether this implies that it is reasonable to think "the bees know what they are doing".

Griffin, best known for his discovery that bats navigate by echolocation (sonar), has argued extensively for the view that it is possible to learn whether non-human animals think consciously. He has founded and defended the recent and controversial scientific discipline of cognitive ethology, which attempts to make a scientific "analysis of the cognitive processes of non-human animals" [Griffin 1992, p. vii]. My analysis of bee behaviour will be made within the framework of cognitive ethology.

Cognitive ethology seeks to combine and apply theory and method from biology, neuro-physiology, and cognitive psychology to animal ethology. As part of my evaluation of the claim that honeybees "know what they are doing", I will argue that there are some serious difficulties in any attempt to make a systematic study of animal psychology; but that the obstacles are surmountable if confronted. However, there will be a methodological and philosophical cost for this success. I will conclude that a

philosophical study of the minds of humans and animals is best conducted within the framework of evolutionary theory of Darwin, and not the dominant philosophical framework provided by Descartes. I will argue that the various considerations taken by Griffin and his colleagues to support the view that animals have important cognitive experiences conflict and so seriously undermine the prospects for cognitive ethology.

Thesis Structure

In making a case for cognitive ethology, Griffin takes on two largely separate tasks. Firstly, he must reject the idea that non-human animals differ in kind and not just in degree from humans; I call this idea the apartness thesis and consider the case against it in chapter one. Secondly, a positive case is required which makes arguments for the *prima facie* plausibility of cognitive ethology. One main problem is that this second task is actually made more difficult once the apartness thesis is rejected. I consider the general case for the plausibility of cognitive ethology in chapter two. In chapter three I consider the case of honeybee behaviour within the program of cognitive ethology as developed in the first two chapters.

Acknowledgements

Contents

Chapter One

The Apartness Thesis

Rejecting The Apartness Thesis

The apartness thesis is the view that animals differ importantly from humans, and that certain attributes are unique to humans. Establishing that an animal like the honeybee might have significant cognitive experiences requires first rejecting the apartness thesis, since it would otherwise form the basis for criticism of such a conclusion.

Introduction to the Apartness Thesis

In the intellectual history of western civilisation "there is a long list of human attributes -- speech and writing, art and science, savagery and civilization" [Walker 1983, p. 1] which have been considered unique to humans. Darwin compiled a list of such attributes:

> I formerly made a collection of above a score of such aphorisms, but they are almost worthless, as their wide difference and number prove the difficulty, if not the impossibility of the attempt. It has been asserted that man alone is capable of progressive improvement; that he alone makes use of tools or fire, domesticates other animals, or possesses property; that no animal has the power of abstraction, or of forming general concepts, is self-conscious and comprehends itself; that no animal employs language; that man alone has a sense of beauty, is liable to caprice, has the feeling of gratitude, mystery, &c; believes in God, or is endowed with a conscience [Darwin 1871, p. 79].

Darwin wisely chose to avoid the task of dealing with each and every attribute, although he chose to comment on those that seemed most important. I will follow a similar strategy.

History of the Apartness Thesis

A formal statement of the apartness thesis might be that there is at least one feature that is privileged as a mark of human-ness. Different versions of the apartness thesis select different features as privileged. For instance, according to the linguistic version of the apartness thesis language is privileged; only humans have language. The contrary view, that animals do not differ in kind from humans, is known as continualism (usually Darwinian continualism); according to which animals differ only in degree. Apart from the psychological issues treated in the rest of this chapter, Darwinian continualism is beyond reasonable doubt. Darwin and Huxley triumphed except in the domain of the mental; the only versions of the apartness thesis that are worth considering concern psychological states and processes.

Jonathan Bennett gives a strong version of the apartness thesis:

> It is common knowledge that there is a very large difference between the level of intellectual ability of humans and that of all other terrestrial creatures. It is commonly believed that this difference is in some important way one of kind rather than of degree: that between a genius and a stupid man there is a smooth slide while between a stupid man and an ape there is a sharp drop, not just in the sense that there are no creatures intellectually half-way between apes and stupid men, but in the sense that there could not be such creatures. [Bennett 1964, p. 4].

Bennett's strong version of the apartness thesis differs from the weaker version usually encountered in the literature in that it makes claims about the possible, as well as the actual, differences between humans and animals. A weaker version of the apartness thesis might admit it is possible for there to be creatures intermediate between humans and animals, but deny such creatures exist. This distinction is important because the weak version of the apartness thesis admits the plausibility, but not the actuality, of Darwinian continualism. The strong version rejects the possibility of continualism.

In general, any apartness principle P will point to some feature f as a privileged feature. According to P, all and only humans will have feature f, and every non-human animal lacks f. If, contrary to P, feature f is found in some animal, we cannot immediately conclude 'the animal is human-like in virtue of having f under P' since it might be equally plausible to revise or reject principle P. Much of cognitive ethology seems to be finding that animals have more of the relevant f's than we thought, but this may be only so much the worse for any particular apartness thesis principle P, rather than the stronger claim that animals cannot have f.

Descartes Versus Animal Cognitivism

Perhaps René Descartes made the strongest enunciation of the apartness thesis. Contrary to modern Darwinism and Aristotelian gradualism, Descartes thought that animals were qualitatively different, and not just quantitatively different from humans [Walker 1983, p. 3-4]. Descartes thought that "brutes not only have a smaller degree of reason than men, but are wholly lacking in it" [Descartes 1637, p. 44].

Descartes' view of animal psychology is not entirely surprising; however, his analysis of human psychology conflicts with his general strategy of explanation. Excepting his treatment of human psychology, Descartes was a reductionist; he explained the complex in terms of the simple. For instance, he tried to reduce math to physics, and succeeded in reducing geometry to algebra [Walker 1983, p. 3]. What is actually surprising is that an otherwise complete materialist and reductionist should become the champion of dualism, where human minds and matter are on opposite sides of a fundamental chasm.

Descartes writes: "there is none more likely to turn weak characters from the strait way of virtue than the supposition that the soul of brutes must be of the same nature as ours" [Descartes 1637, p. 44]. Descartes general view is that there are two sorts of entities: physical non-thinking things (matter), and non-physical thinking things (minds), and that humans are composed of both sorts, whereas animals and other physical things are composed only of matter [Descartes 1642, p. 121]. Since Descartes holds that animals have no non-physical aspect, it follows trivially that animals don't or can't think (since Descartes holds thinking to be an essentially mental activity).

Descartes offers a three-step argument for his view that animals are completely physical. The first step of his argument is an analogy to animals from the intricate clockwork models popular in his day. These models of animals behaved approximately like their living counterparts, but Descartes takes it to be obvious that the models are entirely physical [Walker 1983, p. 5]. Since the physical models can produce the appropriate behaviour without minds, so can animals.

The second part of Descartes' argument for psychological nihilism with respect to animals is based in his own anatomical observations. Descartes was a leader in anatomical research and had made detailed observations of animal anatomy. All the behaviour and workings of the animal seemed accounted for by the observed physical systems. All the needed mechanisms for animal behaviour appeared to be present in the circulatory system, nervous system, and so on [Walker 1983, p. 5; Bennett 1964, p. 16]. In the case of the human body, Descartes knew that the material elements of the physical body were inadequate to explain the obvious behavioural complexities:

> Examining the functions that might result in such a body [animal body], what I found were precisely those that may occur in us unconsciously, without any cooperation of the soul, that is to say of the element distinct from the body of which I said above that its nature is merely to be conscious; the very operations in which irrational animals resemble us; but I could find none of the operations that depend on consciousness and are alone proper to us as men; whereas I could find a place for these on the further supposition that God created a rational soul, and joined it to the body in a way that I described [Descartes 1637, p 41].

This argument seems to depend on the premise that God would not give animals 'rational souls'; I am not aware of any argument Descartes might have given in defence of such a premise.

The third step of Descartes' argument was a thought experiment. Descartes thought that, at least in principle, an animal's behaviour could be perfectly mimicked by an appropriate model [Descartes 1637, p. 43]. On the other hand, a humans' outward behaviour could be mimicked without the model having a corresponding *cogito ergo sum* (I think, therefore I am) intuition. Descartes concluded that there was no explanatory reason to attribute mentality to animals.

Descartes argues further that a model human would not have the creative language ability to generate meaningful conversation.[1] He points out that humans who don't have speech ability (deaf and dumb) manage to communicate, while animals that do have speech ability (parrots etc.), do not [Descartes 1637, p. 42]. Descartes takes this to indicate that "something very profound is lacking in animals" [Walker 1983, p. 9]. Bennett's criticism of Descartes 3-step argument is that Descartes uses the justification of minds in humans as an explanatory bridge which opens the way to psychological scepticism when joined with the problem of other minds [Bennett 1964, p. 16]. That is,

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[1] The modern name for this argument is the Turing Test, after Alan Turing's 1952 article in Mind.

treating the existence of minds as a hypothesis to be confirmed or rejected invites scepticism predicated on the difficulty of providing adequate confirmation. Bennett thinks that Descartes should have been more charitable in the case of animals, even if all the observed behaviour could be explained in terms of physical processes (step 2), it is still possible that animals have a psychology similar to humans.

After Descartes

After Descartes, the controversy over animal psychology remains muted until Darwin. However, other pre-Darwinian philosophers disputed Descartes' claims about animal minds, most notably John Locke and David Hume. Locke accepted a different version of the apartness thesis, claiming that "brutes abstract not, yet are not bare machines" [Locke 1690, p. 208]. Locke rejected Descartes' version of the apartness thesis, and also Descartes' analysis of animals as automatons. Since Locke thought humans were capable of abstract thought, it seems as if abstraction is a crucial difference between man and animals [Walker 1983, p. 23]. Since abstraction is an ability, and not a fundamental entity, Locke admits "if one's mind deteriorates in old age, then one is no better than a cockle" [Locke 1690]. While Walker claims that Locke's view is empirically false [Walker 1983, p. 24] (animals are capable of abstraction), it is interesting to note that Locke recognizes and admits the doubly slippery slope his position commits him to. On his view, anything that can abstract satisfies his apartness thesis, anything that cannot abstract (including biological humans) is excluded; 'human', defined by Locke's apartness thesis, is not co-extensive with the biological category.

Hume clearly rejected the apartness thesis. Hume agreed with Aristotle that animal inferiority is a matter of degree. Unlike Aristotle, Hume's epistemological scepticism demotes the rationality that Aristotle thought distinguished humans.

> Animals, therefore, are not guided in these inferences by reasoning: Neither are children: Neither are the generality of mankind, in their ordinary actions and conclusions: Neither are philosophers themselves, who, in all the active parts of life, are, in the main, the same with the vulgar, and are governed by the same maxims [Hume 1777, pp. 116-117].

However great our cognitive abilities might seem, we humans struggle through life depending on the "experimental reasoning... which we possess in common with beasts, and on which the whole conduct of life depends" [Hume 1777].

Hume explicitly advocates the continuity thesis in The Enquiries where he suggests that human psychology is to animal psychology as human anatomy is to animal anatomy; theories about human psychology would be bolstered by confirmation of the same theories in animal psychology [Hume 1739, p. 375]. In general, Hume supposes that animals differ from humans in just the ways humans differ from each other: "no truth appears to me more evident, than that beasts are endow'd with thought and reason as well as men" [Hume 1739, p.226].

Darwin and Animal Cognitivism

Darwin's opinion of the cognitive abilities of animals is that "the difference in mind between man and the higher animals, great as it is, certainly is one of degree and not of kind" [Darwin, 1871, p. 193]. Darwin did not write extensively on the psychology of animals, but left the task of defending the continuity thesis in the psychological arena to his protégé, George Romanes, whose "publication of Animal Intelligence in 1882 marks the first attempt at a scientific analysis of animal intelligence" [Hodos 1982, p.

35]. Romanes began to publish work based largely on Darwin's notes shortly after Darwin's death, but, for a number of reasons, his views on animal psychology were poorly received.

Romanes' work was poorly received partly because he became caught in a backlash against the excesses of introspective psychology. Introspective psychology had been a particular and early method of psychological research that concentrated on the introspective reports of trained subjects (often the researchers themselves). This method was never able to make a good case for the reports of their specially trained subjects not simply being a product of the training regimen (or the fantasies of the researchers). More importantly, there was an *ad hoc* flavour to the theories developed out of this research; with no clear theory or hypothesis to test they were making it up as they went along. A research community that was suspicious of any work in psychology was hostile to Romanes' work on the psychology of animals.

Another reason for Romanes' failure was that his own work was only marginally scientific, relying heavily on anecdotal observations and implausible data [Griffin 1984, p. 41]. This style of work might have been acceptable earlier, but Darwin's own contribution to biology changed that. Biology now had a sound theory (evolution), and developed methods, based on dissection and classification. Research in biology after Darwin had to meet stringent new criteria. Romanes didn't meet the new standards.

Two famous researchers who rebuked Romanes work were Lloyd Morgan and Thorndike, who warned against the errors of overestimating the mental powers of animals. Romanes clearly violated what became Lloyd Morgan's 'canon':

"we should not ... explain any instance of animal behavior as the outcome of higher mental processes, if it can fairly be interpreted as the outcome of mental processes which stand lower in the order of mental development".

Romanes' failure to present his (and Darwin's) views in a positive light meant that while Darwin's general theory of evolution (with its explicit continuity) was widely accepted by the beginning of the present century, his views about animal psychology (that animal psychology is continuous with human psychology) were not. Instead, the minimal interpretation approach advocated by Lloyd Morgan and Thorndike became the doctrine of biology and animal psychology.

Behaviourism and the Apartness Thesis

In any discussion of behaviourism, it is important to distinguish between three sorts of behaviourism, which I shall call logical behaviourism, philosophical behaviourism and methodological behaviourism. Methodological behaviourism is the view that, regardless of ontology, psychological research must be confined to observable behaviour. Philosophical behaviourism accepts methodological behaviourism, and further denies the ontology of mental phenomena. Logical behaviourism accepts philosophical behaviourism; further, logical behaviourism gives a specific argument in favour of philosophical behaviourism. All logical behaviourists are philosophical behaviourists, but not the reverse. All philosophical behaviourists are methodological behaviourists, but not the reverse.

Because of a philosophical movement called logical positivism, behaviourism became the standard scientific view in the early decades of the twentieth century. According to logical positivism, any proposition that could not be settled empirically or analytically, at least in principle, was nonsense. The logical positivists agreed that

speculation about psychology in general, and *a fortiori* animal psychology, could not be settled empirically and hence was not a legitimate subject for scientific inquiry. They proposed logical behaviourism where a proper study of psychology analysed mental predicates as connoting or denoting nothing but behavioural constructs. On this view, psychology should be confined to behaviour and stimuli, and not postulate "the ghost in the machine" [Ryle 1949].

Heavily influenced by logical positivism, the scientific community adopted methodological behaviourism as a research strategy. Psychology and animal research developed an entire methodological program which didn't allow psychological explanations except in terms of behaviour and input stimuli; no "inner workings" of the mind were posited [Skinner 1938]. Although logical positivism was rejected by the philosophical community in the 1950's, methodological behaviourism persisted in the sciences until the quite recent "cognitive revolution" [Baars 1986].

The cognitive revolution in psychology perhaps began with Noam Chomsky's negative review of B.F. Skinner's Linguistic Behaviour [Baars 1986, p. 186]. Skinner was a founder of, and the leading researcher in, behaviourist human psychology, who thought that linguistic behaviour was the most important sort of behaviour: "the only differences... between the behaviour of rat and man lie in the field of verbal behaviour" [Skinner 1938, p. 442]. Skinner required an account of verbal behaviour to give an adequate account of all human behaviour. Chomsky's public and forceful rejection of this work was the first crack in behaviourist fortifications. Skinner had been attempting to explain language abilities entirely in terms of behaviour and stimuli and response; his

failure indicated that methodological behaviourism was theoretically inadequate to explain human behaviour.

The alternative explanation of human behaviour was to posit some 'inner workings of the mind' that were not empirically observable. This was done within an entirely materialist context, so these 'inner workings' were taken to be the product of brain function, rather than anything immaterial, as Descartes had thought. According to cognitive theory, it is sometimes necessary to suppose that something mediates between stimulus and response in human and animal behaviour.

Cognitive psychology, as it became known, is indeterminate between the apartness thesis and Darwinian continuity. If both human and animal behaviour require cognitive explanations, then the continuity thesis will be confirmed. If cognitive explanations are necessary for explaining human behaviour but unnecessary for explaining animal behaviour, then the apartness thesis will be confirmed.

There was no immediate attempt to apply the cognitive theory developed in the context of human psychological research to animal psychology. Rather, animal researchers remained committed to the minimalist interpretation approach of Lloyd Morgan and Thorndike. This must have been partly because the cognitive revolution in human psychology was heavily predicated on linguistics, and animals were thought to be incapable of language.

Rejecting the Apartness Thesis

According to Griffin, research on, or assuming, animal cognition, is stigmatized by the biological research community; this implicit censure has made for a hostile research environment as described by Kuhn [1960]. Griffin has called the circumstances

that result from this censure the catch-22 objection to animal cognitivism. Griffin takes upon himself the task of freeing his research community from this restrictive sentiment by rejecting the apartness thesis.

