

Animal Pain

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Which nonhuman animals experience conscious pain?¹ This question is central to the debate about animal welfare, as well as being of basic interest to scientists and philosophers of mind. Nociception—the capacity to sense noxious stimuli—is one of the most primitive sensory capacities. Neurons functionally specialized for nociception have been described in invertebrates such as the leech *Hirudo medicinalis* and the marine snail *Aplysia californica* (Walters 1996). Is all nociception accompanied by conscious pain, even in relatively primitive animals such as *Aplysia*, or is it the case, as some philosophers continue to maintain, that conscious experiences are the exclusive province of human beings? What philosophical and scientific resources are presently available for assessing claims lying between these extremes?

These questions are of practical as well as theoretical concern. The first section of this essay describes the practical issues both as they relate to weaknesses in current animal welfare law and as they relate to the insufficiency of existing philosophical work on pain to support the development of more adequate regulations. In the second and third sections I evaluate recent philosophical arguments for and against the attribution of conscious pain to animals, and explain how scientific work on humans and other animals undermines these arguments. The final section on the biological functions of pain argues that philosophical work in this area has relied on overly simple ideas about the functions of pain. Recent scientific work suggests a more nuanced view about the functions of pain, and provides us the best hope for progress in understanding the nature of pain and its distribution in the animal kingdom.

1 Animal Pain and the Law

Laws designed to promote animal welfare are coming under increasing legal scrutiny, and the trend is towards giving them broader scope. For instance,

the 1986 British Animals (Scientific Procedures) Act (ASPA) was amended in 1993 to extend protection to all mammals, birds, reptiles, fish, amphibia, and the common octopus, *Octopus vulgaris*. Bill C-17 introduced into the Canadian parliament in 1999 and superseded by Bill C-15 in 2001 sought to expand the scope of animal protections, representing the first overhaul of that country's laws since the 19th century. And in the United States, a 1999 lawsuit brought against the U.S. Department of Agriculture (USDA), the agency charged with enforcing the federal Animal Welfare Act (AWA), sought to challenge the agency's policy of exempting researchers maintaining rats, mice, and birds, from certain AWA requirements.² In an out-of-court settlement, the USDA agreed to extend its regulations to the previously exempted species, although this agreement was undermined by Congressional action in 2002 which amended the AWA so as to encode the exemptions for rats, mice, and birds (but mandates that USDA report to Congress in 2003 on consequences of including those animals). While many researchers see increased regulation as a threat to scientific progress, they also recognize that there is no scientific or theoretical justification for excluding rats or mice from regulation under the AWA.³ Taxonomic consistency requires that there be no arbitrary divisions between species, and if the octopus is deserving of protection under British law, it is also likely that other, less well-studied invertebrates are equally deserving of protection.

Further legal challenges to government policy seem likely, particularly in the United States given the success of animal welfare organizations pursuing this strategy. Definitions of animal pain, both conceptual and operational, would seem to be prime candidates for legal scrutiny. For example, although the United States' AWA does not itself define pain, written USDA policy defines a *painful procedure* as "any procedure that would reasonably be expected to cause more than slight or momentary pain or distress in a human being to which that procedure was applied."⁴ This anthropocentric criterion completely, albeit conveniently,⁵ sidesteps epistemological questions about how we can tell whether, and to what extent, members of other species experience pain consciously; but ultimately it is indefensible insofar as it can err in both directions. On the one side, certain procedures that do not cause pain in humans might well cause pain in animals—for example, while humans are oblivious to ultrasound, an ultrasonic signal at 22 KHz and 150 decibels might well be painful to a dog—thus animal welfarists have reason for concern about research on animals that involves such procedures. On the other side, there are procedures that a human would find painful, such as walking barefoot across a floor with a temperature of 45°C, that need not cause pain to a desert-adapted species such as a kangaroo rat—thus researchers might justly worry that legitimate research (on adaptations to desert living, for instance) could be inappropriately prevented if the anthropocentric criterion was strictly applied. There are also grey areas. Much basic research on pain perception consists of

administering electric shocks to rats. These shocks have been described as “moderately painful” by researchers who have self-administered the shocks (Grau 2002), but how can we know whether they feel this way to the ultimate subjects of the experiments?

Not only is the use of the anthropocentric criterion questionable policy, it is a policy that is likely to be questioned in the not too distant future.⁶ Those who oppose invasive research on animals would like to show that there are procedures that do not meet the anthropocentric criterion which nonetheless cause morally significant pain to animals, with a view to stopping such research. And while those in favor of animal research may argue that causing pain to animals is sometimes morally acceptable, most likely on utilitarian grounds, they nevertheless have an interest in developing species-specific criteria for assessing pain in animals so as to properly identify which cases require particular moral justification.⁷ Furthermore, as animal welfare protections are extended to cover a broader range of species than at present, the problem of developing species-suitable measures of pain becomes even more pressing. Indeed the proposed Canadian bill reverses the relationship between pain and protection: if passed it would make the capacity to feel pain the criterion for protection under Canadian law (Hughes & Meyer 2000, p. 50).

In the process of developing standards for assessing animal pain that can withstand legal scrutiny one might naturally turn to philosophers for criteria that can withstand philosophical scrutiny. There is a large philosophical literature on the *ethical* aspects of animal welfare which contains arguments that the capacity for conscious pain is properly attributed to nonhuman animals (Regan, 1983; Rollin 1989; Singer 1990; Varner 1998) and sometimes against such attributions (e.g., Carruthers 1992; see also Harrison 1991) but there is relatively little basic philosophy of mind on animal pain⁸ and what there is is of limited usefulness (or so I shall argue). Pain has, of course, been a central topic in the philosophy of mind. But despite the voluminous attention to pain, virtually none of what has been written is of use for the present purpose—that of designing a scientifically, philosophically, and legally defensible policy for the assessment of pain in non-human animals across the entire taxonomic range. Part of the reason for this is that philosophers of mind have been mainly concerned with pain in humans and have had little to say about other animals. But part of the reason lies in the fact that the philosophical study of pain, even in humans, lags behind scientific developments.

Although, thankfully, we have mostly passed beyond the days of discussing whether pain is identical to C-fiber stimulation, philosophers have much to learn about pain. Hardcastle (1999) performs a valuable service by summarizing much of the empirical literature for philosophers but some caveats are necessary. Hardcastle presents the “classical” two-track picture of the pain sensory system comprising a sensory-discriminative system that

projects to somatosensory cortex, and an affective-motivational system that projects to frontal lobes. Based on this picture, it is practically a piece of philosophical dogma that the affective aspects of pain can be completely dissociated from the sensory aspects.⁹ But according to a review by Coghill (1999), early work on which such claims are based conducted only minimal assessments of chronic pain (see, esp., p. 68). More quantitative studies of prefrontal surgery patients reveal a different story, especially when acute pain is included. Although spontaneous complaints of pain are certainly diminished and there is reduced ability to appreciate the significance of pain, there is actually decreased tolerance (hypersensitivity) for acute stimuli in these patients. Furthermore, the latest brain imaging techniques show that pain is processed by multiple regions, in a highly distributed system; there is no single brain region whose destruction completely abolishes the experience of pain, sensory processing occurs in areas classically associated with affect, and affect itself is at least a three-layer process with the prefrontal cortex being mainly important for the latest stage of processing. Although Coghill notes that “somatosensory components and affective components of pain can be experimentally separated” (Coghill 1999, p. 73) he argues that “current views that sensory-discriminative and affective-motivational dimensions of pain are processed in parallel by distinct neural structures cannot account for a number of aspects of pain.” (Coghill 1999, p. 67) The classical account, then, is at best just one of several competing theories, and at worst badly flawed. In either case it would be unwise to assume an understanding of pain on the basis of the classical picture.

It is a particular consequence, I believe, of the understandable difficulty philosophers face in keeping up with the latest scientific research, especially when it comes to assessments of animal pain, that the positions taken on animal pain tend to be at one extreme or another, for superficial understanding tends to foster simplistic views. On the one hand, there are those who think that the obvious similarities between humans and other animals make it a matter of common sense, beyond reasonable doubt, that animals experience pain consciously exactly as we do.¹⁰ On the other hand there are those who think that animals don’t experience pain consciously at all (e.g., Carruthers) or in anything like the same way we experience it, and that to think otherwise is worthless, untutored intuition. I shall say something more explicitly about arguments for this view in what follows, but first it is worth pointing out why such views are likely to have little practical impact on actual policy.

