

<sup>2</sup> Harrison, "Theodicy and Animal Pain," p. 85.

<sup>3</sup> Harrison, "Do Animals Feel Pain?," p. 29.

<sup>4</sup> *Ibid.*, P 30

<sup>5</sup> Marian Dawkins writes, for example: "To say that birds do not suffer because their brains are different from those of mammals is like stating that a jet airplane cannot fly because it has no propellers. Different anatomical structures can serve the same function..." (from "Author's Response," *Brain and Behavioral Science* 13:49-54, p. 50).

<sup>6</sup> Harrison, "Do Animals Feel Pain?," p. 35.

<sup>7</sup> Harrison, "Do Animals Feel Pain?," pp. 36, 38; "Theodicy and Animal Pain," p. 84.

<sup>8</sup> Harrison, "Do Animals Feel Pain?," p. 34.

<sup>9</sup> Harrison, "Theodicy and Animal Pain," p. 86.

<sup>10</sup> Harrison, "Do Animals Feel Pain?," p. 36.

<sup>11</sup> Donald Griffin's works, *The Question of Animal Awareness* (New York: Rockefeller Univ. Press, 1981) and *Animal Thinking* (Cambridge: Harvard Univ. Press, 1984), are directed largely to attacking the anti-mentalist bias that he saw among ethologists and psychologists.

<sup>12</sup> Harrison, "Theodicy and Animal Pain," pp. 88-91.

<sup>13</sup> See, for example, Stephen J. Gould and Richard C. Lewontin, "The Spandrels of San Marcos and the Panglossian Paradigm: A Critique of the Adaptationist Programme," *Proceedings of the Royal Society of London* B205:581-598, 1979.

<sup>14</sup> Griffin, *The Question of Animal Awareness*, p. 145. See also *Animal Thinking*, p. 40.

<sup>15</sup> The principle is not true as stated because there are many features of organisms that have little or no apparent adaptive value. (I consider the issue of what proportion of features are nonadaptive to be an open one.) However, if a feature has a "cost" or disadvantage to it, or if it is likely to have had a fairly complex evolutionary development, it is reasonable to expect some adaptive function to be present to offset whatever disadvantages it may have now or may have had during its development. However, one could still be mistaken: In some cases, disadvantageous features may simply be genetically or developmentally linked to other features that are advantageous.

<sup>16</sup> See, for example, Marian Dawkins' argument in "Minding and Mattering," in *Mindwaves*, eds. C. Blakemore and S. Greenfield (Oxford: Blackwell, 1987), p. 159.

<sup>17</sup> Peter Singer, *Animal Liberation* (New York: Avon, 1990) p. 174.

---

---

## Response: Parsimony Made Simple: Rosenfeld on Harrison and Animal Pain

---

---

David Boonin-Vail  
Georgetown University

Peter Harrison stands accused of misusing the notion of parsimony.<sup>1</sup> He argues<sup>2</sup> that we ought not to attribute pain states to animals on the grounds that doing so would be unparsimonious; the most efficient adaptive mechanisms possible for creatures lacking the ability to make free, reason-based choices<sup>3</sup> would not require such states, and on "the simplest application of the theory of natural selection,"<sup>4</sup> we should not attribute to animals any features which are not required to explain their adaptive behavior. Rosenfeld objects that this conclusion implicitly presupposes the excessively strong claim that organisms always develop the most effective adaptive mechanisms possible, rather than the more realistic claim that they will tend to develop the most effective mechanisms available given the genetic material they have to work with. On this more reasonable standard, Rosenfeld argues, it remains an open possibility that the ability to feel pain (and, I would add, pleasure) was the best mechanism available at some important early stage in evolution, and that evolutionary theory might thus provide support for the attribution of pain to animals after all.

I want here to initiate the project of assessing the case for the prosecution by doing three things: first, I want to try to clarify the nature of the charge itself, by situating Rosenfeld's attack on Harrison's appeal to parsimony in this case within the context of a more fundamental attack on appeals to parsimony in general.



## DISCUSSION

Second, I want to identify and note one feature of an important response to Rosenfeld's position which might be made on behalf of Harrison. Third, I want to show how Rosenfeld's analysis, understood within the context of the more general critique of parsimony appeals, can overcome this response.

### I.

Let me begin with an example of an appeal to considerations of parsimony taken from another dispute within evolutionary biology.<sup>5</sup> When a group of musk oxen is attacked by wolves, the oxen form a circle with adult males on the outside facing the attack and females and young protected on the inside. Two hypotheses might be offered to account for the development of this trait. One is that it is the result of group selection: groups of oxen compete with each other at avoiding predators, and those which form protective circles are more successful at surviving and producing new groups than are those which do not. The trait thus survives because it benefits the group (at the expense of some individuals). A second hypothesis is that the trait is the result of individual selection: individual oxen compete with each other at avoiding predators. Some individuals exhibit the selective trait of standing to fight against relatively smaller predators while fleeing or hiding from relatively larger ones, and those who possess this trait are more successful at surviving and reproducing than are those who do not. Wolves are typically smaller than adult male oxen but larger than female and younger oxen, so when wolves attack, adult male oxen stand and fight and smaller oxen flee to and hide in the interior of the circle. The trait thus survives not because it benefits the group (at the expense of some individuals), but because it benefits each individual.