Discussion of Particular Versions of the Apartness Thesis

A consideration of some historically important versions of the apartness thesis comprises the rest of this chapter. Recall that Darwin's list of features considered as possible candidates for distinguishing humans from non-human animals was long and indefinite. I do not propose to refute every version of the apartness thesis; indeed that would be impossible. I do intend to discuss those versions of which have been used in premature promotions of animal cognitivism.

Animal Language

There is clearly a version of the apartness thesis which is predicated on language. According to the linguistic apartness thesis, language use is at least a crucial symptom of the difference in kind between animals and humans. Such luminaries as Descartes, Chomsky, Davidson, and Bennett have advocated the linguistic apartness thesis. MacPhail defends a view where the difference between humans and animals is merely one of having or not having a language to assist in manipulating ideas. MacPhail [1985] optimistically claims that "language is the difference between us and them, and who 'us' is, and who 'them' are, is a matter of research, largely completed, of language tests".

Descartes' Linguistic Apartness Thesis

Descartes considered language part of the important difference between animals and humans. Recall that Descartes considered it important that while humans without

speech can make themselves understood, animals capable of mimicking the sounds of speech cannot communicate at all:

> For it is a very remarkable thing that there are no men so dull and stupid, not even lunatics, that they cannot arrange various words and form a sentence to make their thoughts understood; but no other animal, however perfect and well bred, can do the like. This does not come from their lacking the organs; for magpies and parrots can utter words like ourselves, and yet they cannot talk like us, that is, with any sign of being aware of what they say. Whereas men born deaf-mutes, and thus devoid of the organs that others use for speech... usually invent for themselves signs by which they make themselves understood... [Descartes 1637, p. 42].

Descartes took this as evidence that animals are "wholly lacking" in "reason" since:

> It may be seen that a very small degree of reason is needed in order to be able to talk; and in view of the inequality that occurs among animals of the same species, as among men, and of the fact that some are easier to train than others, it is incredible that a monkey or parrot who was one of the most perfect members of his species should not be comparable in this regard to one of the stupidest children or at least to a child with a diseased brain... [Descartes 1637, p. 42-43].

By this line of reasoning, Descartes concluded that the lack of language in animals is certain evidence that they are incapable of reason, and so different in kind from humans.

Chomsky's Linguistic Apartness Thesis

Descartes' linguistic apartness thesis has been taken up by Chomsky who claimed "the word is the sole sign and certain mark of the presence of thought" [Chomsky 1966]. Chomsky used language to prove that the parsimonious proofs considered the chief advantage of behaviourism could never, in principle, be adequate explanations of linguistic behaviour [Baars 1986, p. 177].

Chomsky's analysis of language concluded that the rules of language were too complex to be learned by example, as proposed by Skinner. Using the mathematical theory of computation, Chomsky proved that the only machine that could adequately

manifest language was a Von Neuman machine, and according to Gödel's theorem, a Von Neuman machine is the equal to any machine whatsoever, no matter how complex. Hence all theories of linguistic behaviour must be mathematically maximum theories [Baars 1986, p. 177]; "Language is a system that makes infinite use of finite means" [Chomsky 1987].

Chomsky concluded that the infinite complexity of language use could not be learned by example, so the behaviourist account of linguistic behaviour was false. According to Chomsky, a cognitive account could provide a finite set of general instructions which could operate recursively. Chomsky's results mean that the behaviour of any animal that exhibits genuinely linguistic behaviour would be better explained by cognitive theory than by behaviourism.

Communication and Language

It is clear that animals communicate with each other, and with humans; but not all communication is linguistic. "A dog accompanied by a particular pungent odour communicates to us the fact that it has accosted a skunk. ...yet we are not tempted to suppose ... the dog is using language" [Hiel 1982, p. 400]. The important difference between mere communication and linguistic communication is that in the latter, *ceterus paribus*, there is a relationship between the content of the message and the intent of the communicators. This relationship philosophers call intentionality.

Intentionality

Philosophers use 'intentional' as introduced by Brentano [1874] to describe things that have meaningful content. The standard account of intentionality is that provided by Chisholm [1957], where he writes:

Psychological phenomena, according to Brentano, are characterised "by what the scholastics of the Middle Ages referred to as the intentional (also the mental) inexistence of the object... This "intentional inexistence, " Brentano added, is peculiar to what is psychical; things which are merely physical show nothing like it.

The phenomena most clearly illustrating the concept of "intentional inexistence" are what are sometimes called psychological attitudes; for example, desiring, hoping, wishing, seeking, believing, and assuming.

Clearly, mental phenomena are intentional. The philosophically difficult question is whether all intentional phenomena are mental. Amongst philosophers who think that intentionality is an important feature of the world, most think that language use entails intentionality; it is not so clear if intentionality entails language [Allen & Bekoff, 1997].

An important feature of Intentionality is "aboutness"; that is, the content of something intentional is about something else. For instance, if I say 'meet me at 10:00', it seems intuitive that I have certain intentions regarding a rendezvous at 10:00. The contents of my intentions regarding my rendezvous are about my rendezvous. However, the nature of the relation between the content of my intentions and my rendezvous is extremely complex. After all, the content of my intention is about something that has not happened yet, might not happen in the way I foresee, or might not happen at all.

Without cognitive theory, it is mysterious how my intentions regarding a 10:00 rendezvous can be explained. Briefly, the cognitive explanation of intentionality is just that there is an internal cognitive state that manifests the content of an intentional item. This cognitive state stands in relation to that which the content of the state is about. Someone who takes this view will conclude that if animals do manifest intentions, then only cognitive theory can adequately explain animal behaviour. Clearly, some animals exhibit behaviour that suggests that they have intentions. Dennett has described the behaviour of lions stalking prey which he observed as clear indication of their having

intentionality; the only explanation Dennett thought plausible for the lions behaviour was that they 'knew what they were doing' [Dennett 1982].

We will later see that Bennett's account of intentionality is given in terms of rules. Bennett explains the aboutness of intentionality with an account of how intentional content is rule-guided, and not merely regular. Bennett gives this account because behaviour that is regular cannot explain the complex relationship between intentional content, and whatever that content is about.

As will soon be shown, robust, or inflationary, accounts of intentionality assume that all intentional phenomena are mental. Deflationary accounts of intentionality assume that some intentional phenomena are not mental. These naturalised accounts of intentionality can account for the intentionality of a honeybee's cognitive maps, but also attribute intentionality to thermostats and trees. Robust accounts of intentionality have stricter requirements for attributions of intentionality. However, robust accounts are less widely accepted in the philosophical literature. As Allen [1997] describes, there are three ways to integrate intentionality into cognitive ethology.

First, one could accept a deflationary account of intentionality, and accept that thermostats and trees are intentional in just the way that humans are. The most popular version of this strategy would be to also accept an eliminativist account of mentality. Like Allen, I think this option is untenable; there is an important difference between thermostats and trees on the one hand, and humans (at least) on the other hand. Leaving this difference unexplained results in a profoundly unsatisfying account of intentionality.

The second strategy available in adopting a theory of intentionality would be to accept that thermostats and trees are intentional, but provide some account of mentality

that accounts for the difference between trees and humans. In short, this deflationary strategy assumes that not all intentional phenomena are mental, but provides an account of the difference between mental and non-mental intentionality. Bennett takes this strategy in Rationality, where he seeks an account of how intentional honeybees are not rational.

The third strategy available is to accept an inflationary account of intentionality that accounts for the difference between trees and humans without appeal to any other concepts. This strategy assumes that all intentional phenomena are mental, and that the difference between trees and humans is that trees are not intentional. Searle [1992] has vigorously advocated this strategy.

Of these strategies, Allen [1997] thinks the second and third are equivalent strategies. Both approaches assume that something else, besides deflationary intentionality, is required to provide a full account of mentality.

Deflationary Intentionality

Some philosophers think that intentionality is mundane and trivial in humans and animals. For instance Dretske writes: "it soon becomes clear that intentionality, rather than becoming a 'mark of the mental', is a pervasive feature of all reality--mental and physical" [Dretske 1980, p. 356].[2] According to this naturalist view of intentionality, intentional is just another name for the regularities of life. On this view, both trees and humans are intentional because the tree and I are manifesting our biological destinies.

The deflationary view of intentionality, such as Dretske's triviality position, relies on a naturalist account of intentions. According to this view, there is no significant

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[2] It is not clear if Dretske continues to hold this view. His statement of the triviality problem continues however, to be standard and forceful.

difference between 'desiring to do x' and 'the high probability of x'. Since trees usually grow towards light, there is an intentional relationship between the tree and the light.

I think this view is refuted simply by observing that there is an important difference between the things that I desire to do, and the things that I am likely to do, but don't desire to do. It is highly probable that I will slip on ice this winter, but in fact I desire to do nothing of the kind; indeed at times I make great efforts to avoid slipping on ice.[3] The view that intentionality is as commonplace as probability relations does not offer an explanation of intentionality satisfying to anyone proposing to study cognition in humans or animals.

The deflationary account of intentionality does provide us with an explanation of how something might merely appear to be intentional. Thus, I will show how Bennett distinguishes between genuine and apparent linguistic behaviour; and Searle describes "as-if" and "derived" intentionality [Searle 1992]. One way to identify genuine intentionality might be by looking for semantic opacity.

Language, Intentionality and Semantic Opacity

Donald Davidson has argued that attributing cognitive states and processes to animals that do not use language is hopelessly complex. His argument turns on W.V.O Quine's observation that intentional contexts are semantically opaque [Quine 1960]. Quine found that while generally co-extensive terms are intersubstitutable *salva veritate*[4], this is not the case in intentional contexts. For instance, when 'the discoverer of bat sonar' is substituted for 'Griffin' in 'Griffin is the father of cognitive ethology', the truth value of the sentence does not change since Griffin is the discoverer of bat sonar;

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[3] Since writing this, I have slipped on ice.

however, when the same substitution is attempted in 'Jack believes that Griffin is the father of cognitive ethology' it is not so obvious that the truth value should remain the same. If Jack is ignorant of Griffin's discovery that bats navigate by echolocation, then it is false that 'Jack believes that the discoverer of bat sonar is the father of cognitive ethology' even though it is true that 'Jack believes that Griffin is the father of cognitive ethology'; in this context, the co-extensive terms ('Griffin' and 'the discoverer of bat sonar') are not intersubstitutable, *salva veritate* [Martin 1987, p. 193-194]. Quine called sentences where co-extensional terms are not intersubstitutable cases of semantic opacity (as opposed to transparency).

Davidson thinks we can tell if an animal is intentional if our belief attributions to the animal are semantically opaque. Thus, while we correctly attribute the belief to our dog that 'the cat went up the oak tree', we don't automatically attribute to our dog the belief that 'the cat went up the oldest tree in sight' simply because the oak tree happens to be the oldest tree in sight.[5] It seems to matter whether the dog knows that the oak tree is the oldest tree in sight. However, this creates a problem, for we will never know if the dog knows whether the oak tree is the oldest tree in sight:

> If we really can ascribe single beliefs to a dog, we must be able to imagine how we would decide whether the dog has many other beliefs of the kind necessary for making sense of the first. It seems to me that no matter where we start, we very soon come to beliefs such that we have no idea at all how to tell whether a dog has them, and yet such that without them, our confident first attribution looks shaky [Davidson 1982].[6]

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[4] In philosophical use, "keeping the same truth value".

[5] This example is Davidson's (1982, p. 320).

[6] Stich provides a similar example in which his dog is unable to discriminate between squirrels and other squirrel-like objects.

According to Davidson, we are correctly attributing semantic opacity, and thus intentionality, to the dog if the dog has many other beliefs that have co-extensional terms that are not intersubstitutable, *salva veritate*. Thus, it seems we can only attribute intentionality to the dog if it is false, for instance, that the dog believes that 'the cat went up the oldest tree in sight'. If we did not think it false that the dog believes that 'the cat went up the oldest tree in sight', then we would be attributing semantically transparent beliefs to the dog. According to Davidson, semantically transparent beliefs are no beliefs at all, since they are not intentional. Davidson thinks having to think it false that the dog believes that 'the cat went up the oldest tree in sight' is a ridiculous result; he argues "it does not seem possible to distinguish between quite different things the dog might be said to believe" [Davidson 1982, p. 320]. Furthermore, the conclusion that the dog does, or does not, have beliefs about the ages of trees, etc. cannot be justified merely on the evidence of the dog's behaviour towards the cat and the tree.

Davidson admits that sense might be made of the belief sets of animals, although he expresses scepticism that this is a feasible task. However, he infers from the preceding considerations regarding semantic opacity that if an animal has one belief, it must have a number of beliefs, accordingly:

> it is clear that a very complex pattern of behavior must be observed to justify the attribution of a single thought. Or, more accurately, there has to be good reason to believe there is such a complex pattern of behavior. And unless there is actually such a complex pattern of behavior, there is no thought [Davidson 1982].

Davidson now proceeds to defend the view that the only pattern of behaviour complex enough to justify attribution of thoughts, is linguistic behaviour.

Davidson gives a two step argument for this position:

First, I argue that in order to have a belief, it is necessary to have the concept of belief. Secondly, I argue that in order to have the concept of belief one must have language [Davidson 1982].

Davidson justifies his first premise by arguing that intentionality is really only correctly attributed on the basis of semantic opacity if the creature in question could recognise the case as one of semantic opacity. Attributing to the dog the belief that 'the cat went up the oak tree' and not the belief that 'the cat went up the oldest tree in sight' requires the dog's behaviour to show that it understands that believing 'the cat went up the oak tree' doesn't imply that it believes that 'the cat went up the oldest tree in sight'.

Davidson's reasons for this line of thinking remain somewhat mysterious. Fischer thinks that the incompleteness of Davidson's argument stems from the invalidity of the inference from "beliefs that arise in connection with assertion, to the claim that belief arises only in this connection" [Fischer 1987]. Davidson offers an analogy to the concept of surprise as a rhetorical aid:

> Suppose I believe there is a coin in my pocket. I empty my pocket and find no coin. I am surprised. Clearly enough I could not be surprised if I did not have beliefs in the first place... It is not enough that I first believe there is a coin in my pocket, and after emptying my pocket I no longer have this belief. Surprise requires that I be aware of a contrast between what I did believe and what I come to believe. Such awareness, however, is a belief about a belief [Davidson 1982].

Since Davidson assumes that "surprise about some things is a necessary and sufficient condition of though(t) (sic) in general" [Davidson 1982, p. 326], it follows that it is a necessary and sufficient condition of thought that one can have a belief about a belief.[7] Davidson offers a similar analogy in a different setting: "someone cannot have a belief unless he understands the possibility of being mistaken, and this requires grasping the

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[7] Davidson doesn't seem to consider whether animals can have 'beliefs' without 'thought', as his view seems to imply; after all one can imagine on his view an animal incapable of being surprised, but one which has beliefs.

contrast between truth and error - true belief and false belief" [Davidson 1974, p. 367].

Thus, Davidson seeks to establish the first step of his argument.[8]

The first premise of Davidson's argument is not as obvious as he seems to think. Descartes, Griffin, and I agree that the first premise is simply false. Descartes [1637, p. 25] writes: "for the mental act of believing a thing is different from the act of knowing that one believes it; and the one act often occurs without the other. Griffin [1992, p. 122] thinks "that it seems reasonable to infer" that animals "experience disappointment, annoyance, or other subjective emotions when their expectations are not fulfilled". While I agree that beliefs are essentially revisable, I don't see how an inability to understand that beliefs are revisable would prevent one from having beliefs.

Even Davidson admits that the second step of his argument is largely unfulfilled. He suspects that only a communicator (he slips from 'language' to 'communication' at this point) would have command of the subjective-objective contrast he believes is inherent in having a belief about a belief [Davidson 1982, p. 327]. In favour of this suspicion he offers this analogy:

> If I were bolted to the earth I would have no way of determining the distance from me of many objects... Not being bolted down I am free to triangulate. Our sense of objectivity is the consequence of another sort of triangulation, one that requires two creatures... the fact that they share a concept of truth alone makes sense of the claim that they have beliefs, that they are able to assign objects a place in the public world [Davidson 1982, p. 327].

Presumably, 'triangulating' and 'truth' somehow emphasis the difference between beliefs and raw experience.

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[8] We will later see that this suggestion is not unique.

Although lacking the support of argument, Davidson's view that language is necessary for cognition is supported by the Sapir-Whorf hypothesis. According to the Sapir-Whorf hypothesis, thought is post-linguistic because language differences are reflected in thought and reason. If thought were pre-linguistic, then language should not have this effect on thought. According to this line of reasoning, if language influences thought language precedes thought. However, other empirical work on the matter conflicts: for instance, Piaget denies that language is prior to cognition [Walker 1983, p. 95].

Ultimately, Davidson has little more to offer than his intuitions that:

- D1. Language is the only behaviour complex enough to justify belief attributions

- D2. Semantic opacity must be apparent to the creature in question to justify attributing intentionality to that creature.

- D3. Surprise is necessary and sufficient for thought.

- D4. Having a belief about a belief requires the concept of objective truth.

- D5. Semantic opacity is the hallmark of intentionality.