On the one hand, views that take animal pain to be obvious and straightforwardly detectable in animal behavior still leave substantial questions about forms of life that are relatively alien to our mammalian sensitivities, and even about differences among individuals and species within the class of mammals. While we may feel sure we know when a dog or a pig is

experiencing pain, our confidence in detecting pain in a blue whale or a hippopotamus¹¹ should probably be rather reduced, and even more so when trying to assess whether a bird, or a fish is in pain.¹² Invertebrate behavior is even harder to assess: does the common practice of boiling lobsters alive cause conscious pain?¹³ How are we to tell? On the other hand, neo-Cartesian views that make all nonhuman animals out to be zombie-like automata fly in the face of what is now, in Westernized societies, a two-century long trend towards more humane treatment of animals. The laws which have resulted from these movements are unlikely to be undone on the basis of philosophical arguments to the effect that, despite appearances, a whimpering dog is really not experiencing pain consciously or suffering, and should not be regarded as an object of moral consideration or sympathy for its pains. There is a clear need for case-sensitive criteria that can be applied to different species, rather than a wholesale acceptance or rejection of attributions of conscious pain to animals at a high level of taxonomic abstraction.¹⁴

Of course, the mere fact that there is a demand for a middle-of-the-road approach to animal pain does not show that the more extreme views are incorrect. It is incumbent on us as philosophers to assess the arguments that have been given in support of those views.

2 Animal Pain: Common Sense?

The philosophical literature provides two basic strategies for defending the view that nonhuman animals experience pain (and other sensations) consciously in much the same way as humans. One strategy is *inferential*: the presence of mental states is based on an inference from considerations of behavioral and neurological (anatomical, physiological, and biochemical) similarity, typically bolstered by claims about evolutionary continuity.¹⁵ The other approach is *non-inferential* arguing that our knowledge of animal consciousness derives directly from our interactions with animals (Searle 1998; see also Jamieson 1998). As Searle (1998, p. 49) puts it: "I do not infer that my dog is conscious, any more than, when I came into this room, I inferred that the people present are conscious. I simply respond to them as is appropriate to conscious beings. I just treat them as conscious beings and that is that."

The inferential strategy is usually taken to be an argument by analogy.¹⁶ The general form of such an argument involves correlating conscious experience in humans with a property (or set of properties) *P*, and arguing by analogy that other animals possessing *P* are also conscious in the relevant sense. This form of argument is prevalent in the animal rights and animal welfare literatures.¹⁷ As well as citing behavioral evidence, it is common to cite also the neurological similarity between humans and other animals (mammals and birds, at least) in the processing of nociceptive

signals.¹⁸ Finally, considerations of evolutionary continuity are thought to bolster the argument from other kinds of similarity either on grounds of explanatory parsimony or likelihood¹⁹ or on the grounds that such an “obviously” adaptive characteristic as consciousness is unlikely to be the sole property of our own species (Singer 1990, p. 11; cf. Regan 1983, p. 20ff). I shall have more to say about the adaptive value of conscious pain in the next two sections, but for now let me note that many of the adaptive stories told by philosophers (and many scientists) are too simple to be true.

There are a number of problems with the analogy argument for conscious animal pain. A common form of objection is that for every similarity between the behavior or neurology of humans and other nonhuman animals, there is a dissimilarity that can be used to deny the inference to conscious pain in nonhumans (cf. Nelkin 1986, p. 137). Human behavior is not identical to the behavior of other animal species, so it is open to critics to point out ways in which they are significantly dissimilar, for example by insisting on linguistic criteria for consciousness. Human brains are not identical to animal brains, and we have only rather limited knowledge of how and where conscious pain processing takes place in the human brain. Consequently, without a theoretical explanation of the connection between the observed similarity *P* and conscious pain, analogy arguments remain essentially weak.²⁰ Even when bolstered by evolutionary considerations of continuity between the species, the arguments are vulnerable, for the inductive form of the argument never entails that traits found in humans are shared by even our closest relatives.²¹

Another problem has already been noted above, namely that as we get to species that are relatively distant phylogenetically from humans, the similarities are fewer, and confidence in our ability to attribute conscious pain is correspondingly more tenuous. This point has been conceded by proponents of the inferential strategy. Thus, for instance, Regan (1983, p. 29) asks “Ought we to go further than this and attribute consciousness to nonmammalian animals?” and responds that “A systematic answer to this question is well beyond the scope of the present inquiry.” Given that the political objectives of many animal welfarists often begin with species most closely related to our own, their typical unwillingness to be drawn into this complicated issue is understandable; they believe that mammalian cases, and especially those of the great apes, are clear enough.

But others concerned to put the ethical discussion on as detailed a scientific foundation as they can have attempted to survey the empirical literature in order to develop a more comprehensive approach. Most recently, Varner (1998) has presented a table cross-tabulating six conditions labeled “relevant to consciousness of pain in the animal kingdom” with seven taxonomic groups, three invertebrate (earthworms, insects, and cephalopods) and four vertebrate (fish, herpetological fauna, birds, and mammals).²² The six conditions are: 1. the presence of nociceptors; 2. the

presence of a central nervous system; 3. whether the nociceptors are connected to the CNS; 4. whether endogenous opioids are present; 5. whether analgesics affect responses; and 6. whether the behavioral responses are analogous to those of humans. According to Varner's research, only in mammals is there evidence that all six conditions are present, although he finds strong evidence for five of the six conditions in birds, with the only question mark concerning condition 3, the attachment of the nociceptors to the central nervous system. Varner finds firm evidence, however, for the presence of endogenous opioids (condition 4) in six of the seven groups, with the only question mark being under the cephalopods. Varner considers different ways of weighting these factors, tentatively accepting the conclusion of Smith and Boyd (1991, p. 64) that "the most obvious divide is between the vertebrates and the invertebrates," but he qualifies this by saying that "This consensus statement may be false; it may be that some of the lower vertebrates are not conscious of pain." (Varner 1998, p. 54)

Varner's admirable effort to build an empirical case for a dividing line somewhere near the vertebrate/invertebrate divide nevertheless illustrates the hazard I mentioned above, of philosophers risking much by building theories on scientific results that may quickly become obsolete. Although his table indicates firm evidence for the presence of nociceptors only in mammals and birds, the presence of nociceptors in a marine snail has already been noted above, and there is also firm evidence for nociceptors in frogs (Downes et al. 1999) whereas Varner indicates (with a minus sign in the table) that there is firm evidence against nociceptors among the herpetological fauna. Thus it seems rather likely that the indicators in Varner's table noting the lack of evidence for nociceptors among invertebrates, fish, and amphibians and reptiles represent just that—namely a lack of evidence, not a lack of nociceptors in those groups. The direction of discovery here seems uniformly towards identifying *more* similarities between diverse species, especially in the domains of anatomy and physiology. What's left, then, are behavioral differences. But to draw strong distinctions primarily on the basis of behavioral differences such as the tendency to vocalize when in pain seems like a chauvinistic response favoring forms of life that are similar to our own. Of course, there are other behavioral comparisons besides vocalizations, particularly in the domain of learning (discussed further below) and in behavioral responses to pharmacological interventions, but in the absence of a guiding theory, it is virtually impossible to decide how to weight these various dimensions of similarity for the purposes of constructing an argument by analogy to the human case.

Some proponents of the view that animals do consciously feel pain have argued that the argument from analogy is a vestige of a dualistic worldview, according to which we must infer "hidden" mental causes from "visible" behavior and anatomical properties. Searle argues that thoroughly abandoning the Cartesian picture also requires one to abandon the epistemological

puzzles it spawns, and which, he asserts, have no purchase in the real world of interactions between people and animals.

This position is hard to articulate without seeming to beg the question. As Searle summarizes his view, “it doesn’t matter really *how* I know whether my dog is conscious, or even *whether* or not I do ‘know’ that he is conscious. The fact is, he is conscious and epistemology in this area has to *start* with this fact.” (Searle 1998, p. 50) No doubt my assertion that this seems to beg the question is sufficient to convict me, in Searle’s eyes, of being in the grip of one of the “excrescences” of Cartesian dualism.²³ Searle describes his own view as *biological naturalism* according to which “Mental phenomena are caused by lower level neuronal processes in human and animal brains, and are themselves higher level or macro features of those brains.” (Searle 1998, p. 45) As far as I can tell, this assertion amounts to little more than a hunch on Searle’s part, even though it is presented as fact. What scientific research supports the claim that it is “lower level” neural phenomena (whatever this means) that produce consciousness, either in humans or animals? Searle cites none, and blithely admits that “we don’t yet know the details of how it works” (*ibid.*) while reasserting that nevertheless we do know *that* it works. This seems little more than an argument by repetition, which might work for indoctrination into the cult, but is hardly likely to convince those who think that there is a serious epistemological question about the distribution of animal consciousness.