Both explanations are consistent with the facts. Which should be accepted? One could argue, as George C. Williams did in his important study, *Adaptation and Natural Selection*, that the individual selection account should be accepted on the grounds that lower-level selection hypotheses are more parsimonious than higher-level ones. On this view, we would insist that whenever a trait can be explained in terms of either individual or group selection, the individual selection account is always to be preferred. There is no need to invoke the more complex notion of group selection when the relatively simpler notion of individual selection will do.<sup>6</sup>

But why should the fact that the individual selection hypothesis is more *parsimonious* count as a reason for thinking it any more likely to be *true*? Elliott Sober has pressed this question in his recent work, and the results are illuminating: the plausibility of individual over group selection hypotheses can be justified, but only by appealing to specific, empirical claims about population structures.<sup>7</sup> The fact that one hypothesis is more parsimonious than the other, in and of itself, then, does nothing to make it more plausible. And Sober argues that careful attention to other case studies supports this general conclusion: there is no *a priori* justification for preferring parsimonious hypotheses in general; parsimony lends credibility to hypotheses only in particular research contexts, and only given certain auxiliary empirical claims.<sup>8</sup>

I take it, then, that Rosenfeld has accomplished two things with his critique of Harrison. One is to provide additional support for Sober's analysis of parsimony in general by showing that the apparent plausibility of Harrison's appeal to parsimony in this case depends crucially on certain empirical assumptions about the way in which evolution in fact works. The other is to undermine the plausibility of Harrison's appeal by revealing that the empirical assumptions which are required in this case are implausibly strong. When we say that Harrison is guilty of misusing the notion of parsimony, then, we should mean two things: that his appeal to parsimony proceeds as if such appeals have an *a priori*, subject-matter neutral justification, which they do not, and that the empirical claims which would be needed to support his appeal to parsimony in this case are dubious at best. It is this second claim that enables Rosenfeld to rebut Harrison's argument as it stands, but the first, I want to suggest, may be needed to overcome a response which might be offered on Harrison's behalf.

### II.

Let me turn, then, to that response. Rosenfeld has argued that we should expect evolution to produce in organisms not the most efficient adaptive mechanisms possible, but the most efficient mechanisms available given the material they initially have to work with. It is possible that the ability to feel pain was the most efficient adaptive mechanism available at some important point in early vertebrate evolution, and if that is so, then parsimony considerations urge us to

attribute pain states to animals, rather than to deny them such states.

But Harrison might try to respond to all of this as follows: pain states might be the most efficient adaptive mechanisms that were available to early animals, but pain states still require appeals to "higher psychological faculties," while explanations in terms of hard-wiring appeal to "lower psychological faculties," and the latter are less complex than the former. So hard-wiring was the most efficient *simple* mechanism available, while pain states were (perhaps) the most efficient *complex* mechanism available. But we should prefer the more parsimonious explanation to the less, and attributing to animals the most efficient simple mechanism is still more parsimonious than attributing to them the most efficient complex mechanism.<sup>9</sup>

Notice that this reply would allow Harrison to deny Rosenfeld's claim that the rejection of animal pain could be salvaged only by appealing to further empirical studies.<sup>10</sup> On this view, it wouldn't matter what neurophysiologists and others uncovered about the relative merits of conscious and nonconscious adaptive mechanisms. Hard-wired mechanisms would remain the most efficient available *lower-order* adaptation, and on this view would be more parsimoniously inferred than any higher-order adaptation no matter how efficient. This assessment would be guaranteed by parsimony in the same way that, on Williams' account of natural selection, an explanation of some particular trait in terms of individual selection would always be preferred to an explanation in terms of group selection, even if the hypothesized process of selection at the individual level were far more cumbersome than the hypothesized process of selection at the group level. In both cases, no amount of empirical evidence would be able to establish the superiority of the higher-level hypothesis (pain in explaining animals, group selection in explaining trait survival), because the lower-level one would be taken as preferable on essentially *a priori* grounds.

### III.

But if we take Rosenfeld's critique of Harrison's position as underwriting a more fundamental criticism of *a priori* appeals to parsimony in general, then Harrison cannot escape the force of Rosenfeld's attack on one parsimony claim simply by hiding behind another. If Harrison were to revise his position in the

way that I have suggested, he could indeed maintain that the hard-wiring hypothesis remains, at least in some sense, more parsimonious than the pain states hypothesis. But at this point, Sober's question would arise all over again: why should the act that one hypothesis is more parsimonious than another (in this revised sense) count as a reason for thinking it any more likely to be true? And if Sober is right (and I take Rosenfeld's argument to provide further support for Sober's position), then it is true that parsimoniousness confers plausibility on the hypothesis in this case only given certain additional empirical assumptions. But these, surely, will again have to be assumptions about how evolution in fact works (i.e., that it works in such a way that it is more likely to produce mechanisms exploiting "lower psychological faculties" than mechanisms exploiting higher ones, even when using the higher ones would be more efficient) and will thus require the sorts of further empirical studies which Harrison seems determined to avoid.