These intuitions often appear in other guises. D1 presupposes a certain result to the Turing test. I suggest that D2 is a consequence of over intellectualising the problem, but represents an important version of the apartness thesis; that what sets humans apart is an ability to have second order cognitive functions, such as having a belief about a belief. D3 and D4 suggest that intentionality is a function of second-order beliefs; D5 is a significant contribution to the study of animal language; evaluating animal behaviour for evidence of semantic opacity might be a fruitful research strategy.

Apart from learning that sematical opacity is an indicator of intentionality, animal researchers will be nonplussed by Davidson's arguments. Field researchers are not going

to be sympathetic towards Davidson's antirealism with respect to animal beliefs. Surely, a cognitive ethologist will reason, there is a fact of the matter as to whether the dog has beliefs, regardless of whether we can make sense of what the dog's beliefs might be. Anyone who thinks that showing that a dog has beliefs is distinct from knowing what beliefs the dog has, should not be persuaded by Davidson's scepticism with respect to animal beliefs. I suggest that anyone seriously involved in a research program in cognitive ethology would find themselves thinking that this is a legitimate distinction.

The Features of Language

There are several cases of animal behaviour that are purported to be instances where animals exhibit linguistic behaviour. Before considering these, it is important to consider how we might recognise genuine linguistic behaviour. There are three features or properties of language which seem important enough to require of any purported language; these are properties of infinity, novelty, and generational power.

Infinity: the English language can manifest an infinite number of sentences, as does any other human language. This can easily be demonstrated: "I have x pound(s) of honey" is a well-formed sentence of English for any numerical value of x. Since the possible values of x are infinite, there must be an infinite number of well-formed English sentences, even though most of them will never be uttered.

Novelty: a competent language user is able to understand sentences that that person has never heard before. Undoubtedly, there are sentences in this thesis which any reader has never encountered before; indeed if I were constrained to only sentences already known to my intended audience, it is doubtful that I could make my point. A powerful feature of language is that a competent language user can interpret novel

sentences without difficulty by a component analysis of the sentences. It is often thought that this analysis is possible because well-formed sentences are combinations of words made according to rules. A competent language user, knowing and/or using these rules can rationally interpret the sentence.

Generational power: a language can manifest the novelty and infinity properties in two obvious ways. One would be to have an infinite number of words which each have their own rules for usage; this would make the language impossible to learn in a finite amount of time. The second, and more plausible, arrangement would be to have a finite number of words and a separate and finite number of rules. Repeated applications of the rules on words can be used to construct an infinite number of sentences; but since the rules and words form finite sets, the language can be learned in a finite time.[9] The property of being able to generate an infinite set of sentences from finite sets of words and sentences by the application of words is called the generative property.

Given the preceding discussion, an evaluation of animal linguistic behaviour can be predicated on the assumption that linguistic behaviour will manifest the infinite, novelty and generative properties, and that it will also be genuinely intentional, insofar as it is semantically opaque.

Animal Linguistic Behaviour

Since Descartes there have been many attempts to prove the linguistic apartness thesis false on empirical grounds. The general strategy of this program is to show that animals are capable of using language; either naturally or under human tutelage.

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[9] Since humans learn language in a relatively short time, it seems likely that the set of rules and the set of words are both quite small. Whether this is actually the case is a raging debate best left to linguists.

There are many clear cases of non-human animal communication. Obvious examples are bird song, wolf howls, and elk roaring. Equally clearly, these are not cases of language because they lack the four required properties; a wolf howl or a bird song probably does not ever express a novel proposition, for instance. If a bird song expresses anything, it might be a combination of "my territory, back off" if heard by a male of the same species and "I'm a virile male, let's mate" if heard by a female. Also, adding further song or howls does not add more content to the message, thus it fails also the generational test. The one likely case of natural animal language use is the dance language of the honeybee. That case will be considered separately in my last chapter.

Non-Natural Animal Language

There have been many attempts to teach animals language. For instance, Alexander Graham Bell tried to teach his dog to speak by manipulating its mouth and throat while it growled. As in Bell's efforts, the most promising attempts to teach animals language assume that animals lack only the means, and not the ability, to communicate linguistically. For instance:

> the Gardners had noted that wild apes seem to communicate by observing each other's behavior, and they suspected that the extremely disappointing results of previous efforts to teach captive chimpanzees to use words reflected not so much a lack of mental ability as a difficulty in controlling the vocal tract [Griffin 1984, p. 196].

Operating under this basic assumption that there is no cognitive barrier to rudimentary language use in animals, several attempts have been made to teach apes a language they are physically capable of using.

American Sign Language (ASL) is a language that uses hand gestures for whole English words. While apes are unable to utter the range of sounds necessary for spoken

language, they are physically capable of the range of gestures required for ASL. Consequently, if apes are cognitively capable of learning and using language, then they should be able to use ASL.

During the 1960's and 1970's several attempts were made to teach ASL to chimpanzees. The training paradigm required the chimps to sign in response to various presented or indicated objects, correct signing (for instance, the sign for apple when the trainer points to an apple) was rewarded with praise and food rewards. The chimps were raised in a human environment, and all communication observable by the chimps between humans was also conducted in ASL. The chimps were taught to use a limited, but functional, grammar to construct simple sentences like "Sara wants apple". The language (including grammar) taught to the chimps was designed to meet the requirements discussed above precisely because the researchers wanted to teach a genuine language, and not merely communicative behaviour to the chimps. Their thesis was that if the apes did learn to communicate via the abbreviated ASL, then their linguistic behaviour would manifest the infinity, generational, and novelty properties.

According to Philip Lieberman [1988, p.146], the ape language experiments have proved "that apes use, acquire and transmit concepts by means of words". Since the ape language experiments were designed as minimal languages that manifest the important properties of language, it seems that if apes have learned the languages designed for them, it is pointless to argue that these languages are not genuine. It is worthwhile, however, to wonder whether the behaviour of the apes is evidence of language use, or merely simple behavioural responses to training regimes.

Critics have argued that the conclusion that apes use language is merely the result of wishful thinking on the part of the experimenters. The memory of Clever Hans was a wellspring of criticism of this sort. Clever Hans was a horse who apparently had learned to count. When asked by his trainer mathematical or numerical questions, such as "what is 6 plus 5" or "how many people are in this room", Clever Hans would tap out the answer with his hoof. The evidence that Clever Hans could do math was taken as *prima facie* evidence that Hans had complex cognitive abilities.

Oscar Pfungst, who thoroughly investigated this case [Pfungst 1911], found that Clever Hans was responding to uncontrolled features of the experiment, rather than doing mathematical calculations or counting. Hans' trainer was unknowingly cueing the horse. When his trainer inclined his head to look at his feet, Hans would start tapping his hoof. When Hans approached the right number of taps, his trainer would lean forward slightly in anticipation and Hans would slow his tapping. When Hans reached the right number of taps, his trainer would exhale and relax, inadvertently cueing the horse to stop tapping.

Pfungst discovered that Hans was unable to respond correctly in the absence of his trainer, or in the presence of his trainer when his trainer did not know how many taps would constitute a correct answer. Since Hans tapped randomly when the experiments were controlled against cues from the trainer, Pfungst concluded that Clever Hans was not so clever after all; all the complex cognitive work was done by the trainer, the horse merely responded to sensory stimuli.

This early case of wrongly appropriating cognitive abilities to an animal has made animal behaviour researchers generally skeptical of cognitive explanation of animal behaviour. As Rosenthal writes in his introduction to his new edition of Pfungst's book: "Pfungst's findings solved not only the riddle of Clever Hans but in principle the problem of other 'clever' animals."

Critics familiar with ape behaviour have pointed out that apes are adept and clever at producing whatever behaviour gets them "food, praise, social companionship, or other things they want or enjoy" [Griffin 1984, p. 198]. Like Clever Hans, it seems likely that the apes are merely doing their best to produce the behaviour likely to achieve their own ends, and so there is no reason for thinking that the apes behaviour manifests genuine language use.

In summary, according to the critics, the claim that apes can be competent language users remains uninteresting unless this competence is:

1. Generative, infinite, and produces novel sentences.
2. Manifested in linguistic behaviour complex enough to defy a behaviourist explanation of that behaviour.

Language and Animal Cognitivism

In modern terms, the animal language issue revolves around three questions:

Q1. Do animals exhibit genuinely linguistic behaviour?

Q2. Do animals have internal representations that are equivalent to those which humans manifest in linguistic behaviour (intentionality)?

Q3. Do animals have cognitive states?

These 3 questions are important because many philosophers think that they are the same question given three ways. Malcolm reported that "it is the prejudice of philosophers that

only propositional thoughts (Q2) belong to consciousness..." (Q3) [Malcolm 1972, p. 20].

Some philosophers suppose that a positive answer to whether animals can exhibit linguistic behaviour (Q1) is a positive answer to the question of whether animals have cognitive states (Q3). If a positive answer to Q1 makes a positive answer to Q3, then finding instances of animals exhibiting natural language would dispel all cognitive versions of the apartness thesis (not just linguistic ones). For instance, Griffin reveals his real interest in animal language when he writes:

> For our purposes, the particular communication channel is of secondary importance; we want to know if conscious thinking accompanies the sending and receiving of communication signals between animals [Griffin 1984, p. 154].

This is the ultimate project of much research in animal communication, to answer Q3 by answering Q1.

Jonathan Bennett thinks that language is a necessary, but not a sufficient condition for rationality [Kirk 1967, p. 369]. Rationality is the capacity to reason, making choices and having and evaluating beliefs. Being rational implies having propositional attitudes. According to a strong philosophical tradition, having language use necessitates having propositional attitudes, so a positive answer to Q2 is an answer to Q1. Donald Davidson thinks that language is a necessary and sufficient condition for intentionality, and so a positive answer to Q1 is a positive answer to Q2.

Higher Order Cognition

We have already seen Davidson suggest that he would attribute (or be more willing to attribute) cognition to animals which manifested second order experiences, such as surprise, disbelief and deception. Davidson required that an animal must have a belief about a belief. Bennett thinks that a bee must be able to 'deny' a waggle dance that

indicated false distance or direction information (and the denial must express the content of what is being denied) [Bennett 1964]. Allen and Bekoff think that the possibility of mis-information reflects a second-order quality that is important to animal cognition [Allen & Bekoff, 1997]. The tradition of the search for higher-order awareness seems to be rooted in Chisholm's [1957] discussion of Brentano and intentionality where he suggests that the most promising place to look for intentionality in nonhuman animals is expectation [Allen & Bekoff, 1997]. Dretske [1995] describes these views as being "higher-order" theories of consciousness, and thinks that such theories are currently enjoying popularity in the literature. Fischer thinks "creatures capable of higher order intentionality, such as the intention to deceive, would be comparable to humans in mentality" [Fischer 1987, p. 211].

Interestingly, these demands for higher order phenomena are the sort that are usually not manifested in behaviour, and so are difficult to study. My assessment of this issue is that the requirement for second-order cognition is given merely because it would require first-order cognition. Unfortunately, this strategy leads to a regress. If first-order cognition is to be admitted only if it is evidenced by second-order cognition, why not demand the same standard be applied to second-order cognition? This would require evidence of third-order cognition, and so on. Another reason for interest in second-order cognition might be that consciousness (on the psychobiological view popular in cognitive ethology) is also a second order type of cognition. Evidence of second order cognitive operations might, it could be argued, be evidence for consciousness.

One attempt to study second order behaviour in animals was Gallup's attempt to study physical self-recognition in apes [Gallup 1977]. Gallup conducted experiments

with mirrors designed to see if apes would recognise their own image in the mirror as their own. Apes were variously exposed to windows that revealed other apes or mirrors that reflected their own image. When the apes had grown accustomed to these conditions, Gallup sedated the apes and marked them with dye on their heads (so the dye was not visible to the ape except in the mirror). When the apes were exposed to the mirror, they put their hands to the dyed part of their heads, clearly (Gallup thought) recognising something different about themselves; hence they recognised themselves as themselves (apes not previously experienced with mirrors did not react to the dye). Gallup concluded that these apes demonstrated self-awareness; they had a concept of themselves. Interestingly, only the great apes can achieve this; tests with "gibbons, monkeys, and other animals so far tested" [Griffin 1984, p. 205] have failed to have the same results.

A critic of Gallup's work is Marco Poli, who thinks that the results of tests designed to detect self-awareness "haven't been too satisfactory" [Poli 1988, p. 288]. According to Poli [1988, p. 292], the evidence wildly underdetermines Gallup's argument from physical self-recognition to a concept of self-hood.

Another sort of second-order consideration that has had some currency in the literature is the context dependability or audience effect of animal communication. According to Marler, Karakashian, and Gyger the question "Do animals have the option of withholding signals if the situation is inappropriate?" reveals something important about animal behaviour; if animals can withhold signals then the signalling must be under rational control [Marler *et al* 1991]. Marler *et al* survey the relevant evidence for the

context dependency or ability of animals to withhold signals and conclude that some animal communication must be under rational control.

Reason

Reasoning is making inferences and evaluating beliefs. There is a long tradition associated with thinking that only humans can reason; both Thorndike and Lloyd Morgan had "reason" as the principle difference between man and animal [Gillan 1982]. Darwin thought that "of all the faculties of the human mind... reason stands at the summit" [1899, p. 75]. However, it is clearly false that animals cannot reason; for instance, Gillan [1982] has suggested and implemented experiments to test reasoning in apes. Ape behaviour suggests an ability to evaluate and infer beliefs in order to solve complex problems. Indeed, Gillan suggests that testing "reasoning ability" ought to be simple, and useful for inter-species comparisons. Gillan concludes that humans clearly demonstrate superior reasoning ability, but that this superiority is of a degree, not of kind. Despite an impressive historical pedigree, "reason" cannot form the basis of a viable apartness thesis.

Abstraction

An important basis for an apartness thesis has already been introduced in previous discussion. Locke held the view that the difference between people and animals was abstraction, while Griffin has suggested that displacement (thinking about things not immediate in space or time) is symptomatic of cognitive activity. Likewise, Weiskrantz [1988] follows Lloyd Morgan in taking abstraction to be the key to intelligence:

> If a being has the power of thinking "thing" or "something", it has the power of transcending space and time... Here is the point where intelligence and reason begins [Lloyd Morgan 1890, p. 375].

Clearly, there is an important tradition that associates the ability to abstract and or displacement with the sort of complex thinking identified with humans.

It is clear that animals also are capable of manifesting 'displacement' which seems symptomatic of abstraction [Robacker 1988, Gould 1990]. 'Displacement' is the separation of behaviour from its associated stimulus by time or space. For instance, the dances a bee performs in the hive indicate a nectar source that is not an immediately present stimulus.

Adaptability and Flexibility as Marks of Intelligence

Griffin thinks that demonstrated adaptability is an important symptom of animal consciousness. According to current theories of human evolution, the ability to adapt to changing circumstances and environments was an important factor in the evolution of *homo sapiens*. While this is probably true, it does not mean that humans have a monopoly on adaptability and flexibility. Consider that certain animals from many different branches of the evolutionary tree have successfully adapted to an urban environment in a time frame too short to be plausibly explained as an evolutionary adaptation [Griffin 1992]. Despite its current popularity, adaptability and flexibility don't play any obvious role in ranking animals.

There is a particular account of adaptability that deserves serious attention because, on certain theoretical views, it is the manifestation of intelligence. For instance, Hodos [1982, p. 36] endorses Romanes' view that the behavioural consequence of intelligence is flexibility; thus the greater the intelligence of a creature, the more flexible its behaviour. Conversely, creatures that perform tasks by rote genetic instruction are not considered to be very intelligent. Hodos [1982] gives some examples of 'intelligent'

behaviour: problem solving, concept formation, matching-to-sample, reversal learning, learning set, tool use, etc.

Learning

In behaviourist theory, learning was thought to be a simple mechanistic process; after all, even the most simple cellular life forms can learn associations. Behaviourism sought to explain all changes in behaviour over time with a simple account of learning called stimulus-response. For instance, Pavlov demonstrated that a dog could associate the ringing of a bell with food. Critics of behaviourism have long argued that this account is explanatorily impotent; showing that a dog associates a bell with being fed does not explain how the dog makes such an association. Furthermore, it seems that learning is at least sometimes vastly more complex. Piattelli-Parmarini [1989] suggests that human cognitive psychology accepts that some types of learning are different in kind, not just in complexity, from the types of learning that are explained by behaviourist theory.

Tool Use

Using tool use as a distinction between humans and non-humans seems to have started in anthropology, where the presence of tools in archaeological sites was taken as clear proof of human habitation [Groves 1978]. Identifying tool-use with humans in this way seems uncontroversial, since animals don't (haven't yet been observed to) manufacture tools that would be recognisable as such in an anthropological context. Studies of animal behaviour have found that animals in both natural and non-natural situations make a wide variety of uses of objects as tools.

It might be significant that animals use tools if tool use is a privileged activity.

Griffin thinks that tool use is

> one important category of behavior in which it would appear especially valuable for an animal to think consciously about what it is doing. Even in relatively simple cases of tool use, the object used as a tool is something different from whatever the animal uses it for. Such tool use differs significantly from digging up burrowing prey or otherwise capturing it by direct action, because all but the final steps require seeking out, modifying and manipulating something very different from the food that is thus obtained [Griffin 1992, p. 101].