Ultimately, Searle’s proposal for settling the distribution question boils down to an instruction to wait until we know more about the neural basis of human consciousness and then apply the resulting “electrochemical” formula as a criterion for testing hypotheses about consciousness in other species such as snails and termites.²⁴ But there are a couple of problems with this suggestion, especially in light of Searle’s evident certainty that his dog is conscious. First, as I already mentioned, the latest functional imaging evidence shows very widespread neural activity in human brains experiencing pain; it is therefore arguable whether any electrochemical formula will be forthcoming to describe this activity, even one that is “extremely complicated” as Searle envisages, and even if such a formula is forthcoming, it is far from clear that the formula would exactly apply to a dog, with its markedly smaller relative cortical volume, etc. Second, as a practical matter, the redesign of animal welfare legislation or institutional oversight of animal research cannot wait for science “to establish conclusively the causal bases of consciousness in humans.” (1998, p. 47) Even accepting Searle’s certainty about consciousness in his pet dog, there are animals in captivity and used for research for which there is an acute epistemological question of whether they experience pain. Of course, the majority of research animals are mammals, and Searle is already certain about their consciousness, but this does not license us to defer adjudication on the others.

I conclude that for the very real and practical problem of determining which animals suffer conscious pain, neither the inferential approach thus far articulated, nor the non-inferential approach is adequate. In the absence of a much more advanced neuroscience (and possibly even in its eventual presence) we are forced to make inferences about animal pain on the basis of various similarities and dissimilarities of unknown and unspecified significance, between animal behavior and neurology, on the one hand, and human behavior and neurology on the other. Merely cataloging similarities or dissimilarities to make a case for or against animal pain has questionable argumentative force.

What is needed is a more theoretical case for relating certain features of organisms to conscious experience. But there has been very little attention to providing such a case by philosophers of mind; indeed recent attention to consciousness by philosophers of mind has been almost exclusively concerned with ontological questions about the nature of consciousness rather than with the epistemological questions on which we are focusing here.

There are exceptions, however, and two philosophers who have made explicit theoretical arguments about our knowledge of animal consciousness, and specifically about animal pain, are Tye (2000) and Carruthers (2000). Tye's account of phenomenal consciousness characterizes it in terms of immediate sensory representations that are poised to make a direct impact on beliefs or desires (Tye 2000, p. 177). Tye cites flexible, goal-driven behavior in a variety of taxa (e.g. honeybees, fish) to support the claim that they are phenomenally conscious. Evidence for conscious pain experiences in animals would be provided, on Tye's view, by evidence of change in belief or desire manifested in non-reflexive behavior in response to nociceptive stimuli. Although he does not discuss which particular species are subjects of conscious pain, he remarks that the sense in which "simple creatures" experience pain is has no strong implications for ethics, because "they never *suffer*. Suffering requires the cognitive awareness of pain." (p. 182) Thus, despite presenting a first-order account of consciousness, Tye invokes a higher-order account of suffering which would seem to be unavailable many nonhuman animals. Unlike Tye, Carruthers (2000) develops a higher-order thought theory of consciousness itself, which he has used as a basis for argument against any form of phenomenal consciousness in non-human animals. This argument is the subject of the next section.

3 Animal Pain: Untutored Intuition?

A large amount of what we know about the mechanisms underlying pain perception is based on animal studies.²⁵ Of course, this scientific research is precisely of the kind that Nagel (1974) argued could never tell us *what it is like* to be the subject experiencing pain consciously. But at this point our question is not so much *what* it is like, but whether it is "like" anything at

all, for the radical Cartesian thesis that has been resurrected by Carruthers is that animals experience nothing consciously at all.

Carruthers' arguments for this view have changed over the years since his initial expedition into this field (Carruthers 1989, 1992). In the first instance his argument against attributing phenomenal consciousness to animals was just another similarity argument, but for a conclusion contrary to the usual version. Essentially he argued that animal behavior was similar to that of humans engaged in behaviors that are not mediated by conscious states. Thus he argued that all animal behavior can be assimilated to the non-conscious activities of humans, such as driving while distracted ("on autopilot"), or to the capacities of "blindsight" patients whose damage to visual cortex leaves them phenomenologically blind in a portion of their visual fields (a "scotoma") but nonetheless able to identify things presented to the scotoma.

The comparison of animal behavior to the unconscious capacities of humans could be criticized on the grounds that it was based only on unsystematic observation of animal behavior—indeed on nothing more than Carruthers' intuitions about what animals are capable of. A more empirically grounded investigation of animal behavior would reveal that there is not a very close analogy between animal behavior and human behaviors associated with these putative cases of unconscious experience (see also Jamieson & Bekoff 1991). For instance, it is notable that the unconscious experiences of the events that occurred during automatic driving are not remembered by their subjects, whereas there is no evidence that animals are similarly unable to recall their allegedly unconscious experiences. Likewise, blindsight subjects do not spontaneously respond to things presented to their scotomas, but must be trained to make responses using a forced-response paradigm. There is no evidence that such limitations are normal for animals, or that animals behave like blindsight victims with respect to their visual experiences, and indeed there is evidence that monkeys with surgically-induced damage to striatal cortex show many of the same differences from normal monkeys as blindsight patients show from normally sighted humans (Stoerig & Cowey 1997).²⁶ In other words, this similarity argument *against* conscious experience in animals is as weak as its counterparts *for* animal consciousness, being vulnerable to objections based on pointing out dissimilarities between the alleged analogues.

Carruthers no longer appears to place much weight on his similarity argument against animal consciousness and has instead been developing a more complete theory of phenomenal consciousness in terms of higher-order thought processes. Carruthers provides numerous arguments for why his higher-order account is superior to first-order theories, and it is beyond the scope of this paper to evaluate those arguments directly. Instead, I will focus on the way in which Carruthers exploits his higher-order account to denying that nonhuman animals have conscious

experiences. According to the higher-order thought account, phenomenal consciousness requires the capacity to think about, and therefore conceptualize, one's own thoughts. Such conceptualization requires, according to Carruthers, that the subject has a theory of mind. And, Carruthers maintains, there is little empirical evidence for any nonhuman animals possessing a theory of mind (with the possible exception of chimpanzees). Hence, he believes there is no basis for attributing phenomenal consciousness to them.

I think that this argument should be less than convincing on at least two counts. First, the evidence against some animals having a theory of mind is not as clear as Carruthers maintains, although I shall not insist upon this point. Second, Carruthers' linkage of theory of mind to phenomenal consciousness is based upon an evolutionary story about the function of phenomenal consciousness that, I shall argue, is too weak to support the argument because it fails to account for the range of human conscious experience.

The systematic study of self-consciousness and theory of mind in non-human animals has its roots in an approach to the study of self-consciousness pioneered by Gallup (1970; see also Gallup et al. 2002). It was long known that chimpanzees would use mirrors to inspect their images, but Gallup developed a protocol that appears to allow an objective determination of whether it is merely the mirror image per se that is the object of interest to the animal inspecting it, or whether it is the image *qua* proxy for the animal itself that is the object of interest. Using chimpanzees with extensive prior familiarity with mirrors, Gallup anesthetized his subjects and marked their foreheads with a distinctive dye, or, in a control group, anesthetized them only. Upon waking, marked animals who were allowed to see themselves in a mirror touched their own foreheads in the region of the mark significantly more frequently than controls who were either unmarked or not allowed to look into a mirror. Gallup's protocol has been repeated with other great apes and some monkey species, but besides chimpanzees only orangutans consistently "pass" the test.

Gallup interprets the significance of this procedure as having the capacity to show that "Animals that pass the mirror test are self-aware and thus can infer the states of mind of another individual." According to Gallup (1998) "self-awareness, consciousness and mind are an expression of the same underlying process, so that organisms aware of themselves are in a unique position to use their experience as a means of modeling the experience of others." It would take us too far afield here to assess the merits of Gallup's interpretation, but if correct, the success of chimpanzees on the mirror self-recognition task would give some reason to maintain that they are phenomenally conscious on Carruthers' account. The failure of other species that have been tested to pass the test might also constitute *prima facie* evidence against their consciousness on Carruthers' account. But such conclusions must be treated with caution. There is, for

instance, evidence that gorillas' "failure" on the mirror mark test may be due to lack of motivation rather than a deficit in cognitive ability (Shumaker & Swartz 2002).

