

## COMMENTARY

### R. W. Schreiber's Review of *The Sulidae: gannets and boobies*

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Authors are thin-skinned about criticism, and it is usually best to take what comes with as much grace as one can muster. However, sometimes a reviewer so grossly mis-states his case that a reply does service not only to the author but more importantly to those who read the review (and hopefully the correspondence) but not the book. In this reply to my friend Ralph Schreiber's review (pp. 634–637, this issue), I will confine myself largely to facts; on clarity and style other reviews offer an alternative to Schreiber's.

Table 11 is said to illustrate my disregard for sample size, ranges, and variability. But as its footnote says, there is very little published material for one of the three species concerned. In fact, throughout the book, I have presented data as fully as possible but without statistical analysis. Often the raw data were not available, and a simpler presentation, including the range, mean, and sample size, seemed best. Table 94, criticized by Schreiber, is a good example of this. It gives means, ranges, and sample size for morphological measurements of Red-footed Boobies from nine areas, for most of which I had no raw data, and I do not believe that additional statistical exercises would have been useful. In Table 12 (p. 100) Schreiber is undoubtedly correct to reject a difference of 100 g in a 3,000-g species as indicating adult weight loss during feeding of young. But that was precisely the point I was making; the text reference (p. 99) reads "the effort of feeding the chick does *NOT* cause the adults to lose weight (Table 12)." Concerning the low reproductive success of Bonaventure gannets, I *do* in fact consider disturbance by man (p. 106, l. 31), and I use the word "probably" in front of Schreiber's quote. The reference is surely obvious from the many times Poulin is quoted previously in the section concerned. I state that the Bonaventure ecological data are from Poulin's study, so it is totally misleading to accuse me of omitting the reference. *Contra* Schreiber, the two growth curves on p. 96 *are* easily compared. One shows details for the first 7 weeks, and the other covers 11 weeks. Yet for any age covered by both, comparison is easy. For example, a simple reading from each figure shows that at 31 days, Bass birds averaged c. 2,250 g (1,800–2,500 g), whilst those from Bonaventure weighed 2,100 g (1,800–2,200 g). If Schreiber had looked in the sensible place for substantiation of the claim that Bempton relied on immigration of Bass birds, he would have found under Bempton (p. 50) a cross-reference to the relevant details on p. 135. Must one repeat everything *ad nauseam*? As it was, the cross-references were a nightmare. More importantly, it would be a pity if the phenomenon of immigration to growing colonies were to be considered unproven. Despite the considerable potential error in counts of large gannetries, it is certain that the growth of many colonies has exceeded that which could have occurred from their own output, on known reproductive success and mortality rates. This is fully documented and discussed.

I deny that any confusion exists on p. 106 regarding fledging success, and also that in the first sentence fledging and breeding success are equated. The sentence reads: "Gannets have an exceptionally high fledging (and *thus* breeding) success; 89% of all eggs *hatched* gave fledged young." The "thus" follows from the high *hatching* success discussed earlier. High fledging success *thus* gives a high breeding success (fledged from laid). This is fully consistent with the ninth line (Schreiber's citation). I was pedantic enough to add, in brackets (l. 12) that it is "... Bonaventure fledging success (not breeding success)" that is being considered. The reviewer's sermon on the need for definition of terms is gratuitous. In fact, the comparative data in this section *are* fully valid, as any careful reader will find.

I have now reached halfway point in the review and already have taken up too much space. I accept that I fell grievously short in examining museum specimens, but how Ralph managed to miss the egg sizes and weights, given for every species, I simply cannot tell. He says "few egg size measurements are included for any species . . .," whereas a glance at the general index would have referred him, under "Egg, characteristics of;" to the species' Ecology accounts, each of which has a content list with page references. Some of the relevant tables take up half or even a full page! If that is a measure of the reviewer's care, the mistakes that I have already remedied become more readily understandable. Still, I am grateful for the kind remarks, too, and I hope this reply sets part of the record straight.

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