Griffin's opinion about the cognitive importance of tool use seems motivated by the same sort of concerns I addressed in the section on second-order cognition. That is, using a tool seems removed or displaced in an important way from the activity it is used for. However, any real significance relating to tool use seems to be captured under the considerations advanced during my discussion of second-order cognition.

Consciousness

Another purported difference between animals and humans is that animals do not experience consciousness. Some writers in Cognitive Ethology see this argument as question begging. They think that whether animals experience consciousness is a matter for research, not stipulation [Allen & Bekoff, 1997]. However, if there was evidence or argument to the effect that animals are incapable of consciousness, then that would be a principled difference between humans and animals. While there is no evidence to date that animals are not conscious, there is at least one philosophical argument to this effect.

Peter Singer considers an argument against animal consciousness that is predicated upon animals being incapable of language use. As we have seen above, there might be good reason for thinking that animals are capable of language use, which would render the present issue moot. Nevertheless, Singer writes:

> There is a hazy line of philosophical thought, deriving perhaps from some doctrines associated with the influential philosopher Ludwig Wittgenstein, which maintains that we cannot meaningfully attribute states of consciousness to beings without language. This position seems to me very implausible [Singer 1990].

Singer thinks that it is unlikely that "Human infants and young children... unable to use language" are never conscious even though they cannot use language. Simply: "language cannot be crucial" [Singer 1990]. Likewise, it seems perfectly possible that adult humans identical to ourselves in every respect, but incapable of language, would be conscious.

The argument Singer addresses is known as the "private language argument".

According to De Grazia, Wittgenstein's private language argument (PLA) shows that

> nothing 'in the head' of a rule-follower is necessary or sufficient to credit her with understanding: not necessary because competence is demonstrated without anyone's knowing what is in the rule-follower's head; not sufficient because any thing 'in the head' would just be a further sign, and would clearly not constitute understanding if the individual were entirely unable to demonstrate mastery in her behaviour [De Grazia 1994, p. 122].

Since, according to the PLA, nothing in the head of the animal is sufficient to attribute understanding, understanding must be attributed on the basis of observable behaviour.

> De Grazia summarizes the tradition of the PLA as follows:
>
> (1) For an expression to be part of a (genuine) language, it must be meaningful;
> (2) For an expression to be meaningful, it must be possible to use it correctly or incorrectly;
> (3) For it to be possible to use an expression correctly or incorrectly, there must be criteria (rules) for correct use;
> (4) For there to be criteria (rules) for correct use, there must be a practice that allows confirmation or correction of the prospective expression-user;
> (5) No expression referring to a private object (in the sense defined) admits of such a practice; therefore;
> (6) No such expression admits of criteria for correct use; therefore, ... there can be no private language [De Grazia 1994, p. 123].

De Grazia proceeds to suggest that the crucial question for a student of the PLA is whether premise (5) is true. Wittgenstein's argument for premise (5) is that it makes no

sense to ask whether a private sign can be used correctly in the absence of objective or inter-subjective tests (a necessary absence given our forced commitment to methodological behaviourism). "The term for the putative private object lacks content; no criteria for using it exist, so it singles out nothing in the mental world" [De Grazia 1994, p. 124].

Many writers in cognitive ethology have attempted to address (and reject) the PLA. Critics of Cognitive ethology often invoke the PLA, for instance Humphrey [1982, p. 474], and Frey [1980, pp. 101-110] who explicitly invokes the PLA against the possibility of animals being self-consciousness.

If necessary, the PLA can be rejected with a *reductio ad absurdum* strategy (it might imply the obviously false conclusion that human languages cannot exist). But the PLA is really not hostile to animal cognitivism. After all, researchers in cognitive ethology are obliged to be a methodological behaviourists. Only public animal behaviour is amenable to scientific scrutiny. If Wittgenstein has shown that private languages cannot exist, so what? The matter at hand is whether a specific public behaviour is linguistic; a matter to which the PLA does not relate.

Conclusion to Chapter One

Hodos [1988, p. 100] gives a list of behaviours likely to index intelligence in humans and animals and points out that while each item on his list was previously thought to exclude animals; further research found animals exhibiting these behaviours. After enjoying some currency, each specific version of the apartness thesis has been repudiated; animals do manifest the behaviours associated with language, learning, tool-use, second-order cognition and adaptability/flexibility. Although there is some reason to

be sceptical as to whether the behaviour has a cognitive cause, the fact that we can seriously consider this question indicates the very implausibility of the apartness thesis; after all, similar reasons motivate scepticism about the cognitive nature of human behaviour. In consequence of these considerations, it seems clear that there are no privileged features or abilities that are hallmarks of humanity. As we will see, this conclusion makes the job of cognitive ethology much harder.

Chapter Two

A Cognitive Theory of Animal Behaviour

A Brief History of Cognitive Psychology

Since Cognitive ethology seeks to apply the theories and methods of human cognitive psychology to animals, an obvious starting point is a brief review of the history and nature of human cognitive psychology. There are three distinct research strategies dominant in the modern era of psychological research. In the late 19th century, introspective psychologists, such as Freud, conducted research by introspective analysis and psychological therapy. Psychological theories were researched and confirmed or rejected on the reports of test subjects who were asked deep and probing questions, and by the relative success of therapeutic remedies. One of the chief difficulties with introspective psychology was that the introspective reports required theoretical interpretation before they could count as evidence for or against a theory; critics thought that this was too permissive a program for a thorough study of psychology [Baars 1986].

In reaction to this problem, the next phase of psychological research was highly restrictive. From the 1920's to the 1950's, psychological research was highly constrained to directly observable data such as behaviour and anatomy [Griffin 1984, p. 10], as I described in the previous chapter.

> Behaviourism undoubtedly had a healthy effect for many years in forcing students of animal behavior to concentrate on gathering objective evidence that could be verified by other scientists. But in the process, the subjective experiences of people and animals have come to be regarded as inconsequential fantasies [Griffin 1984, p. 19].

All behaviour was explained by reinforcement (behaviour patterns that are successful are reinforced), either learned or genetic; the reinforcement process was stipulated to be non-cognitive, since a cognitive process would have been too divorced from observable data [Baars 1986].

The incredible restriction placed on psychological research in this period reached a crisis when research programs could not explain observed behaviour merely by appeal to reinforcement. Attempts to develop a more robust theory revealed that the foundations of behaviourism were theoretical rather than observational: "behaviourism was a product of philosophical theory rather than based on evidence, since the evidence undermined behaviourism, rather than the reverse" [Wapner in Baars 1986]. Alternatives were developed which differed in theory but also could explain complex behaviours.

Since the late 1950's there has been a significant revolution in the various disciplines which study psychological phenomena. Psychology, zoology, anthropology, and philosophy have all had to revise their methodologies; unobservable cognitive processes play a significant role in their theories and explanations.

> The so-called "cognitive revolution" is concerned with the states of the mind/brain that enter into thought, planning, perception, learning and action. The mind/brain is considered to be an information-processing system, which forms abstract representations and carries out computations that use and modify them. This approach stands in sharp contrast to the study of the shaping and control of behavior that systematically avoided consideration of the states of the mind/brain that enter into behavior. and sought to establish direct relations between stimulus situations, contingencies of reinforcement, and behavior. This behaviorist approach has proven almost entirely barren, in my view, a fact that is not at all surprising since it refuses in principle to consider the major and essential component of all behavior, namely, the states of the mind/brain [Chomsky 1987].

Most recently, there have been serious attempts to study the psychology of animals in much the same manner as human psychology [Griffin 1992].

In human cognitive psychology, behavioural observations are explained by appeal to their immediate inputs, simple response mechanisms (such as instinctual or habitual behaviour), and by internal processes such as memory and information processing. Cognitive theory is not necessarily committed to any view on the nature of cognition or cognitive states, other than that it performs certain roles[10] [Baars 1986]. Of course, cognitive researchers often make conclusions about the nature of cognition and particular positive states, but, in a minimalist cognitive research approach, this is not required.

Methodological Behaviourism

An important problem facing researchers in both animal and human psychology is that the phenomena in question can only be studied indirectly [Place 1993, Fodor 1980]. Our assessment of the psychology of a creature is necessarily theoretical, although its behaviour is clearly observable. The standard solution to this problem is to assume that a creature's behaviour is caused by its psychology, and so the nature of the psychology can be inferred from the behaviour. This point of view Fodor [1980] has dubbed "methodological behaviourism".[11]

Recall that I called this approach methodological behaviourism to distinguish it from philosophical and logical behaviourism. Methodological behaviourism has no ontological presuppositions. Walker admits that all animal research must operate under the constraints of methodological behaviourism [Walker 1983, p. 108]. Likewise, Allen and Bekoff observe that:

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[10] In the philosophy of mind, this minimalist view (no commitments on the nature of cognition or cognitive states except what nature they are required to have to fulfil the role they are postulated to play) is called functionalism.

[11] Methodological behaviourism should not be confused with logical behaviourism, which is a theory about the sort of expressions that can meaningfully describe psychology; nor should it be confused with Skinner's philosophical behaviourism, which is a biological research program predicated on confidence in the ontology permitted by logical behaviourism.

> If psychology takes the scientific road to truth, it will discover that the only observables available are stimuli and responses. That reality means that, at bottom, psychology must be behavioristic. After that, it can be as biological, cognitive, or even humanistic as it wants to be. But disciplines that study something else--like brain, mind, or human potential--without connections to stimuli and responses may occasionally be science--even elegant science--but they are not psychology. Psychology is the science of behavior. [Allen & Bekoff, 1997].

This means that animal cognitive theory will always suffer from a poverty of evidence. This evidential poverty is illustrated by two thought experiments standard in philosophical psychology (designed to show that behavioural evidence underdetermines psychology theory): the Super-Spartan and the Super-Thespian.

Super-Spartans' behaviour never betrays their psychological experience; even while experiencing great suffering Super-Spartans don't flinch. According to methodological behaviourism, we would have to attribute Super-Spartan psychology in the face of Super-Spartan behaviour; but this is counter-intuitive, even if the Super-Spartan doesn't evince the kind of behaviour indicative of suffering, surely the Super-Spartan still suffers, though bearing the suffering in silence.

Contrariwise, Super-Thespians portray all the behaviour associated with psychological states without actually experiencing them. A methodological behaviourist would have to attribute the appropriate psychological states on the basis of the observed behaviour; again this is counter-intuitive, Super-Thespians are just acting; despite an convincing performance of pain evincing behaviour, no suffering occurs.

Clearly, methodological behaviourism is a weak approach to psychological research, it can be either misleading (as illustrated in the Super-Thespian case) or useless (as in the Super-Spartan case). Nonetheless, it is the only methodology available. Even

so, all conclusions regarding animal psychology must be tempered with a healthy scepticism resulting from an awareness that theory will be underdetermined by data.

The Problem of Other Minds and The Argument from Analogy

One of the most basic and perplexing questions in the philosophy of mind is the problem of other minds. It seems *prima facie* obvious to everyone that they are aware of their own experiences. However, it seems impossible to prove that anyone besides yourself experiences anything at all; your awareness of your own experience is private and immediate, and so differs from your awareness of everything else, which is public and indirect. It is consistent with everything you could ever know (with certainty) that everyone besides yourself could be a zombie-like automaton. This problem affects attributions of mind to other humans and animals equally: "there appears to be no way to formulate the other minds problem so that it applies to animals but not to fellow humans" [Fischer 1987, 210]

The problem of other minds seems to discredit any attempt at a serious scientific study of the minds of other animals. Allen formalises what he calls the privacy argument as follows:

> Mental phenomena are private phenomena.
> Private phenomena cannot be studied scientifically.
> Thus, mental phenomena cannot be studied scientifically [Allen 1997].

If this argument can be carried, then Griffin's project is hopeless. However, as Allen points out, this argument depends upon a peculiar understanding of "mental phenomena" that no scientist would be interested in. That is, scientifically interesting phenomena are only those which have causes and effects. Hence scientists are interested in mental phenomena that do have causes and effects, and such phenomena cannot be private, since

they would not (from the scientist's point of view) have effects. If the privacy argument does not depend on this limited understanding of 'mental phenomena', then the first premise is simply false, for the same reason.

The traditional, but unsatisfying, solution to the problem of other minds is the argument from analogy which argues that it is implausible that everyone around you can be similar to you in so many ways and yet not experience things the way you do.

> I conclude that other human beings have feelings like me, because, first, they have bodies like me, which I know, in my own case, to be the antecedent condition of feelings; and because, secondly, they exhibit the acts, and other outward signs, which in my own case I know by experience to be caused by feelings [J.S. Mill 1865, p. 191].

Since everyone resembles you in so many ways, by analogy, you can safely infer that they have feelings like yours.

The two chief problems with the argument from analogy that we should anticipate are (a) the argument assumes that each of us is correct about characterising our own experiences, albeit privately; and (b) that other creatures are sufficiently similar to justify the analogy. Like most researchers in cognitive ethology Griffin ignores problem (a). Problem (b) is going to be particularly troublesome to any study of animal psychology, since animals are less similar to each of us than humans are to each other, weakening the analogy. Allen [1997] identifies this as the "other-species-of-mind problem".

Griffin will present an argument from analogy to the effect that animal experience (which he calls consciousness) is real and important; he argues that physical and behavioural isomorphism between humans and animals justifies psychological isomorphism. Griffin's statement of the problem of other minds, and his response to it goes as follows:

> A primary reason for this avoidance of the subject is a belief that mental experiences, especially those of animals, are inaccessible to scientific investigation because they are private to the organism experiencing them. Hence it is claimed that no statement about them can be verified by others. But we do of course obtain useful if incomplete and somewhat distorted information about other people's thoughts and feelings by making inferences from their behavior, and especially their communicative behavior. Furthermore, the difficulty, or even the impossibility, of conveying to others the exact nature of something does not rule it out of existence or deprive it of significance [Griffin 1992, p. 6].

However, if there is a quantitative difference (a difference not of degree, but in kind) between humans and animals, then the analogy is false, and the argument from analogy can not get started.

A large part of Griffin's project is to build the case for the analogy from other minds. If animals really are a lot like humans, then the argument carries weight; otherwise the argument from analogy is weak. In either case, we need to be aware of the limitations of this reasoning strategy.

Griffin's Description Of 'Cognitive'

Supposing that we could sometimes justify attributing cognition to animals, some sense needs to be made of 'cognition'. Griffin does provide some insight into the meaning of 'cognitive':

> I am using *cognitive* (italics his) in a literal sense to refer to conscious thought and knowledge, thus avoiding a recent tendency to restrict the term to information processing. This narrower usage, as in cognitive psychology, is confusing at best, and at worst it may do us a grave disservice by fostering the belief that even human thinking may consist only of information processing [Griffin 1984, p. vi]

Unfortunately, there is little philosophical agreement regarding the nature of conscious thought and knowledge, so Griffin's account needs some amplification.

Griffin's account of consciousness begins with three central intuitions about cognitive states for which there seems to be general agreement within the literature on cognitive ethology. The first intuition is that some, but not all, cognitive states are conscious ones. For example: my cognitive control over my breathing is usually unconscious. Normally I am unaware of it, although I can also consciously chose to hold my breath. On the other hand, my cognitive control over the words I now type is conscious, I am aware of what I am doing. The second intuition is that while it seems perfectly possible that the cognitive states of some animals are never conscious, those of humans definitely sometimes are conscious; the perceptual mode by which we are aware of our own conscious cognitive states philosophers call introspection. The third intuition is that animals differ markedly in cognitive ability. Humans and apes seem to have more powerful cognitive abilities than honeybees, and honeybees might be more cognitively endowed than other creatures.[12] Donald Griffin and other researchers in cognitive ethology begin their study of the psychology of animals armed with cognitive theory and with the three intuitions just noted.

Griffin chooses to concentrate his remarks on animal consciousness although he provides no further account of what consciousness is. In my analysis, cognition will be defined functionally. By doing so I gain the advantage of avoiding the pitfall of epiphenomenalism. Allen and Bekoff describe how this problem makes it difficult to make a scientific analysis of 'consciousness': "many philosophers' theories of what

[12] I don't mean to imply here that there is some inter-species SAT Test that can be used to rank animals cognitive abilities, but merely that superficial observation reveals marked differences in abilities. The cognitive ability of an animal might be rated on a totally behaviourist methodology according to the degree to which cognition is appealed to in the explanation of the animals behaviour; the explanation of how humans interact socially to build a modern city must appeal to cognitive theory more significantly than the explanation of bird migrations, even though both explanations are cognitive in nature. Of course, animals with simple behavioural repertoires might still have great,

consciousness is make it hard to see what consciousness does for organisms that possess it" [Allen & Bekoff, 1997].

My functionalist approach is meant to be compatible with what Baars describes as "the emerging psychobiology of consciousness" where:

> (w)e are beginning to see human conscious experience as a major biological adaptation, with multiple functions. It seems as if a conscious event becomes available throughout the brain to the neural mechanisms of memory, skill control, decision-makings, anomaly detection, and the like, allowing us to match our experiences with related memories, use them as a cue for skilled actions or decisions, and detect anomalies in them. By comparison, unconscious events seem to be relatively isolated. Thus consciousness is not just any kind of knowledge: It is knowledge that is widely distributed, that triggers off widespread unconscious processing, has multiple integrative and coordinating functions, aids in decision-making, problem-solving and action control, and provides information to a self-system [Baars 1997].