Carruthers suspects that even chimpanzees might lack theory of mind, and therefore (on his view) lack phenomenal consciousness, partly on the basis of some ingenious laboratory studies by Povinelli (1996) showing that in interactions with human food providers, chimpanzees apparently fail to understand the role of eyes in providing visual information to the humans, despite their outwardly similar behavior to humans in attending to cues such as facial orientation. But the interpretation of Povinelli's work remains controversial. Hare and colleagues (Hare et al. 2000; Hare & Wrangham 2002) conducted experiments in which dominant and subordinate animals competed with each other for food, and concluded that "at least in some situations chimpanzees know what conspecifics do and do not see and, furthermore, that they use this knowledge to formulate their behavioral strategies in food competition situations." They suggest that Povinelli's negative results may be due to the fact that his experiments require the animals to reason about cooperative chimpanzee-human interactions that are less natural than the competitive chimpanzee-chimpanzee interactions which provide the conflicting evidence.

Carruthers neither endorses nor outright rejects the conclusion that chimpanzees are phenomenally conscious. Given the uncertainty, he is well-advised in the tentative manner in which he puts forward his claims about chimpanzee consciousness. But even if we grant Carruthers that possession of a theory of mind is rare among nonhuman animals, his argument that possession of a theory of mind is the right criterion for attributions of phenomenal consciousness, which is based on a conjecture about its evolution, is rather too weak to support his negative conclusions about animal consciousness.

Suppose we grant Carruthers that some sort of higher-order account of consciousness is preferable to any first-order account, how might we adjudicate among different higher order accounts? It is here that Carruthers brings evolutionary considerations to bear. In Carruthers' story, phenomenal consciousness emerges as an evolutionary "by-product, not directly selected for" (Carruthers 2000, p. 230) of a two-stage selection process: First there was selection for first-order sensory representations (unconscious experiences), then there was selection for a "mind-reading" capacity which required conceptualization of mental states. On Carruthers' view, once the organism's own first-order sensory representations become directly available for conceptualization they are *de facto* phenomenally conscious. Once experiences become phenomenally conscious, then further adaptive benefits may follow—particularly, Carruthers thinks, for making appearance-reality distinctions (*ibid.*, p. 232; see also Allen & Bekoff 1997, chapter 8; Tye 2000, p. 184).

In the following passage, Carruthers appears to offer this story as a completely general account of the evolution of phenomenal consciousness:

Now the important point for our purposes is that the mind-reading faculty would have needed to have access to a full range of perceptual representations. It would have needed to have access to auditory input in order to play a role in generating interpretations of heard speech, and it would have needed to have access to visual input in order to represent and interpret people's movements and gestures, as well as to generate representations of the form, 'A sees that P' or 'A sees that [demonstrated object/event]'. Mere conceptual events wouldn't have been good enough. For often what needs to be interpreted is a fine-grained gesture or facial expression, for which we lack any specific concept. It seems reasonable to suppose, then, that our mind-reading faculty would have been set up as one of the down-stream systems drawing on the integrated first-order perceptual representations, which were already available to first-order concepts and indexical thought. . . . Once this had occurred, then nothing more needed to happen for people to enjoy phenomenally conscious experiences, on a dispositionalist HOT [higher-order thought] account. (Carruthers 2000, p. 231)

In this passage, the phrase "access to a full range of perceptual representations" seems to be playing a dual role. First, although only two forms of perception—hearing and sight—are mentioned explicitly they seem to be standing service for all the forms of perception which give rise to phenomenally conscious experience, and the full range of perceptual representations should therefore encompass odor, taste, and touch, as well as nociception and other somatic sensations. Second, Carruthers' reference to those elements of perception for which we lack specific concepts indicates another sense in which the range of perceptual representations available for second-order thought is supposed to be construed broadly.

Although questions can also be raised about the claim in the second sense, my concern here is with the first notion of breadth—the idea that Carruthers has outlined an account of the evolution of phenomenal consciousness that applies across a wide variety of experiences from different perceptual systems. We have a huge range of phenomenally conscious experiences, from pains and orgasms, to the taste of sour milk and the feeling of breathlessness caused by the thin air and staggering view from atop a snow-capped mountain. I shall argue that Carruthers' account fails to explain why we are phenomenally conscious in all the ways that we are. In other words, I shall put pressure on the alleged generality of his account.

In the quoted passage, Carruthers draws attention to interpretive acts based on speech and gesture, as well as to a more general class of attributive acts that seem to have less to do with communication per se. Because intentional communication between humans takes place predominantly in the modalities of hearing and vision (and perhaps to some extent using touch), Carruthers' focus on these two modalities in the quoted passage

seems designed to enhance the plausibility of his thesis that interpretation (in the sense of mental attribution) constitutes a driving force for evolution. But the thesis is much less plausible with respect to other sensory modalities, particularly smell and taste, not to mention the many forms of somatosensory perception. The way others look to us, sound to us, and the sensations they produce when they touch us are all possible targets of interpretation. In contrast, there seems little to interpret regarding others' mental states in the way they smell and taste to us, nor in the way our stomachs feel when we have not eaten for a while. I conclude that the mind-reading faculty has no need for access to smell and taste, nor to many somatosensory sensations, for interpretive purposes.

What about more general forms of mental state attribution? Well, it seems trivially easy to think of scenarios in which it would be adaptively useful to know what another individual is smelling or tasting or sensing about about their own bodies. And we also know that natural selection can operate on very small margins, so it is not out of the bounds of possibility that there could have been selection for mind reading with respect to smell and taste. But this just-so story needs fleshing out, especially in light of the fact that it is not a foregone conclusion that these perceptual systems should give rise to phenomenally conscious experience, given that there is at least one perceptual system, the vomeronasal system,²⁷ which apparently responds to pheromones and affects human behavior but with respect to which we utterly lack phenomenal consciousness. Indeed it seems much more straightforward to think of cases where it would be adaptively advantageous (not to mention potentially pleasurable) to know whether one's pheromones have been detected and are generating an intense desire for intercourse in a conspecific, than it is to think of adaptive scenarios for more mundane odors. It is far from obvious why we have phenomenally conscious smell and taste but are oblivious to "vomeroolfaction".²⁸

At best, then, the evolutionary story for phenomenal consciousness with respect to taste, smell, and many somatic sensations, is not proven. None of these senses is important to interpretation, and a weak just-so story is all that has been suggested to explain why we should have evolved the ability to attribute gustatory, olfactory, and somatosensory states to others. It might be thought that this case can be made stronger by pointing to facts such as that my knowledge that some food type tastes bad to you, might make me less inclined to eat it myself. But I see no reason for thinking that the adaptive advantage that accrues from lowering the chances of poisoning oneself needs to be mediated by conceptual recognition of the gustatory experiences of others, when the very same advantages could be derived simply by learning some non-mental facts about their reduced tendency to ingest this type of food. Of course, the capacity to attribute such perceptual states could be coopted once it had evolved (although common scenarios involving deceptive manipulation of appearances are harder to imagine for

taste, smell, and somatic sensations, than for sound and vision), but Carruthers is supposed to be providing us here with an explanation for initial selection of the capacity to attribute such states to others, not its subsequent cooption.

The point is not, however, to trade just-so stories. Rather, it is this: the fact that the vomeronasal system is devoid of phenomenology shows that there is no guaranteed connection between phenomenal consciousness and any given behavior-guiding perceptual system, even among mind-reading creatures such as ourselves. Thus we are entitled to demand from Carruthers a particular and specific explanation for phenomenal consciousness with respect to each of the separate perceptual systems. It is not good enough to say that the “mind-reading faculty would have needed to have access to a full range of perceptual representations” for the mind-reading faculty does not in fact even have access to the full range of perceptual systems.

Additional complexity in the relationship between perception and conscious experience seems unaccounted for by Carruthers’ evolutionary story. For instance conscious experiences of flavor (distinct from taste) are due to a synthesis of gustatory and olfactory inputs, and effects such as the McGurk illusion show that conscious experience of speech can be affected by the visual perception of lip movement (McGurk & McDonald 1976)—the simple act of closing one’s eyes can change which phoneme is consciously heard given identical aural input. Other modalities do not interact in this way; thus, for instance, the phenomenology of vision is not, as far as we know, influenced by olfaction (although flavor perception may be affected by vision). Carruthers might argue that these kinds of phenomena are determined by perceptual systems before the perceptual contents become available to higher-order thought and that therefore it is not incumbent upon his theory of consciousness to explain them. But given, for example, that olfactory and gustatory neurons project to separate brain structures, and that odor and taste can be distinguished in conditioned learning experiments,²⁹ there can be no straightforward explanation with respect to “mind-reading” of why phenomenal consciousness should contain synthetic experiences of flavor rather than concurrent experiences of taste and odor.

