

times more likely to be found than mussels in the range 10 to 30 mm. This compares with a ratio of about 1.8x for the same size classes, for mussels recovered from the surface i.e. unburied shells, in Cayford's study (calculated from Figure 2: Carried shells - Class 5 (51 to 60 mm), 75% found; classes 2 and 3 (10 to 30 mm), 42.5% found, Ratio = 1.76. In situ shells, class 5, 55% found, classes 2 and 3, 30% found, Ratio = 1.83).

The differences in bias towards larger shells in the two studies probably reflect the different protocols which made finding small mussels easier in the Cayford study - that is high density of a known number of mussels in a small very intensively searched area. The dependence of the bias on the experimental protocol reinforces Cayford's suggestion that it is important for each observer to assess their own biases in each study. However it also suggests that the protocol used to quantitatively assess bias should match as closely as possible the collection procedures used in the relevant diet choice study. Cayford's conclusion that the effects of bias in shell collections are likely to be negligible is dependent upon the bias he detected and, as revealed in the current paper, this may be a gross underevaluation of bias for most studies which use searching procedures more closely matched by the test described here.

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BIAS IN THE COLLECTION OF MUSSEL SHELLS OPENED BY OYSTERCATCHERS: A REPLY TO SPEAKMAN

J.T.Cayford

John Speakman (1990) notes rightly that I had failed to refer to the test of mussel shell recovery bias presented in his PhD thesis (Speakman 1984). In doing so, Speakman has, however, made some slightly misleading interpretations of what I wrote in my original paper (Cayford 1988) and I clarify these points here.

Speakman states that "the extent of bias detected by Cayford (1988) does not necessarily reflect the bias involved in recovering shells during studies of prey selection". I agree and concluded my paper with the point that "because the extent of bias is as likely to differ between individual observers, it is important that each observer measures, and corrects for, his/her own bias regardless of the technique used".

Speakman makes three criticisms of my experimental design, namely that "shells were distributed at higher densities than those opened and discarded by foraging Oystercatchers; that the collector had a *priori* knowledge of the numbers of shells in each size class; and the time spent in searching for shells exceeded that spent searching for shells over larger areas in previous studies". Certainly these three features of my design differed from actual field conditions and procedures used by other workers. The main purpose of my study was, however, to explore the potential for bias in shell collections rather than to produce generalized quantitative predictions of bias for every previous study using similar methods. I clarified this in my discussion, by stating that "the extent of bias

found in this experiment is likely to be less than that in a real field study using the same technique (i.e. shell recoveries) because the observer had a *priori* knowledge of the number, approximate density and size-distribution of shells present; and the density of shells was very much higher than that found naturally".

Whilst the absolute density of shells on the mussel bed will in all probability influence the rate of shell recovery, only differences in the relative densities of different size classes could influence the probability of mussels of a particular size-class being found, independently of any size-dependent recovery bias. On the question of a *priori* knowledge, it is difficult to envisage circumstances in which a researcher studying Oystercatcher feeding ecology (which almost invariably necessitates making repeated shell collections) will not have some a *priori* knowledge of the approximate density and size-distribution of shells present on the bed prior to searching. If this is the case, an experimental protocol which fails to reflect this might actually overestimate bias. Clearly the amount of time spent searching per area of mussel bed could be critical if the extent of bias is negatively correlated with duration of searching, as might be expected. In this case Speakman is right to suggest that the protocol used to quantitatively assess bias should match as closely as possible the collection procedures used in the relevant diet choice study, i.e. where the results of the experiment are to be used to calibrate recoveries of shells made under field conditions. It was for just this reason that I quantified my recovery bias

separately for shells presented in three different orientations which matched as closely as possible the feeding method specialisms of Oystercatchers.

Speakman concludes by saying that my conclusion that the effects of bias in shell collections are likely to be negligible "is dependent upon the bias he detected and, as revealed in the current paper, this may be due to a gross undervaluation of bias for most studies which use searching procedures more closely marched by the test described here". In fact I concluded that "the main conclusion from the present study is that recoveries of mussel shells are also biased, but the effect of this is likely to be negligible during most months of the year, because Oystercatchers avoid the size-classes which are most prone to recovery bias". I further qualified this by pointing out that "in spring, when Oystercatchers on the Exe took small mussels, estimates of energy intake based on shell recoveries were subject to large errors".

Overall then this discussion highlights the key point of all this: the importance of

methodological testing in foraging studies of waders, and perhaps also the complexity of developing a good experimental design. I hope that our discussion will encourage more wader researchers to devise similar methodological tests for their own studies, and to report their results on a topic that is as yet poorly covered by published literature.

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NEW WORLD SECTION

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WHEN DOES THE WILLET 'PLOUGH' THE WATER TO CATCH FISH?

R. McNeil & J.R. Rodríguez

McNeil, R. & J.R. Rodríguez S. 1990. When does the Willet 'plough' the water to catch fish? *Wader Study Group Bull* 58: 50-51.

When feeding in turbid water in the Unare Lagoon, Venezuela, we observed Willets *Catoptrophorus semipalmatus* feeding by 'ploughing' through the water with a partly opened bill. The birds were seen to catch small fish. The 'ploughing' technique may be used when poor visibility prevents visual feeding.

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On 20 March 1987, while censusing shorebirds and other waterbirds of the Unare Lagoon, State of Anzoátegui, northern Venezuela, we observed 15 solitary Willets *Catoptrophorus semipalmatus* from the road which runs for 22 km that runs on the sand bar separating the lagoon from the Caribbean Sea. Only five Willets were seen feeding, but all five fed in the same way: they ran along the shore in shallow water (6-10 cm deep, generally above the tibiotarsal-tarsometatarsal joint), covering distances of 5 to 15 m in a straight forward rush, sometimes in zigzag movements, cutting the water with their partly open bill, half-length under water (Figure 1a). Occasionally, the whole bill

and/or much of the head was submerged (Figure 1b). Sometimes a rush ended with the bird moving to rest on the shore or moving to another nearby zone where the same behaviour started again, but on other occasions it ended with the bird catching a small fish almost as long as the bird's bill. On each occasion the Willet brought the fish on the wet sandy shore (above water line) and swallowed it after multiple pecks and shakes, apparently to kill it. We saw a Willet catching and swallowing three fish in the same way in an interval of 15 minutes.

Willetts generally feed on small crabs, marine