Clearly, this approach has a theoretically robust understanding of the function cognition and consciousness play in the lives of humans and animals.

Cognitive Ethology

The principle thesis of cognitive ethology is that animal behaviour is most adequately explained by appeal to cognitive theory. Some researchers in cognitive ethology want to defend the stronger view that animal consciousness is "real and important" [Griffin 1992]. Although the nature of cognition is the subject of much debate in the scientific and philosophical literature, Griffin thinks that animal cognition is much like human cognition in that some, but not all, of it is conscious [Griffin 1992, p. 16]. In the human case, consciousness "adds so much to an unconscious, 'sleepwalker'[13]

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unmanifested, cognitive abilities; mice might secretly be great philosophers, but their observable behaviour doesn't reveal this.

[13] Griffin doesn't elaborate what he thinks a sleepwalker existence would be like.

existence" [Griffin 1992, p. vii], that it seems likely to Griffin that non-human animals are conscious too. Griffin argues that one hallmark of consciousness is 'cognitive displacement'; the "ability to think about objects and events, whether or not they are part of the immediate situation" [Griffin 1984]. Other features of consciousness mentioned by Griffin are self-awareness, and subjective feelings such as pleasure and pain.

Cognitive ethology is supported by observations of human/non-human anatomical isomorphism and by observations of the behaviour of animals, with especial emphasis on communicative and social behaviour which suggests that animals have similar cognitive equipment to what humans have. The two main lessons advocates of cognitive ethology want to draw from this support are:

1. Animals are less unlike humans than previously thought;

2. Animal behaviour suggests that the cognitive theories that explain human behaviour might also explain animal behaviour.

If animal behaviour is best explained by the same explanation as those which explain human behaviour, then it seems plausible that the cognitive states of animals might be just like humans states in that some, but not all, of them are conscious. Some, but not all cognitive ethologists, want to draw from (1) and (2) that: (3) the cognitive theories that most adequately explain human and animal behaviour are true, and hence animals have cognitive processes and may be conscious.

My exposition and criticism of cognitive ethology will rely on Griffin's work, and subsequent studies modelled on his program. His important role in the literature, plus his exhaustive surveys of relevant literature and research, make this a safe shortcut for the present purpose.

Motivations For Cognitive Ethology

Griffin's case for cognitive ethology is clearly motivated by his respect for the complexity of the biology and behaviour of the animals he has studied. As an expert on bats, Griffin was stimulated by Thomas Nagel's [1974] influential philosophical paper "What is it like to be a bat" [Allen & Betkoff 1997], which asks whether it would ever be possible for us humans to know exactly what it is like to be a bat.[14] Griffin and other researchers took Nagel's paper to be a scientific problem capable of resolution; they were reluctant to accept that there was no meaningful answer to the question "what is it like to be a bat?".

Like many of his fellow animal researchers, Griffin is also motivated by ethical considerations. Being convinced that at least some animals are conscious and/or that they feel pleasure and pain seems to have implications for the nature of the relationship between animals and humans. Of course, these motivations beg the question; only if one were already convinced that animals were conscious and feel pleasure and pain would ethical considerations motivate a pursuit for evidence that animals are conscious. However, many animal researchers are motivated in their research to find strong evidence that animals are conscious in order to convince their more conservative colleagues that the relationship between animals and humans is founded unsoundly [Dawkins 1980]. Fortunately, Griffin is wise enough to separate the ethical questions from the scientific questions. His task is, in part, to satisfy the scientific questions so that the ethical debate can be fully informed. I will not be commenting at all on the ethical implications of

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[14] Nagel's paper in turn relies on Griffin's findings about bat echolocation.

cognitive ethology. Were I to take the opportunity to do so, I would argue that there are none.

The New Problem For Cognitive Ethology

I conclude from the analysis of the apartness thesis in chapter 1 that there is no principled reason for thinking that humans differ in kind from animals. The very same considerations mean, however, that cognitive ethology lacks an obvious starting point. Repudiating the apartness thesis has a cost; if no privileged behaviour(s) serves as the mark(s) of cognitive function(s), then cognitive ethology must begin by identifying the sort of behaviours that might be the mark(s) of cognitive function(s). The privileged behaviours sometimes taken to be clear indications of cognition in animals may not indicate anything at all.

For instance, evidence that apes use language is often combined with a quote from Descartes on the uniqueness of language ability to justify the claim that apes are psychologically equivalent to humans [Beneveniste 1982]. The problem with this strategy can be illustrated, making explicit the role the linguistic apartness thesis plays in the argument:

> Language is a distinguishing feature of humans;
> Humans are rational (or intentional, or conscious etc.);
> Animal X uses language:
> Hence animal X is rational (or intentional, or conscious etc).

Despite being self-defeating (if animal X uses language then language is not a distinguishing feature of humans) this argument form has enjoyed serious attention and respect. Its intuitive appeal seems to rely on conferring, by analogy, 'honorary' "human-ness" to animals which have attributes picked out by some version of the apartness thesis.

However, as a result of considerations noted in the previous chapter, the apartness thesis is not the basis for any reasonable scientific investigation.

This problem I take to be a central conflict in cognitive ethology. Cognitive ethology completely repudiates the apartness thesis; but rejecting the apartness thesis undermines its entire research program. Most animal behaviour research designed to prove that animals experience cognition is predicated on the assumption that certain human behaviours are privileged; thus, finding the same behaviour in an animal would be an important discovery. Unfortunately, the basis for the privilege of these features is just the apartness thesis.

Proposed Strategy For Cognitive Ethology

I think reconstructing the argument in terms of explanatory adequacy can solve this problem. Griffin's program clearly appeals to the explanatory adequacy of cognitive explanations of animal behaviour. Consequently, the argument discussed above can be reconstructed as:

Language behaviour is best explained by cognitive theory;
Animal X uses language;
It is reasonable to attribute cognitive states to Animal X.

So that, more generally, the argument is:

Y is best explained by cognitive theory;
Animal X displays Y
It is reasonable to attribute cognitive states to animal X.

This research strategy requires developing cognitive theories that explain animal behaviour and dismissing, through negative research, other explanatory possibilities.

The beginning point for identifying behaviours best explained by cognition in animals is the same list of behaviours given previously in association with the discussion of the apartness thesis; after all, our intuitions about these behaviours are not entirely wrong. Clearly, the most important of these behaviours is language, simply because of Chomsky's proof that only a cognitive explanation is explanatorily adequate. Other important behaviours can be identified by their theoretical description requiring an explanatory separation between stimulus and response.

As discussed in the previous chapter, there is good evidence that behaviour that requires a cognitive theoretical explanation is manifested in animals. Observation of such behaviours is the foundation upon which a respectable cognitive ethology must depend. Of course, the cognitive interpretation of this observation must be defended from the criticism that these behaviours are not genuinely manifested in animals. If they are, as Bennett has argued with respect to honeybees, merely apparent, then we have been wrongly attributing cognitive states to animals.

Attributions of Cognition

Human cognitive theory is minimally described as positing internal states that are functionally described as being causally intermediary between stimulus and response. While proponents of folk psychology believe that our intuitive notions of 'beliefs' and 'desires' are the functions from experience to behaviour and vice versa [Bennett 1983], cognitive theory could rely on other analyses besides folk psychology. Cognitive ethology intends to make the same theoretical apparatus work for animal psychology as for human psychology.

Theories Of Animal Cognition

A theory of animal cognition needs to be compatible with the major theory of biology: Darwin's theory of evolution. In order to defend the idea that certain behaviours should be associated with cognition, a role for cognition in animals consistent with evolutionary theory must be developed. To be integrated with evolutionary theory, an account of how cognition is adaptive must be included in the theory of animal cognition.

Given that humans also evolved from the same origins as animals, it is clear that under certain conditions cognition is adaptive, else wise, humans would not have evolved as cognate animals. However, it could be argued that these conditions only pertain to humans. Just as opposable thumbs are adaptive for certain species, they must not be adaptive for every species, simply because most species don't have them. One attempt to work out how cognition might be adaptive is the Jolly-Humphrey hypothesis.

The Jolly-Humphrey Hypothesis

The Jolly-Humphrey hypothesis assumes that cognitive functions are biological phenomena, and presumably these functions were selected for according to biological theory [Gregory 1980]. In this context Allison Jolly and Nickolas Humphrey postulate that an adaptive advantage would be conferred on animals which could easily and reliably predict the behaviour of other animals with which they are likely to interact. Humphrey thinks that it is a consequence of this hypothesis that:

> a revolutionary advance in the evolution of mind occurred when, for certain social animals, a new set of heuristic principles was devised to cope with the pressing need to model a special section of reality - the reality comprised by the behaviour of other kindred animals. The trick nature came up with was introspection; it proved possible for an individual to develop a model of the behaviour of others by reasoning by analogy from his own case, the facts of his own case being revealed to him through examination of the contents of consciousness [Humphrey 1979].

Jolly and Humphrey suggest that some cognitive functions are adaptive traits that solve the computational problem of social interaction between animals (including humans). Their theory also explains why the self-evaluative process philosophers call introspection would evolve.

Humphrey evaluated behaviourism versus cognitivism in terms of adequacy of explanation of successful social interaction. According to Humphrey, it is implausible that animals have the computational power to manage the complex variables involved in a behavioural analysis of fellow creatures that could guide social interaction. Alternatively, a cognitive analysis is, according to Humphrey, computationally simpler, and so is a good explanation of successful social interaction. The basis for thinking that the cognitive explanation is simpler is summarised by Premack & Woodruff [1978] as a model where "an animal need only ask itself what it would do in a similar situation to predict the behaviour of another, rather than have to make detailed observations of its fellows. This model's chief advantages are that an animal has a basis for interaction on first contact with another animal, and can predict complex behaviour with simple computations. Premack's slogan [Premack and Woodruff 1978, p. 526] illustrates this point: "the ape could only be a mentalist. Unless we are badly mistaken, he is not intelligent enough to be a behaviourist". The Jolly-Humphrey hypothesis has been endorsed and defended by many writers in cognitive ethology; for instance, Marian Dawkins [1993] uses this approach to develop her case for animal rights.

Humphrey suggests it is 'consciousness' or 'introspection' which is the cognitive function that reduces the computational load of social interaction; he suggests this because a socially interacting animal can infer and generalise to the predicted behaviour

of its fellows from its own motives and desires. Premack and Woodruff have described this view as the theory that animals use a folk psychology theory of mind to predict the behaviour of their fellows by imputing "mental states to himself and to others" [Premack and Woodruff 1978, p. 515]. Obviously then, according to Humphrey's explanation of social interaction, a socially interacting animal needs to be aware of its own motives and desires; according to Humphrey and his colleagues, introspection is a vital biological activity.

There are some clear reasons for thinking that Humphrey's view has some merit. Since large-brained animals are slightly more likely to have complex social interactions, Humphrey's theory has some empirical basis; although the honeybee and other social insects are important apparent exceptions [Dawkins 1983, p. 361]. However, Walker [1983, p. 235] found no evidence that animals which have larger brains, such as apes and humans, have significantly more complex behaviour. Instead, Walker found that the largest share of the computational load on animal brains is perceptual; and there is evidence that animals with smaller relative brain size have less perceptual acuity. There thus seems to be good reason for scepticism that social interaction is the driving force for the cognitive function Humphrey calls consciousness. This scepticism needn't infect the more general hypothesis that cognition is a biological phenomena.

Accepting the Jolly-Humphrey hypothesis solves certain other important philosophical problems for cognitive ethology. Humphrey observes that Darwinian continualism clashes with the philosophical doctrine that "consciousness is essentially private". The privacy doctrine clashes with Darwin's theory that animals have evolved by adaptation. The Jolly-Humphrey hypothesis makes this conflict explicit by assuming

that cognitive functions are adaptive biological traits which, according to Darwin's theory, must confer some evolutionary advantage; as such, it must have some public effect since the competition for survival, the modus of evolution, takes place in the public domain. In short, if consciousness evolved, then it must not be wholly private.

Since cognitive ethology certainly assumes that cognitive behaviours evolved, then it follows that cognitive ethology must not accept the doctrine that consciousness (or any other cognitive process) is wholly private. Clearly, the problems of the Super-Spartan and the Super-Thespian must be only epistemic problems; while we can't know the nature of the Super-Spartan's psychology, this must not entail that the Super-Spartan has no psychology; otherwise, the Jolly-Humphrey hypothesis, and Darwin, would both be mistaken. The positivist tendency to subjugate ontology to epistemology must be rejected.

Another important consequence of the Jolly-Humphrey hypothesis is that Wittgenstein's private language argument is simply moot. We might be persuaded by this argument that anything wholly private is meaningless; but since cognitive functions are not wholly private, they don't pertain to Wittgenstein's argument. Likewise, there must be, in principle, some way to make a good empirical study of cognition, since it must have public effects.

It will be worthwhile to consider if the central background assumption of the Jolly-Humphrey hypothesis, that cognitive functions are biological functions, is defensible. There are two general criticisms of this assumption worth considering. Firstly, there are well worn arguments in philosophy and psychology which conclude that cognitive functions don't exist, and so, *a fortiori*, are not biological phenomena. Like

Griffin, I haven't offered a defence against such arguments, I have only undertaken a

defence of animal cognitivism, not cognitivism in general.

The Jolly-Humphrey Hypothesis and Epiphenomenalism

Secondly, critics of the Jolly-Humphrey hypothesis argue that the inference from

evolutionary theory is illegitimate; accepting that cognitive functions are biological

phenomena does not warrant concluding that cognitive functions have public effects.

The theoretical possibility that cognitive functions are epiphenomena is a genuine

problem for an advocate of the Jolly-Humphrey thesis [Harnad 1982]. Briefly,

epiphenomena are effects but not causes. If cognitive functions do not cause anything,

then they cannot be adaptive, because evolutionary selection is a causal process. Critics

of the Jolly-Humphrey hypothesis point out that cognition might be a by-product of other

evolutionary adaptations, since the nature of evolution is such that not every trait is

necessarily adaptive. Consider the following remarks from Richard Milner's The

Encyclopaedia of Evolution:

> The concept of adaptation is one of the most troubling and puzzling in
> natural history. Since *an animal is the product of a long contingent
> history*, its adaptation is relative at any given time... Early flightless
> "wings" may have been used to stabilize swift running birds, as ostriches
> use them today, or they may have first functioned as heat regulators. *For
> many structures, we cannot determine how they originated, nor what may
> be their future...* One of Darwin's enduring demonstrations was that
> adaptations are usually not marvels of perfection at all, but historical
> compromises. On closer examination, they usually turn out to be jerry-
> built contraptions -- products of a unique, opportunistic history [Milner
> 1990, p. 4].

In short, not every biological trait is adaptive, so it would be wrong to infer that

consciousness is adaptive just because it is a product of an evolutionary history. Instead,

it could have evolved as an epiphenomena; a by-product of evolution. For instance, if as a

result of genetic happenstance, complex brain function were closely correlated with the occurrence of opposable thumbs, then even though there is no direct causal relation between complex brain function and evolutionary advantage, complex brain function might still be selected for, since the adaptive advantage of opposable thumbs would be conferred on complex brain function indirectly.

The possibility that cognition is epiphenomenal seriously undermines the Jolly-Humphrey hypothesis, since epiphenomena are necessarily private, for they have no effects at all. Clearly, it is possible that the Jolly-Humphrey thesis is wrong, and that cognition could be essentially private, since it could have evolved without being adaptive; and only if cognition is adaptive does applying evolutionary theory (the Jolly-Humphrey hypothesis) entail public consequences [Gregory 1980]. Obviously, Darwinian evolution is compatible with cognition being wholly private.

An interesting argument against evolutionary epiphenomenalism has been advanced by Plotkin [1988] who advocates "an evolutionary epistemological approach to the evolution of intelligence". Plotkin asserts that more fundamental, or simpler, modes of intelligence and learning have less biological costs associated with them. Each biological structure has benefits and costs. The benefits are the advantages the structure confers on the creature, while the costs are the energy consumed, and any disadvantages conferred, by the structure. The cost and benefits pertaining to any new structure means, according to Plotkin, that there is an inertia effect in evolution; it is easier to modify an existing structure than to add a new one so the return for a higher mode must be great. Plotkin writes:

it is a rule of inertia which asserts that the modules that have already evolved, and the variant generators of which they are already composed, will be used for as many forms of knowledge gain as possible. New forms... will evolve only if the costs of additional variance generators and selectors are consistently outweighed by benefits in the form of more efficient behavioral adaptations. More fundamental levels are cheaper and more reliable [Plotkin 1988, pp. 82-83].

Plotkin suggests, based on these considerations, that human consciousness cannot be epiphenomenal, or an evolutionary byproduct. According to Plotkin's reasoning, evolutionary byproducts are only those which have an extremely low cost. Since Plotkin thinks the energy costs associated with the brain activity necessary for consciousness are high, consciousness must confer an offsetting evolutionary advantage. As we have seen, conferring an evolutionary advantage is incompatible with cognitive functions being epiphenomena.