Carruthers’ response to these complaints³⁰ is to argue that these issues arise at a stage of conceptualizing and planning that is prior to phenomenal consciousness. He agrees that there are interesting questions about how the evolution of these first-order capacities might entail the kinds of sensory integration (or absence thereof) that I have outlined, but he locates these capacities in a “single functionally-defined short-term memory store” whose contents, as a whole, would be available to a second-order, mind-reading system. If the latter requires only visual and auditory information to carry out its functions, on Carruthers’ account it nonetheless has access to the contents of other senses as a byproduct, although presumably not to vomerolfaction because this, for whatever reasons, are not provided to the

conceptual/planning apparatus. Why should there be access to everything in the store? Carruthers suggests it is because “a single perceptual short-term memory system is simpler—and less costly to build and maintain.” (ibid.) But this speculation doesn’t bear much weight neurologically, for we know that one of the fundamental principles of neural architecture is that connections are maintained to the degree to which they are used. Given, as I have argued, that there’s not much use for olfactory or gustatory information in mind-reading, it’s hard to see how an organism could maintain energetically expensive neural connections connecting the mind-reading system to contents of the first-order memory store that are not used for its function. At the very least, more justification is needed to support Carruthers’ speculation.

I have argued that the evolutionary explanation of phenomenal consciousness delivered by Carruthers is rather weak. This conclusion is compatible, however, with his claim that he has provided a better evolutionary explanation for phenomenal consciousness than others, for the truth of this claim depends on the relative strength of the alternative accounts. But I believe that Carruthers gives too short shrift to at least one of the alternatives he considers and rejects, specifically that a higher-order capacity for conscious discrimination between different experiences might aid learning. (See Carruthers 2000, p. 215, first item). Carruthers claims that learning to avoid harmful events is possible without higher-order representation of pain states (i.e., without consciously experiencing pain, on his account) for all that is required is the (first-order) capacity to distinguish painful stimuli (i.e., stimuli that trigger nociception) from other stimuli such as tickles. It is “hard to see” the point, he writes, of “discriminating between experiences of pain and experiences of tickling . . . in the absence of a capacity for [higher-order thought].” (2000, p. 215)

Carruthers provides no empirical citations to support the claim that learning to avoid noxious stimuli does not depend on consciousness, although it is, in fact, borne out to a certain extent by work on spinal nociceptive mechanisms in rats. Rats whose spinal cords have been severed cervically show quite sophisticated kinds of associative learning in response to noxious stimuli, such as electrical shock, applied to the hind legs (Grau 2002; see next section), even though we can be sure that no signals are reaching the brain, and reasonably certain, therefore, that there is no conscious experience of pain associated with these stimuli.³¹ The spinal cord distinguishes noxious stimuli from other stimuli, and adaptive changes in behavior result. It is true, therefore, that learning about noxious stimuli can occur in the absence of conscious awareness of pain.

Carruthers is correct to reject simplistic adaptive hypotheses about the function of phenomenally conscious pain. Phenomenally conscious pain is not required for withdrawal from noxious stimuli, nor is it required for associative conditioning of pain responses. Yet learning, even associative

conditioning, is not a unitary phenomenon; operant learning seems to require the brain (Grau 2002), as do some forms of classical conditioning (see next paragraph). For pain-related learning, it is by no means clear that these more advanced forms of learning can be sustained on the basis only of “first-order information-bearing states differentially caused by tissue damage in the one case, and stroking or tickling in the other” (Carruthers 2000, p. 215), for while such states may indeed be sufficient to support discriminative avoidance of the noxious stimuli in some circumstances, the varieties of organismic adaptation are far more subtle than Carruthers lets on. One of the chief puzzles of conscious pain is its relative independence from tissue damage.³² Whatever role the internal state is playing is much more complicated than mere correlation with tissue damage³³ although this does not rule out detection of such damage as *one* of its functions. It is also the case that pain has an evaluative component—we can decide, often in retrospect, whether a certain level of conscious pain was worth the reward or reasonably correlated with the danger, and such evaluations affect subsequent behavior (i.e., consciously mediated learning results). Here, the point may not be whether there is a capacity to distinguish between experiences of pain and experiences of tickling, as Carruthers puts it, but whether there is the capacity to discriminate among different pain experiences having different intensities. Since these different intensities are variably related to tissue damage, it might very well be adaptive to discriminate between the painful experiences *as such* so as to allow independent assessment of the severity of the underlying conditions that they purport to represent.

Although not directly related to pain, another intriguing finding linking the brain (specifically the hippocampus) to a sophisticated form of learning derives from Clark & Squire’s (1991) experiments on “trace conditioning”, a form of classical conditioning involving a temporal separation between the conditioned stimulus (CS) and the unconditioned stimulus (US). Clark & Squire used the eye-blink reflex as the response and showed that when presented with an audible tone (CS) that terminated either a half or a full second before a puff of air (the US), not all normal human subjects learned to blink in response to the tone (prior to the puff), but all and only those who did reported during debriefing that they were *explicitly* aware of the temporal relation between the two stimuli. In contrast, all subjects can be conditioned to blink in response to the tone when the puff is presented after the onset of the tone but before its termination—“delay conditioning”—regardless of their knowledge of the temporal relationship between CS and US. Trace conditioning occurs in rabbits, and, like the humans, there are much greater individual differences in success rate for trace conditioning than for delay conditioning in these animals (Thompson et al. 1996). While Carruthers might assert that this requires only first-order representation of the temporal relation between two events, not between the experiences themselves, it seems likely that the human capacity to represent this

relationship depends on phenomenally conscious awareness of the stimuli; there's at least *prima facie* evidence that one cannot be *explicitly* aware of the temporal relation between two stimuli of which one has no phenomenal consciousness, and that one cannot learn the association without being explicitly aware of the relationship. At the very least, this provides an empirical lead into a possible function of consciousness with respect to a certain type of learning.

The varieties of animal learning are indeed more subtle than Carruthers lets on. As in his initial foray into the domain of animal consciousness, he has been betrayed by his reliance upon an intuitive understanding of animal learning and behavior that is not adequately informed by the scientific results, with the outcome that one simplistic hypothesis about the function of pain has been replaced with another. Even if higher-order thoughts are required to mediate the more sophisticated kinds of learning, Carruthers' case for mind-reading as the sole functional explanation for the initial natural selection of such thoughts is weak. This undermines his denial of phenomenal consciousness, not only to nonhuman animals, but to young children and autistic individuals as well, on the basis of their apparent lack of full mind-reading capabilities. The kinds of learning discussed above seem within the range of at least some nonhuman animals whether or not they can "read minds". If higher-order thoughts are required for evaluative modification of behavior, this too seems within the range of some non-human animals without it being required that they have the ability to attribute mental states to others.

I do think, however, that there is something right about Carruthers' engagement with the question of what functions are served by phenomenal consciousness. (See also Allen & Bekoff 1997, chapter 8.) In the absence of a consensus theory of consciousness, a functional approach provides a reasonable approach to investigating the distribution of animal consciousness in general, and conscious pain in particular, by investigating links between cognitive function and consciousness. But as we have seen, some of the "obvious" functions of pain can be carried out by detached spinal cords that are presumably devoid of conscious phenomenology, and thus we need to become more sophisticated about the functions of conscious pain than is often the case in the philosophical and scientific literature.

Carruthers' denial of phenomenal consciousness to animals runs strongly counter to most people's intuitions on this matter. "It really is something of a scandal," he writes, "that people's intuitions, in this domain, are given any weight at all" (Carruthers 2000, p. 199). Such intuitions, however, need not be free-floating for they can in fact be grounded in considerations of similarity and evolutionary continuity. The question to be pursued in the rest of this paper is whether those considerations can be further bolstered by thinking about the functions of conscious pain. But whether or not this project is successful, it should be understood that, at this stage of our

understanding of behavior and neuropsychology, to circumscribe phenomenal consciousness on the basis of one person's inability to imagine alternative accounts for its evolution would be far more scandalous than taking seriously the *prima facie* evidence that animals feel pain consciously.

4 Pain and its Functions

This final section examines claims about the biological (adaptive) functions of pain and argues that, in the light of what's known about the complexity of spinal mechanisms, some common suggestions are not defensible.