That Harrison's approach encourages us to pay insufficient attention to facts about animals is also suggested by an additional feature of his writings which I would like to note in conclusion. In both of the articles to which Rosenfeld has referred, Harrison presents in response to the claim that we may infer pain from pain behavior the claim that the wildebeest "remains silent" as it is torn apart by predators and that it "dies in silence."<sup>11</sup> Now, I have never actually seen a wildebeest being torn apart by predators. And I take it that Harrison hasn't either. But there are people who have. Biologist Hans Kruuk, for example, has observed a number of successful hunts of wildebeest by hyena (Kruuk is an authority on the hyena). This is how he describes the wildebeest's behavior after it has been captured: "generally speaking the [wildebeest] just stands *uttering loud moaning calls* and is torn apart by the hyenas. It appears to be in a state of shock."<sup>12</sup>

It is a well-known feature of *a priori* arguments about animals that they are insensitive to empirical details. We are urged to give more weight to philosophical considerations of parsimony than to scientific observations about animals. But I take it that the upshot of this discussion is that considerations of parsimony in themselves carry no weight without auxiliary assumptions which in turn demand empirical support. To say that Harrison has misused the notion of parsimony, then, is in the end to say that he has made parsimony simple. And it is not.

## Notes

<sup>1</sup> Robert P. Rosenfeld, "Parsimony, Evolution, and Animal Pain," p. 4.

<sup>2</sup> Peter Harrison, "Theodicy and Animal Pain," *Philosophy* 64 (1989), pp. 79-92; "Do Animals Feel Pain?," *Philosophy* 66 (1991), pp. 25-40.

<sup>3</sup> I follow Rosenfeld (pp. 4, 7) here both in granting this claim about animals for the sake of argument and in noting that it is surely open to dispute.

<sup>4</sup> Harrison, "Do Animals Feel Pain?," p. 33.

<sup>5</sup> The example which follows is taken from Elliot Sober, "Let's Razor Ockham's Razor," pp. 80-81, in D. Knowles, Ed., *Explanation and its Limits* (Cambridge: Cambridge University Press, 1990), pp. 73-93. Sober in turn is following the use made of the example in George C. Williams, *Adaptation and Natural Selection: A Critique of Some Current Evolutionary Thought* (Princeton: Princeton University Press, 1966), pp. 218-220.

<sup>6</sup> This is Sober's characterization of Williams' argument. Sober writes that "Williams rejected this hypothesis of group adaptation and that this was typical of his reasoning: 'No need for group selection here; the more parsimonious individual-selection story suffices to explain'; 'Rather than suspending judgment about which explanation is more plausible, Williams opts for the lower-level story, on the grounds that it is more parsimonious' ('Let's Razor Ockham's Razor,' pp. 80, 81). It is not entirely clear to me that this fairly represents Williams' actual discussion of the musk ox case in particular. Williams seems in this case to suggest only that "it could be that a purely statistical effect is at work" and does not explicitly come down on one side or the other (Williams, *Adaptation and Natural Selection*, p. 219). Nonetheless, it is true that Williams' work as a whole argues against group selection explanations. And, in any event, my concern here is to provide an example of a parsimony appeal which can be used both to illustrate Sober's thesis and to illuminate the nature of Harrison's position. In this sense, it does not matter whether Williams himself explicitly endorses the claim that parsimony compels us to prefer the individual-selection account of the musk oxen's behavior. The point is that the claim is one that could be made.

<sup>7</sup> Elliot Sober, "Let's Razor Ockham's Razor," pp. 81-83.

<sup>8</sup> The other case Sober considers in "Let's Razor Ockham's Razor" concerns the problem of inferring the phylogenetic relationship between species from facts about their similarities and differences (pp. 84-90). He concludes that "if parsimony is the right method to use in phylogenetic inference, this will be because of specific facts about the phylogenetic process. The method has no *a priori*, subject-matter neutral

justification" (p. 90). The problem of phylogenetic inference in particular and Sober's analysis of parsimony in general are both explored in more detail in Sober, *Reconstructing the Past: Parsimony, Evolution, and Inference* (Cambridge: MIT Press, 1988).

<sup>9</sup> I take it that Harrison might be inclined to respond in this way because he views Morgan's canon as "asserting that a complex biological system would not evolve if a simpler one could perform the same function" ("Do Animals Feel Pain?," p. 34 n. 19), and he could argue that hard-wiring could perform the same function as pain states, even if the hard-wiring available was not the most efficient possible.

<sup>10</sup> Rosenfeld, "Parsimony, Evolution, and Animal Pain," p. 10.

<sup>11</sup> Theodicy and Animal Pain," p. 84; "Do Animals Feel Pain?," p. 33. In both instances, Harrison cites only the brief three-paragraph entry on "Pain" in the *Oxford Companion to Animal Behavior* (Oxford: Oxford University Press, 1982), which provides no sources for this claim.

<sup>12</sup> Hans Kruuk, *The Spotted Hyena: A Study of Predation and Social Behavior* (Chicago: University of Chicago Press, 1972), p. 158, emphasis added.