I want to suggest, however, that the possibility of cognitive functions being epiphenomena isn't one that I need to take seriously. I can ignore this possibility just because cognitive functions (at least in this project) are defined functionally; the task of cognitive theory is to explain behaviour. Since cognitive functions are defined according to their effects, it follows trivially that they have effects. Cognitive psychology, as applied either to humans or animals, is a theory driven by explanatory potency. Since epiphenomena are as explanatorily impotent as they are causally impotent, there are no epiphenomena in cognitive theory. If cognition has no function, then, on a functionalist account, it does not exist.

A related concern is that there is no obvious account of how cognitive functions are adaptive; and it is not entirely clear how adequate cognitive explanations are. Humphrey's account in terms of social interaction is an acknowledged "just-so" story; it isn't scientifically compelling [Humphrey 1982]. Unfortunately, a robust account of

cognitive functions isn't available to either human or animal psychologists. Until we know much more, the demand for a complete theory of the evolution of cognition will be unfulfilled.

An ignored problem for the cognitive researcher who embraces the Jolly-Humphrey hypothesis is that it undermines the central intuition in cognitive research: the argument from analogy. If Humans have evolved, as Humphrey suggests, to use cognitive theory (folk psychology) to predict the behaviour of our fellows, then we should be unimpressed by the intuitive appeal of the argument from analogy; this intuitive appeal may be genetically determined. The argument from analogy might be appealing because of its instrumental value, not because of its veracity. We may be incorrectly inferring, by analogy, that others have the same feelings we do because we making this inference has an instrumental evolutionary value.

Lawrence Weiskrantz [1988] has expressed serious doubts about the viability of the Jolly-Humphrey hypothesis, which he thinks entails that animals must be absurdly empathic. Weiskrantz thinks that behaviours involving social complexity are well within the means of automatic routines of the sort described by Skinner; and furthermore, automatic routines don't get caught in paranoid delusions or recursive loops. Weiskrantz ignores, however, that humans and animals do seem to get caught in paranoid delusions and recursive loops; the mental lives of humans and animals do involve occasional mental illness. The advantages Weiskrantz sees in a behaviourist alternative aren't necessarily manifested in real animals.

Ramachandran [1980, p. 4] discusses a theory he attributes to Barlow that consciousness evolved among "social animals to permit gregariousness and communication". Ramachandran considers this thesis to be inconsistent with his

reflections upon his own case; much communicative behaviour seems to be 'ahead' of consciousness; for instance, yelling when injured before pain is consciously felt. While it is certainly true that certain communicative behaviour seems to be caused by 'reflex actions', we must make a distinction made between linguistic and merely communicative behaviour. The fact that some behaviour is merely communicative, as seems to be the case in Ramachandran's example, does not mean that genuinely linguistic communication is impossible. Ramachandran seems to ignore the case where he communicates after a considered and deliberate decision to do so.

Summary Of The Jolly-Humphrey Hypothesis

The Jolly-Humphrey hypothesis represents important theoretical progress for a cognitive study of animal behaviour. The Jolly-Humphrey hypothesis assumes that animal cognitive abilities have evolved, and hence theories about these abilities can be evaluated on the same basis as any other biological phenomena, according to Darwin's theory of evolution. By constructing a coherent theory on this assumption, Jolly and Humphrey legitimise this theoretical strategy. The Jolly-Humphrey hypothesis gives us a possible explanation for the adaptive nature of particular cognitive activities. However, this plausible explanation for the evolution of cognition and consciousness undermines the intuitive persuasiveness of the argument from analogy. Perhaps the appeal of that argument is determined by our genes, not our reason.

Cognition and Triviality

Philosophers are not nearly as confident as Griffin that cognition and consciousness can be established to be important feature in the lives of humans. Even if we suppose that consciousness exists, we might be concerned that it is not as important as

Griffin thinks it is. Griffin describes a sentiment that he suggests lies behind the triviality objection:

> the claim that the distinction between conscious and unconscious mental states is an empty and meaningless one, at least when applied to non-human animals, because, they say, anything an animal does might equally well be done without any accompanying consciousness [Griffin 1992, p. 234].

Earlier I mentioned and dismissed the epiphenomenalist suspicion that cognitive states and processes are causally and explanatorily impotent on the grounds that a functional account of cognitive states and processes, such as in this thesis, is incompatible with such a view.

A related concern, however, is that cognition is real and causally efficacious, but not important. This problem is familiar to philosophers as the triviality objection. According to this objection, the cognitive nature of cognitive states and processes is trivial; what is important, allegedly, is the physiological actions which embody the cognitive states and processes. Given the obvious success of research in biology, and the relative paucity of success in cognitive ethology, we might conclude that the physiological aspects of 'cognitive' behaviour are more important, and more worthy of research, than the cognitive aspects. This objection differs from arguments in favour of reduction, in asserting that there really are cognitive phenomena. However, according to the triviality objection, the important aspects of cognitive behaviour can be explained without appeal to cognitive theory.

To establish the importance of cognitive activities, including consciousness, we must make a general argument that makes clear the theoretical advantages of cognitive explanations. The first claimed advantage of cognitive explanations is the supposedly obvious compatibility with evolutionary biology. This compatibility was best illustrated

in my consideration of the Jolly-Humphrey hypothesis. However, since such a radically differing view of cognitive phenomena as eliminative materialism is also compatible with evolutionary biology [Churchland 1984, p. 21], there seems to be no real advantage. Only if evolutionary biology is harnessed with Griffin's commitment to the idea that human behaviour is best explained and described by cognitive theory, is there any advantage for cognitive theory, since animal cognitivism would be continuous with human cognitivism.

The Ad Hoc Criticism

Another important criticism of cognitive theory in general is that it is *ad hoc*. According to this criticism, cognitive theory just makes up explanations to fit the data, rather than giving explanations within a systematic theoretical framework.[15] Ristau [1983, p. 374] considers this criticism and suggests that the lack of "firm auxiliary hypothesis" makes this criticism seem more important that it actually is. According to Ristau, firm auxiliary hypotheses will increase the theoretical resources available, and reduce the need to extend the theoretical framework to fit new observations.[16] Ristau suggests that once cognitive ethology matures, it will no longer appear to be *ad hoc*.

I can dispense with the *ad hoc* criticism without discussing the complexities of scientific rationality, since the same criticism pertains to behaviourism, the theoretical competitor to cognitive ethology. According to Sober, behaviourism is *ad hoc*; since a "causal chain" can explain any behaviours [Sober 1983]. At best, the *ad hoc* criticism can be made against either competing explanation.

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[15] The Churchlands make this criticism of folk psychology, in particular.

[16] A good, short, discussion of the role of auxiliary hypothesis within scientific theories, and scientific theories in general, is Hemple 1966.

Summary Of Chapter Two

In this chapter I have attempted to describe and resolve some problems involved in applying cognitive theory to animals. The rejection of the apartness thesis makes it possible to ascribe cognitive states to animals, but it also means that the traditional basis for this ascription is unavailable. This problem can be resolved by accepting a more modest research strategy that supposes that behaviours associated with cognition in humans can be associated with cognition in animals. The Jolly-Humphrey hypothesis was discussed as an example of how cognitive theory might be integrated into evolutionary biology. The Jolly-Humphrey hypothesis shows that useful hypotheses can be generated by supposing that animal behaviour might be explained by cognitive theories.

Chapter Three

Honeybee Cognition

The Honeybee

The honeybee, *apis melifera*, is a social insect with a symbiotic relationship with nectar producing plants. The honeybee relies on the plants for food and the plants need to be pollinated, a task at which honeybees are proficient. The honeybee collects and stores plant nectar and pollen in a hive where the bees maintain an environment conducive to the rearing of their young. Honeybees forage for nectar and pollen over a wide area, and are clearly well adapted for exploiting the food supply available from flowering plants.

Honeybees have a simple ganglionic, or distributed, nervous system that has been thought to place severe limits on the computational power of the bee brain. The honeybee brain has about 850 thousand neurones, compared to the 12 billion in the human brain [Mobbs 1985]. On the assumption that brain size is indicative of rough computational power, the honeybee has very limited computational means to perform its behavioural tasks. Recently, this assumption has been questioned:

> The general assumption until relatively recently has been that animals as distantly related as insects, with their compound eyes, crunchy exoskeletons, frenetic movements, supernumerary legs, and minuscule, distributed nervous systems have little to tell us about the cognitive processes of mammals. This picture has changed over the past decade; honeybees, at least, turn out to be more like birds and mammals, and birds and mammals more like bees than had previously been thought. Since the lines leading to arthropods and chordates probably diverged before the emergence of well-organized nervous systems, the similarities are the result of evolutionary convergence, and suggest that there may be one set of generally optimal processing strategies which work across a wide range of brain sizes [Gould 1990, p. 84].

According to Gould, the honeybee manifests complex behaviour that is not easily integrated into the minimalist interpretation explanations of animal behaviour, which relies on instinct and associative learning as the chief explanatory mechanisms.

Griffin's minimalist cognitivism.

Gould's research notwithstanding, it seems foolish to consider whether as small a nervous system as the honeybee could ever manifest cognition. For instance, Gould rehearses the sceptical view that honeybees are incapable of complex cognitive activity:

> some of the resistance to the idea that honey bees possess a symbolic language seems to have arisen from a conviction that 'lower' animals, and insects in particular, are too small and phylogenetically remote to be capable of 'complex' behaviour. There is perhaps a feeling of incongruity in that the honey bee language is symbolic and abstract, and, in terms of information capacity at least, second only to human language. Despite expectations, however, animals continue to be more complex than had been thought, or than experimenters may have been prepared to discover. Especially in ethology, it is difficult to avoid the unprofitable extremes of blinding skepticism and crippling romanticism [Gould 1975, p. 692].

It seems *prima facie* implausible to attribute the psychological states and processes to honeybees that have been associated with human language. For instance, Griffin writes:

> We must face at the outset the common and very widespread belief that no matter how complex and effective an insect's behavior is, it is too small and it's central nervous system too differently organized to be capable of conscious thinking and planning or subjective feelings [Griffin 1984:170].

The basis for this sort of scepticism is the commonly held belief that cognition must be a fairly complex and demanding computational activity. However, this belief is not grounded in any fact or theory. Indeed, it seems that such a view is a vestige of the apartness thesis. At this point in the discussion, it should be clear that we shouldn't assume anything about animal cognition.

Griffin's program includes generalising the lessons of the Jolly-Humphrey thesis and so rejecting the idea that cognition is a computationally demanding activity. Darwin, Romanes, and their critics all thought that animal mentality required prodigious computational power: Darwin thought animals had this power, his critics did not. Griffin clearly rejects this point of view when he suggests repeatedly that cognition might simplify the computational requirements on animal brains. This point of view I will call Griffin's minimalist cognitivism. The intent of minimalist cognitivism will be to reduce the burden of proof necessary to advance a cognitive explanation of the behaviour of animals whose brains are computationally limited.

Griffin argues that in the case of non-human animals with simple nervous systems, but with complex behaviours, we might consider cognition as an alternative to standard behaviourist explanations of the behaviour. Griffin thinks that a cognitive explanation of the behaviour is simpler, and he assumes that the simpler explanation is manifested in a simpler implementation in the brain of the animal. Thus, if it is true that a cognitive explanation of a honeybee's foraging behaviour is simpler, then according to Griffin, the computational requirements of the cognitive solution must be less.

Griffin suggests that cognitive explanations would be simpler because they can be implemented with relatively few rules where behaviourist explanations require case-by-

case rules. The behaviourist explanation requires more rules because it does not admit that the animal is capable of applying general rules to specific situations. Since the rules must therefore be specific, there must be enough to cover all the situations requiring rules. Griffin's cognitist explanation supposes that a very few general rules can be applied to a broad range of specific situations by a cognitive process.

An obvious criticism of this point of view is that it may be that Griffin is confusing his general position of realism with respect to explanations with realism with respect to the qualities of the explanation. That is, if an explanation is judged as better than its competitors because it is simpler, then a realist assumes that explanation is true; Griffin further assumes the theoretical simplicity, which commends the theory, is realised within the creature in question. Although a critic of Griffin's point of view would do well to investigate the basis for this assumption, I can find no reason to reject it, apart from general arguments against realism with respect to explanations.

A more serious problem for Griffin's minimalist cognitivism is that he ignores the problem of the computational requirements of implementing general rules. Presumably, additional computational capacity will be required to provide a capacity for the application of the cognitive model Griffin proposes. At best, this problem implies that there will be a threshold for Griffin's model. That is, animals will require a minimum computational capacity in order to implement general rules. At worst, this problem might offset any gains made by implementing a simpler set of rules.

Allen and Bekoff think that Griffin is looking for cognition in the wrong places. They describe his minimalist hypothesis as "Griffin's puzzling view" that "consciousness might help organisms such as honeybees by compensating for the limited processing

power afforded by their relatively small nervous systems" [Allen & Bekoff, 1997].

According to Allen and Bekoff, Griffin's

> suggestion seems odd in light of evidence suggesting that consciousness is a phenomenon arising from large, interconnected networks of neurons (Shallice 1988)--networks vastly bigger than anything found in a honeybee [Allen & Bekoff, 1997].

Although their suspicions are well founded, this criticism seems harsh, given their own explicit reluctance to define or describe 'consciousness'; preferring to leave it as a vague term. It is also harsh, given their own analysis of Griffin's misuse of terms like 'consciousness' and 'cognitive'. However, Griffin's minimalist cognitivism must explicitly reject the view that large neural networks are necessary for cognitive phenomena, although they may be sufficient. Griffin's position is compatible with Walker's [1983] observation that the largest computational load on the nervous system is sensory.

Unfortunately, Griffin isn't sensitive to the important distinctions between cognition, consciousness, and self-consciousness. I have already argued that Griffin relied heavily on the apartness thesis, so that searching for abilities thought to distinguish humans from animals was an important part of his program. Griffin's search for consciousness is equally flawed, since he relies on the conflation of cognition, consciousness, and self-consciousness, so that evidence of one is evidence of the other. I assume however, that not all cognition is conscious, and consciousness does not imply self-consciousness. Clearly then, evidence of cognition in honeybees is not evidence of consciousness, nor self-consciousness:

> The fact that it is not obviously incoherent to accept that bees have cognitive maps of their environments while simultaneously denying that bees have minds suggest that there is a distinction to be drawn here [Allen 1997].

I will below argue that bees do evince behaviour indicative of cognition. I don't want to necessarily claim that bees experience either consciousness or self-consciousness.

A defence of Griffin's Minimalist Cognitivism

Allen and Bekoff have argued that Griffin's minimalist hypothesis flies in the face of what we know about cognition. I think Griffin might argue exactly to the contrary. We have seen throughout earlier sections of this thesis that cognitive explanations have provided simple explanations of complex behaviour where behaviourist theory has been unable to do so. Indeed, in the case of linguistic behaviour, Chomsky has proven the infinite complexity of the competing behaviourist explanation. On this basis, we might accept the generalisation that cognitive explanations are simpler than their behaviourist alternatives. Griffin's minimalist hypothesis seeks a theoretical application for this generalisation: since cognitive explanations are generally simpler, whenever computational resources are scarce, cognition is a good competitor as an explanation of the behaviour. This theoretical point has been anticipated by Premack's [1978] slogan "apes cannot be behaviourists" because "they are not smart enough". I am suggesting that the cases of complex behaviour such as language where behaviourist explanations are inadequate are extreme cases of a more general pattern of the explanatory power and parsimony of cognitive explanations. Griffin ultimately is suggesting that the simplicity of cognitive explanations make for a superior explanation than adequate behaviourist explanations.

The argument I suggest for generalising the simplification effect of cognitive explanations has one obvious, likely criticism. It is vulnerable to a *reductio* argument. Taking seriously the idea that cognition is a likely explanation in cases where a

behaviourist explanation is adequate, but where computational power is limited, means that we might be forced to accept a cognitive explanation of the behaviour of computationally challenged single-celled organisms (and possibly non-organic objects such as thermostats). After all, the computational power of a thermostat is limited, so why would a cognitive explanation not be a better explanation of the behaviour of the thermostat?

This criticism can be avoided by observing that the animal or object in question must have the cognitive capacity to implement the general rules Griffin thinks can replace the specialised rules of the behaviourist explanation. Clearly, there will be a threshold effect. Obviously false attributions of cognitivism are thereby avoided.

Griffin's minimalist cognitivism significantly reduces the burden of proof to anyone wanting to make cognitive claims about animals with limited computational power. This is because there is a good case for thinking that the standard view that cognitive explanations require more computational power than behaviourist explanations is wrong.

Honeybee Behaviour: The Data.

Karl von Frisch was awarded the Nobel prize in biology for explaining the common observation that once a feeding source is discovered by one bee, other bees arrive in greater number, and less time, than can be explained by random searching behaviour. Aristotle theorised that the bees must communicate information about food sources within the hive by dancing [Haldane 1955]. Maeterlinck [1901] confirmed this hypothesis. He let a foraging bee find a feeding station and then return to the hive. When that bee tried to leave the hive to re-visit the feeding station, he captured it so that

it could not act as a guide to other bees. The hive mates of the original forager still found the feeding station sooner than can be explained by any other hypothesis than that they learned from the original forager at least the approximate location of the feeding station. Von Frisch [1950] also observed that bees returning to the hive, after visiting a feeding source, put on a behavioural display that correlates strongly with the distance and direction of the feeding source.