At first blush, the idea that conscious pain brings a selective advantage seems so obvious as to need no detailed justification. But while the capacity for conscious pain undoubtedly *seems* very important to survival and reproduction, without independent evidence of selection for the sensation over and above what can be delivered by the nociceptive system in the absence of consciousness, we should not fool ourselves into thinking that a selectional explanation for conscious pain has been provided. Cases of congenital insensitivity to pain, which are often trotted out by philosophers and scientists to support the claim that the conscious experience of pain is important, fail in this regard precisely because they are cases where several elements of the nociceptive system are also nonfunctional and so the finger cannot be determinately pointed at the absence of conscious pain experiences.³⁴

One way to approach the question of what functions are served by the conscious parts of the nociceptive system is to investigate the capacities of the vertebrate spinal cord independent of its connections to the brain.³⁵ The work by Grau and colleagues introduced above indicates that nociception-related learning in the spinal cord shows many advanced features such as latent inhibition (where repeated presentation of a cue diminishes subsequent associative conditioning to that cue) and overshadowing (where learning about a less salient cue is blocked by presentation of a more salient one). In intact animals, both these phenomena have been explained in cognitive terms as due to attentional processes (indeed they are widely believed to *require* cognitive explanation). But the fact that both occur in detached spinal cords indicates that this form of attention is not mediated by conscious experience³⁶ and that we should therefore be careful about assuming a role for conscious cognitive systems when similar phenomena are observed in intact organisms (see Grau 2002).

Another area in which statements about the function of pain are often naive is in failing to distinguish different behavioral responses, e.g., withdrawal from the noxious stimulus, and vocalizations. Antinociception is a phenomenon whereby exposure to a noxious stimulus reduces subsequent reactivity to the same or other noxious stimuli. By measuring the amount of time it takes for a rat to flick its tail, it can be shown that prior exposure to a

mildly painful stimulus (shock) produces antinociception for up to 10 minutes (Grau 2002). But it would be a mistake to infer from this that the animal consciously experiences less pain, for other measures of pain reactivity, such as time to vocalization and motor reactivity, show *increased* sensitivity in the same time frame (King et al. 1996). These results were not simply due to sensitization on the motor side for the treatment also enhances learning. Interestingly, however, it was also found that with higher intensity shocks, the spinal antinociception was accompanied by reductions in vocalization and fear conditioning, indicating possible hypoalgesia (Meagher et al. 2001).

What can we make of these complicated results? I'll start with a note of caution about Hardcastle's noteworthy attempt to give an account of the functions of pain that goes beyond the usual platitudes about avoiding danger. She makes much of the fact that the pain system consists not only of ascending pathways from nociceptors to the brain (the pain sensory system), but also of descending pathways from the brain to the dorsal horn of the spinal column (the pain inhibitory system). Hardcastle claims that this dual system serves two different (evolutionary) goals: the sensory system provides information about injuries and the potential for injury, while the inhibitory system "shuts down the [pain sensory system] when flight or fleeing is immanent, and then enhances the [pain sensory system] response in moments of control." (Hardcastle 1997, p. 408) Conscious sensation of pain is the joint product of these two sometimes antagonistic, sometimes facilitatory subsystems, on this view. There is something right about this view, but as we have seen, the functional story is not simple. Some behavioral responses are inhibited while others are simultaneously enhanced, thus we cannot just assume that because there is inhibition by some measures, there is also a reduction in conscious pain, or that the function of conscious pain is simply as a warning signal to be suppressed whenever the going gets tough.

Discoveries about the distributed nature of pain processing argue for a more nuanced view about the functions of pain. The conscious experience of pain is most likely not simply an "alarm bell" to be suppressed when it becomes essential to act, but instead has a complicated role in the capacity of organisms to learn how to behave when confronted with actual or possible tissue damage.³⁷ The variety of different forms of animal learning has been insufficiently appreciated by philosophers. Attention to the details of learning would serve to allow more fine-grained distinctions among organisms of different species. At one level, these details provide a richer base for the analogical argument—a more complete version of Varner's table, for example. This is useful, and if applied systematically to a wide range of species it would allow us to avoid inappropriate generalizations about entirely too broad taxonomic categories. But, at another level, the approach also has the potential to establish a more theoretical basis for

relating the observed capacities and the attribution of consciousness to animals. It appears that certain kinds of associative learning are strongly correlated with phenomenal consciousness in humans, thus strengthening the basic argument by analogy when similar forms of learning are described in nonhuman animals. But they may also be theoretically linked to phenomenal consciousness if the best explanation of such learning involves the organism's ability to notice relationships between its own experiences.

It may seem that the foregoing suggestion constitutes an endorsement of a higher-order account of consciousness. In fact, however, I think we should remain neutral on the ontological question of what constitutes consciousness—the answer to that question cannot be decided without a lot more empirical work. It is nevertheless possible to maintain that evidence for higher-order capacities provides a strong form of evidence for phenomenal consciousness even if those capacities aren't part of any ontological account of phenomenal consciousness, in much the same way that a person's description of their deeds can provide strong evidence of those deeds even though the capacity to describe them need not be a necessary condition for the deeds themselves.

While nociception-based learning and centralized modification of peripheral nociception are both found in the marine snail, *Aplysia*, its range of learning abilities is undoubtedly less sophisticated than what is known for at least some mammals and other vertebrates. To my knowledge, work on nociception-related learning in invertebrates with more sophisticated nervous systems, such as cephalopods, simply hasn't been done; in fact, it has only been done for a very limited selection of vertebrates, selected mostly for the convenience of the experimenters. There is a need for serious comparative work in this area, but there are, of course, questions about the ethical propriety of doing more of this kind of work, precisely because it might cause morally objectionable pain.³⁸ Without it, however, it is dishonest to pretend that we "know where to draw the line," on animal pain, the existing arguments for and against the existence of conscious animal pain remain essentially weak, and legal and moral principles based on such arguments will remain controversial. Although the scientific developments may not resolve all our legal and ethical quandaries, they can surely help us to construct more appropriate regulations and policies than at present.

Finally, although I have criticized some recent attempts by philosophers to bring an evolutionary, functional perspective to bear on questions about pain for being too simplistic, I nonetheless endorse the general approach, for I agree with Hardcastle that it is only by understanding the functions of pain that we can dispel confusion about this most basic of experiences, and integrate behavioral and neurological findings.³⁹ I would add that questions about the distribution of conscious pain in nonhuman animals are central to the evolutionary approach and deserve focused attention from both scientists and philosophers of mind. While the unfolding scientific story about

the functions of pain across the animal kingdom is more complicated than we have yet imagined, there is nothing to indicate that it is beyond our comprehension.

Notes

¹ While many philosophers believe that pain is *essentially* conscious, not all do. I therefore use the phrase “conscious pain” for the sake of being explicit. I wish to remain neutral on the question of whether there are such things as unconscious pains, distinct from unconscious nociception.

² Suit filed July 1999 by Alternatives Research & Development Foundation et alia vs. USDA.

³ There may be pragmatic justification for the USDA given that rats, mice, and birds comprise something close to 95% of all laboratory animals, thus greatly increasing the regulatory burden if they are to be included. Researchers also argue that common laboratory animals are adequately protected by other regulations.

⁴ USDA policy #11, on the web at <http://www.aphis.usda.gov/ac/policy/policy11.pdf>.

⁵ It may well be sensible legal practice to keep epistemological questions from the purview of juries and judges as much as possible.

⁶ See Hughes & Meyer (2000) who write that anti-cruelty legislation should allow “sufficient flexibility on the part of the judiciary to determine on a case by case basis whether distress or suffering can occur, based on the most current scientific and veterinary advice available” (p. 50).

⁷ Orlans (1996, 1997) discusses the use of a “pain scale” or “invasiveness scale” which can be applied with a reasonable amount of intersubjective agreement and which can be used to restrict the use of procedures according to educational and research objectives.

⁸ Some recent writings that at least mention the topic include Nelkin 1986; Carruthers 1989, 2000; Tye 2000.

⁹ E.g., Hardcastle (1997, 391) who writes “The frontal lobe (and its connections) process our actual suffering.” (see also Carruthers 2000)

¹⁰ Much of this is in the ethics literature, e.g. Regan, Singer, Rollin (the exception provided by Varner’s attempt to build a more empirically detailed case is discussed further below); but see also Searle 1998.

¹¹ But see Darwin 1965, p. 70, for a description of the behavior of a female hippopotamus giving birth at the London Zoological Gardens.

¹² A similar point is made by Bateson 1992.

¹³ This question is raised in an ethico-legal context by Hughes & Meyer 2000, p. 49.

