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REBUTTAL TO REYNOLDS (2003)

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RECENTLY, JIM CROCK, Christine Snow and I (2001) reported on calcium, phosphorus, sodium, and potassium levels in the leg bones of White-tailed Ptarmigan (*Lagopus leucurus*) and employed dual-energy, X-ray absorptiometry (DXA) to measure bone mineral density (BMD) and bone mineral content (BMC) (Mitlak et al. 1994). In this issue, Reynolds (2003) offers a critique of that paper.

Generally speaking, I find the critique to be constructive and useful though largely misguided. I would concede Reynolds (2003) made a good point when he suggests that our paper would have been stronger had we presented bone calcium data as a function of a hen's reproductive condition. That criticism was also made by one of the paper's original reviewers. Were I a reproductive physiologist, I might have done the analysis suggested, but I am not, so the relevant question seems to be "Are the data valuable as presented?" At the time of review, the editors seemed to think it was. Their rationale may have been partly based on the fact that all females (including all yearling females) of this species are reproductively active and at least attempt to breed (Braun et al. 1993). In fact, all females collected for this study and subsequently labeled "prelaying" were collected in May and were on territories where they were paired with and actively defended by males. Necropsies of these prelaying hens revealed that all were in the process of egg formation. Reynolds' (2003) assertion that "...there is no guarantee that birds would have bred had they not been collected" is, therefore, overstated.

Our brief note (Larison et al. 2001) dealt with the issue of how female ptarmigan meet the extraordinarily high calcium demand of egg formation. Both in our abstract and in our

introduction we clearly set out two possible strategies for resource use. (1) "Some birds..." we pointed out, "...may accumulate calcium over the weeks or months prior to reproduction and then mobilize those reserved during egg-laying." (2) Others, we wrote, apparently "consume sufficient resources on a daily basis to meet the temporally high calcium demands" of egg-laying. We then tested those strategies against our data and concluded ptarmigan use the former strategy, storing calcium in their leg bones prior to the onset of the egg-laying period. We further suggested the "ability to store calcium in the skeleton may afford this species more flexibility in selecting suitable breeding habitat than would be possible otherwise."

Curiously, the bulk of Reynolds' (2003) critique speaks not to the questions addressed by our paper, but rather to a separate issue altogether, that of the calcium storage medium. Reynolds (2003) contrasts two competing theories of calcium storage. One of those theories is that birds store calcium as medullary bone and the other is that calcium is stored as cortical bone. He argues that we were wrong when we concluded that the calcium we observed was stored as cortical bone. That is an odd assertion because our original paper does not so much as mention cortical bone. At no time in our paper did we argue how or where (in the bone) calcium was being stored. In fact, we judiciously avoided the subject of the storage medium precisely because our data are silent on that issue.

In crafting his argument, Reynolds (2003) frequently takes our words out of context clearly altering their original intent and meaning. Take for example the numerous times that Reynolds misquotes us as having said calcium storage occurred "months prior to reproduction." If you look back at the third paragraph of our paper one would see we actually said test subjects stored calcium "...in the months prior

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to reproduction." The words "in the" might seem like a small omission but an important one for Reynolds' argument. "Months prior to..." suggests two or more months prior to an event, whereas "in the months prior to..." suggests merely that calcium was stored prior to egg-laying. Our original paper was purposely vague on the subject of timing because the data provide no clarity on that subject. The calcium we observed may have been stored days, weeks, months, or just hours before our measurements were taken. Reynolds' omission seems suspiciously convenient because it provides the foundation for his entire critique. To support his argument, Reynolds (2003) then defines for us "short-term" which he says means "weeks" and "long-term" which he says means "months", but you should remember those are his definitions, not ours. We simply argue that storage occurs (which we call "long-term" storage) and we contrast it with same-day consumption and use. We never "infer..." as Reynolds asserts, that the storage occurred "...months before egg-laying." What we do say (twice) about timing is simply "...storage begins at least several weeks prior to (the egg-laying period)," an assertion that is supported by the data.

Reynolds' (2003) point that birds store calcium as medullary bone is not disputed, certainly not by our paper. I agree medullary bone is the most likely storage medium in many birds—in fact I think we said so in our introduction—and

I would go one step further and say that nothing in our ptarmigan data suggests otherwise. That is precisely why we selected leg bones for this study. But our data do not provide clarity on the storage medium debate and that is why we avoided mentioning it. I would suggest, therefore, that Reynolds' (2003) argument might better be presented as an independent paper on the subject of storage mediums with its own data to back it up rather than as a critique of a paper on a wholly different subject.

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