Further observation of this behaviour indicated that the bees which are present during this behavioural display often arrive at the feeding source soon after. Jonathan Bennett gives this description of what von Frisch called 'the waggle-dance':

> A honey-bee finds a source of sugar, imbibes some of it, and then returns to the hive where it performs a dance. Other bees observe this dance and then fly straight to the food, unaccompanied by its original discoverer. If we know where a given bee has discovered some sugar solution, and what the latter's concentration is, we can predict certain features of the bee's subsequent dance; and from watching a dance we can predict where the bees which observe it will subsequently fly [Bennett 1964, p. 8].

Von Frisch hypothesised that bees observing the dance display learn the distance and direction of the feeding source. Despite some controversy [Wenner & Wells 1990], this hypothesis has been generally accepted.

Recent research indicates that bees also use the dance language, in other contexts, to indicate the sources of other needed commodities such as resin, pollen, and water, as well as locations of potential hive cavities during swarming. It has also been shown that the vigour of the dance is correlated with the quality of the source and the need for the commodity within the hive [Seeley 1997].

Bennett and the Dance language

In 1964, Jonathan Bennett explored the philosophical implications of von Frisch's discovery. Bennett's analysis of honeybee behaviour concluded that honeybees' linguistic behaviour was not evidence that they were rational; Bennett therefor concludes that honeybee language is not sufficient for rationality. Bennett predicts that the behaviour of a rational honeybee, after a given linguistic experience, should include behaviours showing that the rules of the bees' language are rules 'to the bee'. Bennett concludes that since honeybee behaviour does not include these behaviours, honeybees are not rational.

In the course of his analysis, Bennett identified three apparent similarities between human languages and bee dance behaviour. Bennett argued that honeybees were not rational because the similarities between human and bee behaviour are merely apparent, and not genuine. This agreed with von Frisch's own assessment; he intended the description of the bee's behaviour as a language to be purely metaphorical [von Frisch 1950].

According to Bennett, the requisite similarities are:

1. the language must have "aboutness" which is expressible in rules;
2. the language must manifest understanding;
3. the language must have complexity and richness.
   [Bennett 1964, p. 12-14].

Bennett has argued the honeybee dance language does not have 1., and on his analysis this means that the honeybee language is not intentional. For the moment, I will follow his analysis.

Similarity 3, by itself, is not controversial; bees can combine information about distance, direction, and concentration to form a large number of different "sentences" [Bennett 1964, p. 13]. Indeed, since the bees dance can indicate any point in a plane, and planes are infinite, the bee dance can express an infinite number of 'sentences', although in practice they are limited to expressing distances within their flight range.[17] Bennett points out that:

> each apian 'sentence' falls under three rules at once (distance, direction, concentration)[18], and each rule correlates some variable quantity in the dance with some variable quantity in the world. There are thus very many ways in which the three rules can intersect in a single 'sentence', and this provides the bees not only with a large stock of kinds of 'sentence' but also with a 'language' in which there is a genuine capacity for 'saying' what may never have been 'said' before [Bennett 1964, 13].

Thus, similarity 3 seems to capture the same properties of language as do the novelty and generative features I described earlier.

Similarity 2 is likewise unproblematic, the bees must 'understand' the dances since the information contained in the dance has an effect on their behaviour. *A fortiori*, we humans can understand the language, since we can observe a dance and thus predict where a nectar source is:

> If a number of creatures are said to have a common language, then there must be a behavioural basis for saying that they understand one another's utterances in the language. For there to be such a basis, there must be a pattern of relationships... Such a pattern exists for the honeybees, for their behaviour obeys rules which enable us to predict where a bee will fly if we know what sort of dance it has just observed [Bennett 1964, p. 12-13].

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[17] In fact, the bees can indicate a denumerably infinite number of points, there are an infinite number of points in each direction, and in infinite number of discreet directions.

[18] Bennett was most probably unaware that the dance language can also indicate sources of things besides nectar, since this was not general knowledge at the time he published Rationality.

In short, there is a behavioural basis for concluding that the dance language satisfies the "must manifest understanding" requirement on two counts. First, the bees seem to use the information. Second, as Bennett observes, it manifests understanding for human observers, even if the bees have no capacity for understanding.

Similarity 1 seems to have been confirmed empirically; for instance, Brines and Gould [1979] detail the rules the bees conform to when exhibiting their dance behaviour. These rules define the relationship between the bees dances and what the dances are about; namely, nectar sources and the like. Thus Bennett concludes that "apian dances are language-like to this extent, for they fall under rules or generalizations which enable us to tell, from how a bee dances, where it has found food.

"Aboutness" and Intentionality

Bennett argues that while the bee dance behaviour appears to have rules, it does not have 'aboutness'. Bennett argues that the reason that the bee dance behaviour does not have 'aboutness' is that it does not have genuine rules that formulate the relationship between the causes of the bee's behaviour and the world. This relationship I have described earlier as an intentional relationship. That is, if there are rules 'to the bee' then these rules make an intentional relationship between the bees sensory appreciation of the world and the world.

To show that bees do not have genuine rules, Bennett distinguishes between rule-guided and regular behaviour. To understand the distinction between 'regular' and 'rule-guided', consider the law of gravity. It is merely metaphorically a 'law', and is certainly not a rule to guide the behaviour of bodies with mass. The law of gravity is really a description of an observed regularity. Human observers often conflate 'descriptions of

observed regularities' with 'laws of nature'. Bennett argues that in describing the

honeybee dance we must not confuse regular behaviour with rule-guided behaviour.

Bennett's argument is that the waggle dance is not linguistic because it is regular

(like gravity), not rule-guided (like a chess game).

> It looks on the face of it as though we can say of honey-bees that their
> dancing behaviour is covered by rules, but not that honey-bees have rules
> according to which they dance [Bennett 1964, p. 15].

It is only by confusing 'regular' with 'rule-guided' that there appears to be a similarity

between human and honeybee communication.

> (A) denial that the dancing phenomena are rule-guided... would be
> justified if the rules governing the dances had the status of causal laws
> which held for every fairly normal bee. For it would then be causally
> impossible for any such bee to break one of the rules, and rules which no
> normal bee can break are not rules which can guide the behaviour of any
> normal bee. For a creature to be correctly said to have a rule, it is
> necessary that it should be able to break the rule [Bennett 1964, p. 17].[19]

According to Bennett the "rules" of the bee dance are not rules at all, but merely

descriptions of regularities.

Bennett's analysis now proceeds by supposing that bees do meet the requirements

he proposes. He supposes that we would admit that the rules of the honeybee language

are rules 'to the bees' if they react to breaches of the rules. Bennett suggests that bees

would "be shown to be rule-guided and not merely regular if they evinced special

reactions when the rules that ordinarily cover their behaviour would be broken" [Collins

1968, p. 556].

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[19] Students of ethics should recognise a similarity between this argument and a certain issue in the free-will/determinism argument, where it is often argued that genuine free will requires that one 'could have done otherwise'. This argument has been much criticised by Dennett [Elbow Room] who argues that this is a false requirement. I believe the same criticism would be telling here, but do not develop the point.

Bennett supposes that bees might perform 'denials' as special reactions to breaches of their rules.

> There should be a recognisable kind of performance which a bee goes through if and only if it has just observed a dance, or a post-dance foraging flight, which it knows to be in breach of the rules. Let us adopt the fiction that honey-bees do have a kind of behaviour which conforms to this condition, and let us call any performance of the kind in question a denial [Bennett 1964, p. 18].

Bennett's view is that a performance of a denial would be evidence that the rules of honeybee language are rule-guided and not merely regular.

Honeybee denials would be analogous to a human's reaction to believed falsehood; for instance, having been told there is cheese in the larder, a disappointed human might return and say "you said there was cheese in the larder, but I could not find any" [Bennett's example, 1964, pp. 25f]. Merely returning from the larder with an attitude of disappointment would not count, according to Bennett, as a 'denial', since it could merely be a regular expression of disappointment correlated with feelings of hunger and anticipation of cheese.

Just like human denials, bee denials require a sensual confrontation with the contrary of what is being denied. Neither bees nor humans deny things they have no reason to think are false (at least not rational humans). Thus, the correct performance of a denial is when a bee is "sensorily confronted with a rule-breach, and by such facts about the world as make it a rule-breach" [Bennett 1964, p. 20]. Bennett suggests that bees that investigated the place indicated by the distance and directional information of a dance, but failed to find nectar, and then returned to the hive and communicated this failure, would be performing a denial. There is no evidence that bees actually do this. I will later argue that there is honeybee behaviour that ought to count as a denial.

Bennett gives us a useful description of the circumstances where a denial might

occur:

> A bee performs a dance which is rule-correlated with a certain place and a certain kind of food; observers of the dance fly to the dance-indicated place and find there no food of the dance-indicated kind; they then go through the characteristic kind of physical performance which we are calling a 'denial' [Bennett 1964, pp. 21-22].

However, a denial under these circumstances is still not adequate evidence that the rules

are rules 'to the bees'. This is because the denial itself might be merely regular, and not

rule-guided.

While Bennett admits that the bees might deny violations of their rules, it would

be impossible to tell if the denial was a result of a genuine, or merely an apparent, breach

of the rules [Bennett 1964, p. 25]. This problem manifests itself as a problem of tense

discrepancy. Bennett imagines how tense imprecision in bee dances might lead to

unnecessary denials when a bee might dance to indicate a food source that would be

exhausted by the time the dance followers could arrive at the site indicated [Bennett

1964, p. 23].

Unlike human language users, the bees cannot resort to a complex dialogue to

identify the source of the discrepancy. Human language users would soon conclude that

although the food had been present, it no longer is. Furthermore, Bennett concludes,

humans would consider relevant evidence that food had been present. To avoid this

problem, Bennett suggests how bees might avoid denying indicated foraging sites that did

contain food when it was visited by the bee who dance-indicated the location, but were

out of food by the time the dance observing bee arrived at that location. His suggestion is

to withhold denials in the absence of food at the dance-indicated site when there is

evidence that there was recently food there. This refinement is immaterial to my

argument, although the tense-discrepancy issue will reflect upon my comments later on predictive results of the lake experiments.

Bennett's refinement requires that bees are intelligent enough to evaluate evidence regarding the recent presence of food. Since I don't claim that actual bees are this intelligent, I cannot appeal to this refinement in my account. Consequently, I am faced with a regress. Denials that occur every time a dance-indicated foraging site does not contain food (or whatever) might be merely regular, and not rule-guided, behaviour. Only a rule-guided denial is a genuine denial, and only a genuine denial is an indicator of regular, not rule guided behaviour.[20] Consequently, using denials to identify rule-guided behaviour requires appeal to rule-guided behaviour.

Bennett discusses whether there is any other way to identify genuinely rule-guided behaviour. His answer, a denial that expressed the content of what is denied, would show that the bee understood that a rule was being broken. Such a demonstration of understanding indicates rule-guided behaviour without appeal to behaviour that may itself be merely regular. Conversely, denial behaviour that did not include the content of what is being denied would not unambiguously count as a denial, since it might merely be a behaviour caused by the breaking of a rule. In summary, the only grounds for definitive attributions of rule-guided behaviour, according to Bennett, is that denial behaviour includes an expression of the content of what is being denied.

In summary, Bennett's view is that the intentional relationship of 'aboutness' can only be correctly attributed to honeybees when the bees demonstrate denials which include expressions of the content of what is being denied.

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[20] De Grazia suggests that this is false, a legitimate denial might in fact be merely regular. I ignore this.

Following Bennett's analysis, a philosophically careful description of the behaviour of actual honeybees will not be in terms of 'rules' at all. Bennett outlines such a description of the dance behaviour:

> For the 'correct' performance of a dance can be explained in terms of the digestive state resulting from the concentration of the imbibed sugar solution, the degree of muscular fatigue resulting from the distance flown since feeding, and the optical state produced by the direction of flight from the food. Real bees do dance 'correctly', and their doing so is explicable in these terms. The apian behaviour which manifests 'understanding' of observed dances can be explained along similar lines: observation of a dance causes the observer to move in such a way as to attain a certain optical state, and thus to point in a certain direction; and it creates a bodily disturbance which causes flight and fades away after a certain length of flight... Again, something along these lines is the explanation of actual bees (Bennett 1964, p. 25).

This description of the bee behaviour seems to explain all the observations considered so far. According to Griffin, Bennett's description manifests:

> the conviction that all animal communication is a direct result of internal physiological states that are not under any conscious control. Animal communication is thus held to be comparable to human eye blinks, blushing... These do serve to communicate... but they are not intentional signalling employed for some conceived purpose [Griffin 1992].

This view of animal communication is compatible with the minimalist interpretation used by behaviourists.

Rejecting the Minimalist Interpretation of The Honeybee Dance-Language

Griffin considers the minimalist interpretation an inadequate view of honeybee communication; he argues that it cannot account for all the data. Seeing how Bennett's description is inadequate requires considering the full complexities of honeybee behaviour. To this end, a few remarks are required before considering the counter-examples to the minimalist interpretation favoured by Bennett.

Bennett does not think that honeybee behaviour can be shown to be rule-guided rather than regular simply because there are anomalies in the dance behaviour. Evidence of anomalous behaviour is not evidence that the behaviour is not regular. Bees do occasionally make "mistakes", and Bennett does not think that this is significant; indeed, regularities in biological phenomena are usually statistical, rather than absolute. Furthermore, just as English is a language despite my (occasional) ill-formed sentences, likewise, honeybee communication is a language despite anomalous bee behaviour. More importantly, English is not a language because of my mistakes, and the waggle dance is not a language because a bee might stumble while dancing.

Bennett recognises that he does not mean to insist that the dance behaviour is not a language because it could be given a physical explanation. It is, in principle, possible for human language to be given a physical explanation, a fortiori, so could honeybee language. I agree with Bennett that the mental-physical debate is tangential to the present question of whether honeybee dance behaviour manifests a genuine language.

Having set aside these secondary issues, I will consider whether Bennett's explanation, given in terms of physical regularities, is adequate to handle a more complex example of bee behaviour. Robacker, in his article "Man, Bee and Boojum", notes an interesting observation made in so-called 'detour' experiments:

> Von Frisch trained bees to fly around a ridge to a concentrated sucrose solution. The bees communicated to the other bees in the hive (by their waggle dance) a single distance and direction straight over the ridge rather than the course they themselves actually flew. Somehow, the trained bees had converted their crooked path around the ridge into one resultant distance and direction. How they accomplish this calculation is a matter for conjecture... [Robacker 1988, p. 181].

Can Von Frisch's observations, confirmed by J.L. Gould [1975, 1994], be handled by Bennett's explanation of the dance behaviour?

A simple comparison shows that Bennett's 'minimalist interpretation' explanation is too simplistic to incorporate this observation. Bennett's explanation relies on correlations between physical states and dance behaviour (regularities), and so cannot handle observations where the physical state does not agree with the observed behaviour. Although bees seem to report distance based on effort [reviewed by Gould 1976, p. 215], since headwinds and drag weights affect the distance reported, the distance reported is not always directly correlated with distance flown. Honeybees sometimes communicate a simple distance and direction after flying an indirect route; the physical state of fatigue, to use Bennett's terminology, is not directly correlated with the distance that would be predicted by observing the dance behaviour [Von Frisch 1950, pp. 173-186, also reviewed in Gould 1976, pp.221]. Likewise, the optical state of the bee caused by flying in a certain direction will not directly correlate to the direction indicated by the bee's dance, since the path the bee flew deviated from the 'bee line' indicated in the subsequent dance.

Bennett's description also does not handle the fact that bees use exactly the same dances, under different circumstances, to indicate distance and direction for other materials needed by the hive, such as resin (for repairing the hive), water, pollen (used for protein), and cavities suitable for colonisation by swarms. Presumably, there might be simple one-to-one correlations to contextual components of the honeybee dance that might be added to Bennett's explanation in order to solve this problem. Since Bennett doesn't address this issue, it seems reasonable to assume he would add these refinement to his account if he were aware of their necessity.

The bee dance which indicates a simple distance and direction even though the scout bee doing the dance travelled a compound, and hence longer, distance is analogous to one of the two ways I might give directions to a person looking for the university library. I might say either "the library is 350 metres North-West" or I might say "take a left at the administration building, and go straight until you come to the engineering building, then..." The bee dance obviously uses the first sort of direction, while Bennett's minimalist explanation is more similar to the second. Considering only the simple cases where the indicated direction and distance are the actual distance and direction travelled is like comparing the two types of directions I might give to the visitor when we are standing in sight of the library. In this special (though not infrequent) case, the directions to go "100 yards due south" and the directions to go "straight ahead" do not differ substantively, and so might be confused. The unusual, but important, test case where the distance and direction flown differ from the distance and direction indicated reveals the inadequacy of the minimalist interpretation. In short, the complexity of the behaviour in the case where a direct distance is indicated but where an indirect route was taken is evidence of rule-guided, and not merely regular, behaviour.