¹⁴ The exact connection between conscious pains and their moral significance is under debate by philosophers. Carruthers (1989, 1992), for example, has argued that while animals experience pain, they do so unconsciously, and that their pains are therefore morally insignificant. The Cartesian roots of this view can be found in Descartes’ *Reply to Objections VI*. Carruthers originally held that the presence or absence of conscious mental states was the *only* morally significant factor (see his 1989, 1992), a view that is also implicit in much of the animal ethics literature; see Varner (2000). Carruthers now attributes moral significance to the frustration of desires, whether conscious or unconscious (see, e.g., his 2000). Tye (2000) argues that despite involving phenomenal consciousness, the pains experienced by “simple” creatures do not constitute suffering because there is no cognitive awareness of pain. Nelkin (1986) argues that pain is a cognitive attitude not a sensation, and that what matters morally is the attitude, regardless of the presence or nature of the accompanying sensations (1986, p. 147). Further discussion of the exact relevance of pain attributions to animals for the purposes of ethics and legislation must be deferred due to limitations of space, but regardless of one’s view of this

relationship, the epistemological questions about conscious pains in animals will remain central to the actual legal and ethical debate.

¹⁵ See Singer, Regan, etc.

¹⁶ But see Sober (2000a, ch. 2) for the view that such arguments are better viewed as likelihood arguments.

¹⁷ A particularly clear example from Rollin (1988, p. 149) is this: "Much of the behavioral evidence which licenses us to attribute experienced pain to other humans is present in animals. Animals cry out when injured, are tender at the point of injury, cringe before blows, avoid electrical shock and heat, and so on." But see also Darwin (1965, p. 69 ff).

¹⁸ E.g., Singer 1990, pp. 11ff; especially pp. 12–13 where he quotes Serjeant 1969.

¹⁹ For more on this topic in a general context of inferring animal minds, see Sober (2000b).

²⁰ Allen (1998); but see Perrett (1997) for an argument that the plausibility of theory-based arguments are parasitic upon a form of argument by analogy.

²¹ Thus, e.g., Povinelli & Giambone (2000) have argued that most arguments from behavioral similarity are especially weak because outwardly similar behaviors between humans and our closest relatives, chimpanzees, are controlled by very different cognitive mechanisms. They believe this conclusion to be established by Povinelli's research into how chimpanzees use cues to track visual attention without comprehension of the cognitive aspects of seeing. Povinelli's conclusions are, however, controversial, as I discuss further below.

²² Varner 1998, Table 2.3, p. 53. The table updates one that originally appeared in Smith & Boyd 1991. The other sources for Varner's survey are DeGrazia & Rowan 1991; Bateson, 1991; and Rose & Adams 1989. In his table, Varner places a plus sign in cells where he judged that the available evidence gave firm support for the condition in the taxonomic group, a minus sign where he judged the evidence to be firmly against the condition, and a question mark where the evidence appeared indecisive.

²³ Those excrescences comprise monism, materialism, behaviorism, token-token identity, functionalism, and Strong A.I. (Searle 1998, p. 39).

²⁴ See Searle (1998, p. 47); since Searle offers nothing more than that this should be an electrochemical condition that is necessary and sufficient for human consciousness, the argument here might seem to boil down to one based on neurological similarity, with all the weaknesses that have been noted of that argument form. No doubt Searle would respond that allegations of weakness are just another symptom of a cryptodualistic mindset.

²⁵ For a history of the science of pain, see Rey 1995.

²⁶ Carruthers (2000) denies the significance of this finding on the grounds that "there may be other differences between the species sufficient to make a difference in phenomenal consciousness" (2000, p. 197). This weak response is just more evidence of the argumentative tension between similarities and dissimilarities relating humans and other animals.

²⁷ The human vomeronasal organ consists of sensory pits in the nasal septum, distinct from the olfactory system. Widely studied in nonhuman animals but originally thought to be vestigial in humans, there is increasing evidence that it is functional in humans. For a review see Monti-Bloch et al. (1998).

²⁸ I owe this term to Gordon Burghardt; see Cooper & Burghardt 1990.

²⁹ Taste is an interoceptive cue that is not easily associated with external conditions, but readily associated with food poisoning. Odor alone is a good exteroceptive cue that can easily be associated in learning with other external conditions, but when presented in conjunction with taste it becomes an interoceptive cue that is not easily associated with external conditions. See Palmerino et al. 1980; von Kluge & Brush 1992; Batsell et al. 2001.

³⁰ See his 2000 contribution to the *Sito Web Italiano per la Filosofia* at <http://www.swif.uniba.it/lei/mind/forums/carruthers.htm>.

³¹ Could the spinal cord be phenomenally conscious? In the absence of any other reason to suspect that it is, I will assume that these forms of associative learning don't give us a strong reason for thinking so.

³² See, e.g., Keefe et al. 1999. They write: “The original Cartesian view of a direct relationship between nociception and the experience of pain has been found . . . unable satisfactorily to explain variability in pain response. . . . [T]he relationship between pain, physical impairment, and the level of disability is remarkably variable. . . . In chronic back pain in particular there is frequently little relationship between demonstrable physical impairment, and accompanying degree of functional incapacity or psychological distress.” (Keefe et al. 1999, p. 219)

³³ See Hardcastle 1999, for an overview. For a detailed review of neuronal responses to injury see Walters 1994.

³⁴ Cf. Hardcastle, for example, who writes: “a pain sensory system tied to the somatosensory processors makes good evolutionary sense. As creatures eking out lives in a hostile environment, having a system which could warn us when damage occurred and which could force us to protect damaged parts until they healed would be tremendously beneficial. (Indeed, persons who cannot feel any pain at all often live a nasty, brutish, and short life.)” (Hardcastle 1997, p. 395) I presume that “feel pain” here refers to the capacity for conscious sensations of pain.

³⁵ This is a rapidly evolving area of understanding and any firm conclusions I might make here run the risk, mentioned twice already, of becoming rapidly obsolete. But the argument I wish to give is in principle one: given empirical research on spinal cord function it will be possible to develop a more sophisticated view of the functions of conscious pain experiences.

³⁶ Again, I am assuming that there’s no locus of consciousness in the spinal cord, and that there are no “split-CNS” puzzles analogous to “split-brain” puzzles about multiple consciousnesses. To discuss this possibility would take us too far afield here.

³⁷ For a similar thesis about conscious experience in general, see Cotterill 2001; Cotterill also provides a useful reminder of the importance of understanding neural functions in relation to motor systems. Tye’s (2000) connection of phenomenal consciousness of pain to modification of beliefs and desires, and to subsequent changes in behavior, also links conscious pain to learning, albeit rather generally.

³⁸ It is worth noting here that the Humane Society of the United States distinguishes between research causing acute and chronic pain to animals, finding the former to be acceptable when conducted within International Association for the Study of Pain guidelines, but calling for a complete termination of the latter by the year 2020.

³⁹ See also Walters (1994, p. 94) who writes, “a consideration of the functional and evolutionary context of injury-related plasticity can begin to tie together diverse findings and foster broad, multidisciplinary approaches to physiological processes, such as pain and memory, that are of considerable interest to society.” One might add that there must be a genetic component to this story, but we are far enough from understanding the genetic basis of behavior that this must wait.

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References

- Allen, C. (1998). “The Discovery of Animal Consciousness: An Optimistic Assessment,” *Journal of Agricultural and Environmental Ethics*, 10: 225–246.
- Allen, C. & Bekoff, M. (1997). *Species of Mind*. Cambridge, MA: MIT Press.

