

Re: Population genetics question regarding sexual selection

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- *From:* Guy Hoelzer <hoelzer@xxxxxxx>
 - *Date:* Thu, 31 Aug 2006 18:41:33 -0400 (EDT)
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Jim,

I'm glad to see that you are interacting with John on talk.origins. He used to be a frequent contributor to sbe and he is a good evolutionary biologist.

My take on your debate is that you are both right. We can blame Darwin for artificially separating sexual selection from natural selection. I understand why he did this, and I think his strategy was successful in its realized heuristic value. However, it has become clear over the past 30 years or so that this is a false distinction. Sexual selection is just a form of natural selection that operates within a population without the source of selective pressure emanating from the external environment.

Models of the evolution of epigamic traits that honestly signal overall quality of some sort were originally derived based on the Darwinian tradition. Therefore, the logic of the models started with the assumption that epigamic traits are ornaments that evolved through a process that was different than, and independent of, natural selection. This meant that the fitness effects of epigamic traits were treated as additive to fitness effects of traits evolved in response to natural selection. These models then predicted that an epigamic trait will be favored if it yields a net increase in fitness (typically approximated as lifetime reproductive success) by increasing reproductive output sufficiently to offset the cost in viability.

Of course, these are just models that make simplifying assumptions, like additivity. Your point about non-linearity is right on target, but the validity of the models does not rest on the accuracy of their assumptions; rather it depends on how robust the predictions of the models are to relaxation of those assumptions. I think John is trying to explain the logic of the models, while you are pointing out an important point about reality.

Cheers,

Guy

Re: Population genetics question regarding sexual selection

in article ed5v0r\$63c\$1@xxxxxxxxxxxxxxxxxxxx, Perplexed in Peoria at jimmenegay@xxxxxxxxxxxx wrote on 8/30/06 11:19 PM:

A discussion in talk.origins cross-posted here to get some comments from population genetics experts. Joe?

"John Harshman" <jharshman.diespamdie@xxxxxxxxxxxx> wrote in message [news:HxOJg.4132\\$tU.2558@xxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxx](mailto:news:HxOJg.4132$tU.2558@xxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxx)

Perplexed in Peoria wrote:

"John Harshman" <jharshman.diespamdie@xxxxxxxxxxxx> wrote in message [news:5nJg.21829\\$gY6.19405@xxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxx](mailto:news:5nJg.21829$gY6.19405@xxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxx)

Perplexed in Peoria wrote:

[snip]

There are three ways a tail affects male fitness. First, it really is (by assumption) a handicap – it decreases survival chances over the period from birth to sexual maturity. Second, due to the non-linear feature that Harshman insists on, it is correlated (post-survival-selection) with other positive genetic traits. [Third way – sexiness to females – snipped]

Actually, a non-linear relationship is not necessary for this to be true. The statistical expectation for all surviving males is that they will have some mean "survivability". Since the long tail is costly in terms of survival, the expectation is that long-tailed birds will have compensating average increased levels of "quality".

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I think you are wrong here. Without non-linearity, there would be no compensating correlation. The surviving long-tailed birds would not be particularly high in quality – at least no more so than the surviving short-tail birds. Both are impacted equally by quality, if there is no non-linearity.

You have to remember that "quality" is specifically defined so as to ignore the cost of the long tail. So if the expected survivability of all surviving birds is S , the cost of long tails is C , and the quality is Q , then for short-tailed birds $S = Q$, while for long-tailed birds $S = Q - C$. But since we agreed that the expected S is the same for long- and short-tailed birds, ...

I agreed to no such thing.

... $Q(\text{short-tailed}) = Q(\text{long-tailed}) - C$. And thus the long-tailed birds have higher expected quality. What you have noticed is that expected S is the same for all birds, long-tailed and short-tailed.

I notice no such thing.

But Q is indeed higher for the long-tailed birds.

I'm really quite surprised that you are making such an elementary mistake. If selection on the tail handicap and selection on 'quality' are independent (which is another way of saying that no non-linearity exists) then selection will not create a correlation between these traits. Your argument to the contrary is just wrong, but I don't see any quick-and-easy argument to convince you of this. One or the other of us needs to do some rethinking. I will leave it to any lurkers to suggest which of us ought to do this rethinking.

I'm going to cross-post this to sci.bio.evolution to see if we can get Joe Felsenstein to comment.

Snip remainder, as we are mostly disagreeing on terminology or on how to partition up the causality.

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