Bennett's explanation is only suggestive, and establishing that there is no possible minimalist explanation is more difficult than merely showing that the tentative explanation he gave is implausible. Bennett thought that his explanation showed that bees did not have a language, since the explanation of the behaviour could be given without invoking linguistic concepts. Showing that the dance behaviour is rule-guided and not regular is tantamount to showing that only explanations given in terms of rule-guided behaviour will adequately explain all the observations. My approach to this issue

will be to introduce new observations of honeybee behaviour which require descriptions in terms of rule-guided, and not merely regular, behaviour.

As a result of Bennett's work, bee researchers tried to see if bees could ever evince denial behaviour. Gould has developed several misdirection experiments designed to see if the honeybee dance language could meet the standard set by Bennett. I will discuss two of these experiments here; the 'blinded ocelli' and the 'lake' experiments.

Blinded Ocelli Experiments

The ocelli are the three simple eyes behind the complex eyes of the honeybee. When these eyes are blinded by painting them over, they become significantly less sensitive to light, so that the subject bee's foraging behaviour is significantly disrupted [Shricker 1965]. Bees inside the hive normally dance using gravity as a reference direction, but when exposed to light, they dance using the light source (normally the sun) as a reference point. Gould hypothesised that, while exposed to light, the dances of bees with blinded ocelli will use the direction of gravity as a reference while bees with normal ocelli will dance using the light source as a reference [Gould 1974, 1975, and 1976].

Gould predicted that 'treating' bees by painting their ocelli would have the following effects:

> When a light is used to reorientate dances, the intensity can be adjusted so that the untreated bees are completely reorientated without affecting the dance orientation of the less sensitive, ocelli-covered bees. By moving the light to different angles, it should be possible to send recruits off in any specific direction [Gould 1976, pp. 234-235].

When Gould performed this experiment, his hypothesis was confirmed. It is possible to misdirect honeybees to stations not 'intended' by the dancing bee. When honeybees were misdirected to empty feeding stations, they did not perform any behaviour equivalent to a denial, either at the feeding station, or upon returning to the hive.

The lake experiments

Gould and Gould's [1982, 1988] lake experiments were designed to test whether a bee would perform a denial in response to what Gould called 'impossible' dances. An impossible dance would be a dance indicating a food source where bees with experience of the local geography would not expect one to exist. To elicit 'impossible' dances, Gould and Gould provided a feeding station on a boat in a lake. They hypothesised that bees familiar with the local terrain might in some way 'deny' a dance indicating a food source in the middle of the lake. Gould and Gould trained a small number of bees to forage at this floating feeding station following the usual training regime developed by von Frisch. According to Gould and Gould, under appropriate controls, the 'lake' feeding station failed to attract any new recruits (foraging bees), although dancing in the hive [Gould 1990] indicated its existence. This seems to demonstrate that the bees do modify their behaviour in the face of a perceived 'false' dance.[21] Although it is unclear what Gould and Gould were prepared to count as a denial, this modified behaviour would not be adequate for Bennett, because he requires that the 'denying' bees must include the content of what they deny in their denial. Merely ignoring a dance does not satisfy this criteria, because it could be regular, and not rule-guided, behaviour.

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[21] The bees would fly over the lake to forage at feeding stations on the opposite shore, so the observations do not merely reflect a reluctance to fly over water.

I think that this behaviour should count as a denial. To make this plain, I require

further exposition of Bennett's argument in <u>Rationality</u>. Recall that Bennett argued that,

although honeybee behaviour can be described using rules, it does not follow that the

bees use the rules: "We cannot say that the bees have rules unless they somehow

manifest an awareness of their rules as rules" [Bennett, p. 18, 1964]. In short, the rule in

question must be a rule 'to the bees'. Recall also that Bennett suggested that we could

tell if the honeybees understood their rules if they responded to violations of their rules

with denials that included the content of what was being denied.

Bennett made it clear that a denial must be something more complex then just

evincing disappointment at finding a indicated foraging site without nectar:

> For we are now in a position to see that denials do not have to be
> described as performances which occur when and only when a bee is
> confronted by a food-situation which is inappropriate to its dance induced
> digestive state. Behaviour which at first seemed to be shaped like a
> manifestation of an awareness of a breach of the rules can now be seen to
> be shaped like a manifestation of physical disappointment [Bennett, p. 27,
> 1964].

At first glance, ignoring an 'impossible' dance is compatible with being regular, and not

rule-guided behaviour, because a denial that does not include the content of what is being

denied can be correlated with a physical state of the bee and so could be what Bennett

calls "regular" behaviour.

However, Bennett's reasons for requiring that a denial include the content of what

is being denied shows that ignoring an 'impossible' dance should count as a denial.

Ignoring an 'impossible' dance cannot be correlated with a physical state. According to

Gould's explanation of his results, a bee ignores an 'impossible' dance because of

predicted disappointment, not because of experienced disappointment. Predicted

disappointment cannot be a cause of regular behaviour, because the disappointment (if

the prediction is accurate) is never realised. The bee does not experience disappointment because, on the basis of its cognitive map, it does not expect to find nectar at the site indicated in the dance, even though the dancing forager is dance-indicating that food is available at that site.

I think the non-relationship between this bee behaviour and what is happening at the site indicated in the dance is symptomatic of the ephemeral relationships commonly recognised as being intentional. That is, in intentional relationships, there is no easy one-to-one correlation between the intentional content, and the subject of that content. In this case, there is no easy correlation between the behaviour of the bee that ignores the 'impossible' dance and the site indicated by the impossible dance.

In summary, although ignoring an 'impossible' dance does not include the content of what is being denied, Bennett's regress argument does not apply because the denial is rule-guided, and not merely regular, behaviour. Recognising the 'impossible' dance as impossible requires the application of a cognitive process, which cannot be correlated with anything about the world. Instead, it is correlated with what we take to be the bee's previous experience of the world.

Gould and Gould recognise that their research shows that bees are not rigidly "stimulus bound". For instance:

> bees appear to take stored information about physical features of their environment into account in their responses to the dances of hive members (Gould 1986); this degree of freedom in bees' responses has been cited as evidence for cognitive maps in bees (Gould & Gould 1994) [Allen & Bekoff 1997].

In the light of this and previous considerations, I conclude that honeybee language is rule-guided, and not merely regular behaviour. This implies that honeybee language is genuinely intentional.

Summary of Honeybee Language

Another test for distinguishing between genuine and merely apparent language is suggested by John Heil [1982]. Heil argues that using displacement in time or space distinguishes language from merely communicative behaviour. An inability to use tensed expressions, or expressions relating action at a distance is a symptom of merely communicative, but not linguistic, communication. We might tentatively adopt the principle that "language is not stimulus bound" [Hiel 1982, p. 406]. Interestingly, the bee dance seems to meet this criterion, since the dance language communicates information about a state of affairs not immediately present to the bees; "bee dances appear to be used to communicate information about the 'there and then' rather than only in the 'here and now'" [Bekoff & Allen 1992, p. 4]. Heil's example, "there is a bone in a butcher shop a mile up the road", does not differ importantly from 'there is nectar to be found at x distance at y direction' [Millikan 1984, p. 116-118]. The honeybee dance language seems to meet the displacement criteria for being genuine language.

However, recall Bennett's analysis of what he called the tense-discrepancy problem. According to Bennett's analysis, although the bees might appear to use tensed expressions, they cannot reconcile conflicting expressions by the correct application of tense to those expressions. Accordingly, Bennett concludes that honeybees do not really use tensed expressions. It would seem to follow that Bennett might object to an assessment of honeybee behaviour that attributes displacement to their language behaviour. Presumably, Bennett would offer a similar 'genuine vs. merely apparent' distinction as an account of honeybees do not manifest genuine displacement. Bennett does not offer any objection to the claim that honeybee expressions are physically

displaced from the subject of their expressions. I suspect he would argue that this is not significant.

We saw that Davidson suggested semantical opacity as an indicator of intentionality. While we might infer from a bee's dance that it believes that there is nectar available at a certain distance and direction, we certainly wouldn't think it obvious that the bee believes all the other things that are true of the site indicated. For instance, we wouldn't attribute to the bee the belief that that location has a particular historical significance, even though this might be true of that location. Therefore, the beliefs we would attribute to linguistically equipped honeybees do have co-extensional terms that are not intersubstitutable *salva veritate*. Clearly then, the beliefs we might reasonably attribute on the basis of the bee's dance are semantically opaque.

Ruth Millikan provides an alternate analysis of honeybee communication in her "Biosemantics" [1989]. Millikan provides a deflationary account of intentionality where content is made referential by its evolutionary history. Millikan concludes that bee dances are examples of what she calls "intentional icons" because bee dances are about the location of nectar which bear an evolutionary relationship to the honeybee. Her assessment is that the ancestors of current bees were able to pass on the behavioural trait of dancing to current bees as a consequence of the selective advantage afforded to those who exploited correlations between features of dances and the locations of nectar. Millikan concludes bees are intentional, but her theory of intentionality also includes trees, so we might wonder if her assessment is over generous [Allen 1997].

Is the waggle dance a genuine language? The honeybee seems to meet all the important criteria: the honeybee dance language seems to have infinity, novelty and generational power. It also appears to be semantically opaque. Gould's lake experiments showed that honeybee language was rule-guided and not merely regular, from which we can tentatively conclude that honeybee language is genuinely intentional.

Confirming the Cognitive Map Hypothesis

If bees can evaluate the information presented in dances, then they must have a mental or cognitive map that models their local terrain, otherwise they would only be able to 'deny' the dance after physically 'checking' the site indicated. If the bees had to check the site indicated, then their reaction would not be evidence for intentionality, because there would be no prediction component to the behaviour. Consequently, Gould's theory that bees use cognitive maps to evaluate proposed foraging sites is a crucial part of my argument that honeybee language is genuinely intentional. Rather than leave the cognitive map theory undefended, I want to follow some of Gould's subsequent research supporting this hypothesis.

Allen summarises the cognitive map theory, and also reviews the sort of criticism to which this theory has been subjected.

Gould (1986) has argued that individual honeybees form internal maps of their spatial environments that represent features such as the locations of large bodies of water relative to the hive. These maps are postulated to explain several phenomena, including that bees are not recruited by dances that indicate a direction and distance to a food source in the middle of a lake. (The dances were cleverly produced by exposing foraging bees to a boat with a food source on board that was gradually moved into the middle of a lake.) Challenges to Gould's hypothesis (e.g. Dyer 1991) do not typically start from the premise that bees are incapable of having cognitive maps; in other words, it is generally considered a reasonable (but perhaps false) hypothesis that bees utilize internal representations that integrate various spatial features of their environments. In contrast, many people would reject out of hand the claim that bees have minds, or that they have beliefs about the locations of bodies of water--thus rejecting mentalistic explanations of bee behavior [Allen 1997].

Supposing that bees can map their terrain requires us to upgrade our assessment of their

cognitive abilities:

One of the cognitive capacities that could be of considerable use to a social insect would be the ability to construct and use maps of the home range. To the extent that these maps could be used to plan novel routes, they would satisfy my intuitive definition of cognition [Gould 1990, p. 91].

Unfortunately, Gould has wisely avoided the complications involved in making explicit

his "intuitive definition of cognition":

Defining cognition, either formally or operationally, is fraught with difficulty, but I am going to take it to mean "planning" or "turning things over in the mind", a process I call "cognitive trial and error" [Gould 1990, p. 84].

It should be obvious however, that Gould clearly understands that there are serious

philosophical issues at hand.

The alternative explanation of insect navigation has been the landmark-vector

approach [Dyer 1991] where insects learn to associate certain flight paths with landmarks

and navigate from point to point, as described by Gould:

The typical model for invertebrate landmark-based navigation imagined that insects are supposed to store a set of "snapshots" taken along familiar routes which they could use to guide them in a dot-to-dot fashion to the goal, each snapshot pointing the way to the next [Gould 1990, p. 91].

This model is consistent with the minimalist interpretation, but inconsistent with Gould's observation that bees 'ignore' dances indicating impossible foraging sites. Gould's work rejects the landmark-vector strategy and confirms the cognitive-map hypothesis.

Gould argues first that the landmark-vector approach is only consistent with the minimalist interpretation if it is used exclusively; that is, if honeybees use the landmark-vector strategy in conjunction with the cognitive map strategy when navigating, then the minimalist interpretation is false. Likewise, if one strategy might be preferred in certain terrain this is still consistent with the cognitive interpretation; for instance, when there are plenty of landmarks available for navigation honeybees may rely more on the landmark-vector strategy, but still have available the cognitive map strategy. In short, the minimalist interpretation is falsified by evidence that bees **ever** use a mapping strategy. Conversely, evidence that bees do use landmarks does not automatically discredit the cognitive map hypothesis of honeybee navigation.

Crucial Test: Cognitive Maps Vs. Landmark-Vector Strategy.

Gould designed an experiment to decisively confirm either the exclusive landmark-vector explanation or the cognitive map theory. This experiment was designed so that honeybees which forage using only the landmark-vector strategy would respond differently than bees navigating with the use of cognitive maps. After letting foraging bees become experienced with a feeding station, bees departing for that feeding station were captured and transported to a release site with which they were unfamiliar. The landmarks at the release site were ambiguous with the landmarks at the hives location.

Gould predicted that if bees use the landmark-vector navigational system exclusively, then the bees would not have been able to fly directly to the feeding station, while if they used a mapping navigational system, they would be able to fly directly to the feeding station:

> if experienced foragers can use familiar landmarks to place themselves on a mental map, they should be able to depart directly for Site A (the feeding station); if they are restricted to sets of route-specific snapshots, the best they could hope to do is find Site B in their mental album and pick up the trail back to the hive, where upon they could restart the sequence of pictures leading to Site A. Another possibility is that foragers might not realize that they had been moved at all, and so continue at the azimuth normally employed in flying from the hive to site A [Gould 1990, p. 94].

Gould's experiment is predicated on the hypothesis that bees navigating by a cognitive map would produce different behaviour than if they only navigated by the landmark-vector strategy.

The results of Gould's test were that: "Foragers, in fact, departed accurately toward site A, and arrived there in about the time required for a direct flight" [Gould 1990, p. 94]. Since 'site A' is not the site bees would be guided to by using only landmark clues, Gould concludes from this test that bees do use a cognitive map as an aid to navigation.

Confirming the cognitive map theory of honeybee navigation is a double victory for cognitive ethology. First, it secures my conclusion that ignoring 'impossible' dances ought to count as a denial, despite Bennett's regress argument. Second, it is a textbook example of how a cognitive theory can be a better explanation of behaviour than a minimalist (behaviourist) theory. The cognitive theory that honeybees navigate using cognitive maps beat out the landmark-vector theory in the crucible of scientific study.

Honeybee Consciousness.

Having concluded that honeybee language is genuinely intentional, it is tempting to swiftly attribute mentality to honeybees. Griffin proposes that honeybee communication behaviour provides a 'window' through which we might 'look' at the honeybee mind. It is not clear how sense is to be made of this suggestion. It is difficult to see how we might use honeybee language to learn more about honeybee psychology. Allen and Bekoff [1997] describe Griffin's 'window' proposal as purely "allegorical":

> Nonetheless, there is a limit to which analyses of animal communication can provide the kinds of support that Griffin needs to bolster his notion of animal consciousness; communication is not a perfectly transparent window that permits access to other individuals' subjective states [Allen & Bekoff, 1997].

Griffin's 'dialogue' research paradigm seems doomed to the unlikely hope that animals are communicative and introspective enough to report their own thoughts and feelings. Unless honeybees can report their reasons for why they behave as they do, it is unclear what we could learn through any 'dialogue'.

Unlike Griffin, I have not equated cognition with consciousness, and self-consciousness. Therefore, I am not prepared, in light of my conclusions regarding honeybee language, to conclude that honeybees are conscious or self-conscious. Since, like Allen and Bekoff, I consider animal cognition an important area of research, finding that honeybees are cognitive creatures is an important result.

> The strategy of condemning cognitive ethology because it has trouble with consciousness is about as credible as the strategy of so-called creation scientists who seek to undermine astronomy by pointing out that astronomers can't explain what caused the Big Bang. There is much more to astronomy than that, and there is much more to cognitive ethology than questions about animal consciousness [Allen & Bekoff, 1997].

Like Allen and Bekoff, I have sought to elucidate another important feature of cognitive ethology besides consciousness; namely, intentionality.

Proponents of robust intentionality (those who think everything intentional is mental), are likely to draw from my results the conclusion that honeybees do have minds, and that they do know what they are doing. My own sentiments are thus. However, until a satisfactory account of robust intentionality can be given, I will cautiously withhold such judgement.

Conclusion

In this project I have tried to develop an understanding of how behavioural evidence for cognition and consciousness in honeybees is to be understood. I have developed an analysis of such behaviour within the broad outlines of the research program promoted by Donald Griffin under the name 'cognitive ethology'. Within this program, I developed Griffin's argument for thinking that cognition can be reasonably attributed to honeybees. I considered and accepted evidence that honeybees use cognitive maps to represent their known foraging environment. I rejected Bennett's argument that the honeybee dance language is merely apparent, and not genuine, language. I concluded that the Gould's lake experiments show that honeybee language is robustly intentional, since ignoring an impossible dance must be rule-guided, and not merely regular, behaviour.

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