- Bateson, P. (1991). "Assessment of Pain in Animals," *Animal Behaviour*, 42: 827–839.
- Bateson, P. (1992). "Do Animals Feel Pain?" *New Scientist*, 25 April, 1992: 30–33.
- Batsell, W. R., Jr., Paschall, G. Y., Gleason, D. I. & Batson, J. D. (2001). "Taste Preconditioning Augments Odor-Aversion Learning," *Journal of Experimental Psychology: Animal Behavior Processes*, 27: 30–47.
- Carruthers, P. (1989). "Brute Experience," *The Journal of Philosophy*, LXXXVI: 258–269.
- Carruthers, P. (1992). *The Animals Issue*. Cambridge: Cambridge University Press.
- Carruthers, P. (2000). *Phenomenal Consciousness*. Cambridge: Cambridge University Press.
- Clark, R. E. & Squire, L. R. (1998). "Classical Conditioning and Brain Systems: The Role of Awareness," *Science*, 280: 77–81.
- Coghill, R. C. (1999). "Brain Mechanisms Supporting the Pain Experience," in M. Max, ed., *Pain 1999—An Updated Review*. Seattle: IASP Press, pp. 67–76.
- Cooper, W. E., Jr. & Burghardt, G. M. (1990). "Vomerolfaction and Vomodor," *Journal of Chemical Ecology*, 16: 103–105.
- Cotterill, R. M. J. (2001). "Evolution, Cognition and Consciousness," *Journal of Consciousness Studies*, 8: 3–17.
- Darwin, C. (1965). *The Expression of the Emotions in Man and Animals*. Chicago: University of Chicago Press.
- DeGrazia, D. & Rowan, A. (1991). "Pain, Suffering and Anxiety in Animals and Humans," *Theoretical Medicine*, 12: 193–211.
- Downes, H. Koop, D. R. Klopfenstein, B. & Lessov, N. (1999). "Retention of Nociceptor Responses During Deep Barbiturate Anesthesia in Frogs," *Comparative Biochemistry and Physiology Part C: Pharmacology, Toxicology and Endocrinology*, 124: 203–210.
- Gallup, G. G. Jr. (1970). "Chimpanzees: Self-recognition," *Science*, 167: 86–87.
- Gallup, G. G. Jr. (1998). "Can Animals Empathize," *Scientific American*, 9: 66–71.
- Gallup, G. G. Jr., Anderson, J. R. & Shillito, D. J. (2002). "The Mirror Test," in M. Bekoff, C. Allen, & G. M. Burghardt, eds., *The Cognitive Animal*. Cambridge, MA: MIT Press, pp. 325–334.
- Grau, J. (2002). "Learning and Memory without a Brain," in M. Bekoff, C. Allen, & G. M. Burghardt, eds., *The Cognitive Animal*. Cambridge, MA: MIT Press, pp. 77–88.
- Hardcastle, V. G. (1997). "When a Pain is Not," *The Journal of Philosophy*, XCIV: 381–409.
- Hardcastle, V. G. (1999). *The Myth of Pain*. Cambridge, MA: MIT Press.
- Hare, B., Call, J., Agnetta, B. & Tomasello, M. (2000). "Chimpanzees Know What Conspecifics Do and Do Not See," *Animal Behaviour*, 59: 771–785.
- Hare, B. & Wrangham, R. (2002). "The Evolution of Social Cognition: An Integrated Approach," in M. Bekoff, C. Allen, & G. M. Burghardt, eds., *The Cognitive Animal*. Cambridge, MA: MIT Press, pp. 363–370.
- Harrison, P. (1991). "Do Animals Feel Pain?" *Philosophy*, 66, 255: 25–40.
- Hughes E. L., & Meyer, C. (2000). "Animal Welfare Law in Canada and Europe," *Animal Law*, 6: 23–76.
- Jamieson, D. (1998). "Science, Knowledge, and Animals Minds," *Proceedings of the Aristotelian Society*, 98: 79–102.
- Jamieson, D. & Bekoff, M. (1991). "Carruthers on Nonconscious Experience." *Analysis*, 52: 23–28.
- Keefe, F. J., Bradley, L. A. & Main, C. J. (1999). "Psychological Assessment of Pain for the General Clinician," in M. Max, ed., *Pain 1999—An Updated Review*. Seattle: IASP Press, pp. 219–232.
- King, T. E., Joyner, R. L., Meagher, M. W. & Grau, J. W. (1996). "The Impact of Shock on Pain Reactivity II: Evidence for Enhanced Pain." *Journal of Experimental Psychology: Animal Behavior Processes*, 22: 265–278.
- McGurk, H. & McDonald, J. (1976). "Hearing Lips and Seeing Voices." *Nature*, 264: 746–748.

- Meagher, M. W., Ferguson, A. R., McLemore, S., King, T. E., Sieve, A. N., Crown, E. D. & Grau, J. W. (2001). "Stress-Induced Hyperalgesia: Generality," *Journal of Experimental Psychology: Animal Behavior Processes*, 27: 219–238.
- Monti-Bloch, L., Jennings-White, C. & Berliner, D. L. (1998). "The Human Vomeronasal Organ: A Review," *Annals of the New York Academy of Sciences*, 855: 373–389.
- Nagel, T. (1974). "What is it Like to be a Bat?" *Philosophical Review*, 83: 435–450.
- Nelkin, N. (1986). "Pains and Pain Sensations," *The Journal of Philosophy*, LXXXIII: 129–147.
- Orlans, B. (1996). *In the Name of Science: Issues in Responsible Animal Experimentation*. New York: Oxford University Press.
- Orlans, B. (1997). "Pain: Invasiveness Scales for Judging Animal Experiments" in Bekoff & C. Meaney, eds., *Encyclopedia of Animal Welfare*. Westport, CT: Greenwood, pp. 267–269.
- Palmerino, C. C., Rusiniak, K. W. & Garcia, J. (1980). "Flavor-Illness Aversions: The Peculiar Roles of Odor and Taste in Memory for Poison," *Science*, 208: 753–755.
- Perrett, R. W. (1997). *Journal of Applied Philosophy*, 14 (1997): 49–58.
- Povinelli, D. J. (1996). "Chimpanzee Theory of Mind?" in P. Carruthers & P. Smith, eds., *Theories of Theories of Mind*. Cambridge: Cambridge University Press.
- Povinelli, D. J. & Giambone, S. (2000). "Inferring Other Minds: Failure of the Argument by Analogy," *Philosophical Topics*, 27: 161–201.
- Regan, T. (1983). *The Case for Animal Rights*. Berkeley: University of California Press.
- Rey, R. (1989). *The History of Pain*. Cambridge, MA: Harvard University Press.
- Rollin, B. (1988). *The Unheeded Cry: Animal Consciousness, Animal Pain and Science*. New York: Oxford University Press.
- Rose, M. & Adams, D. (1989). "Evidence for Pain and Suffering in Other Animals," in G. Langley, ed., *Animal Experimentation: The Consensus Changes*. New York: Chapman and Hall, pp. 42–71.
- Searle, J. (1998). "Animal Minds," *Etica & Animali*, 9: 37–50. This is an enhanced version of the essay originally published as "Animal Minds" in *Midwest Studies in Philosophy*, XIX (1994).
- Serjeant, R. (1969). *The Spectrum of Pain*. London: Hart Davis.
- Shumaker, R. & Swartz, K. (2002). "When Traditional Methodologies Fail: Cognitive Studies of Great Apes," in M. Bekoff, C. Allen, & G. M. Burghardt, eds., *The Cognitive Animal*. Cambridge, MA: MIT Press, pp. 335–344.
- Singer, P. (1990). *Animal Liberation*, Revised edition. New York: Avon Books.
- Smith, J. & Boyd, K. (eds.) (1991). *Lives in the Balance: The Ethics of Using Animals in Biomedical Research*. New York: Oxford University Press.
- Sober, E. (2000a). *Philosophy of Biology, Second Edition*. Boulder, CO: Westview Press.
- Sober, E. (2000b). "Evolution, and the Problem of Other Minds," *The Journal of Philosophy*, XCVII: 365–386.
- Stoerig, P. & Cowey, A. (1997). "Blindsight in Man and Monkey," *Brain*, 120: 535–559.
- Thompson, L. T., Moyer, J. R. & Disterhoft, J. F. (1996). "Trace Eyeblink Conditioning in Rabbits Demonstrates Heterogeneity of Learning Ability Both Between and Within Age Groups," *Neurobiology of Aging*, 17: 619–629.
- Tye, M. (2000). *Consciousness, Color, and Content*. Cambridge, MA: MIT Press.
- Varner, G. (1998). *In Nature's Interests? Interests, Animal Rights, and Environmental Ethics*. New York: Oxford University Press.
- Varner, G. (2000). "Sentientism," in D. Jamieson, ed., *A Companion to Environmental Philosophy*. Oxford: Blackwell, pp. 192–203.
- von Kluge, S. & Brush, R. (1992). "Conditioned Taste and Taste-Potentiated Odor Aversions in the Syracuse High- and Low-Avoidance Strains of Rats." *Journal of Comparative Psychology*, 106: 248–253.

- Walters, E. T. (1994). "Injury-Related Behavior and Neuronal Plasticity: An Evolutionary Perspective on Sensitization, Hyperalgesia, and Analgesia," *International Review of Neurobiology*, 36: 325–427.
- Walters, E. T. (1996). "Comparative and Evolutionary Aspects of Nociceptor Function," in C. Belmonte and F. Cervero, eds., *Neurobiology of Nociceptors*. New York: Oxford University Press, pp. 92–114.