

given no supporting figures for correlated variations in fecundity recruitment or size of population. Hence there is no means of knowing whether recruitment is at the 'optimum level'.

Turning more briefly to the red grouse *Lagopus lagopus scoticus*, this bird does not bring food to its young in the nest, so it would be extremely unlikely that natural selection would restrict brood-size for the same reason as in the swift. Instead, I would suggest that a factor of minor influence in the swift may be much more important in the grouse, namely, the physiological condition of the female at the time of laying. Jenkins *et al.*⁵ showed that the heather on which red grouse basically depend for their food was in good condition in the spring in 1957, 1960 and 1961, in which years the average clutch was between 7.8 and 8.1 eggs, but that the heather was in bad condition in the spring in 1958 and 1959, when the average clutch was only 6.9 and 6.1 eggs, respectively. It is reasonable to attribute this variation to the food available to the females prior to laying. Further, this agrees with the earlier work of Siivonen⁶ on other gallinaceous birds in Finland, where the partridge *Perdix perdix*, capercaillie *Tetrao urogallus* and Blackgame *Lyrurus tetrix* likewise have larger clutches in those seasons when food is more abundant prior to laying. The physiological condition of the female prior to laying may not be the sole factor determining clutch-size in gallinaceous birds; for one thing, in all species so far studied, the average clutch is smaller later in the season. But the food for the female is clearly important, and there is, once again, no evidence for Wynne-Edwards's alternative view. Indeed, he has not yet produced any positive or quantitative evidence for it in any species of birds.

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¹ Wynne-Edwards, V. C., *Nature*, **201**, 1148 (1964).

² Perrins, C. M., *Nature*, **201**, 1147 (1964).

³ Lack, D., *Ibis*, **98**, 606 (1956).

⁴ Lack, D., *Swifts in a Tower* (London, 1956).

⁵ Jenkins, D., Watson, A., and Miller, G. R., *J. Anim. Ecol.*, **32**, 317 (1963).

⁶ Siivonen, L., *Pap. Game Res.*, **17**, 1 (1956); **19**, 1 (1957); *Suomen Riista*, **12**, 43 (1958).

DR. LACK'S communication clarifies some important points, among them the influence of weather at the time of laying in the swift, and the earliness or lateness of the laying date, on clutch-size. But, notwithstanding the errors in the figures I extracted on clutch-size frequencies, he does not challenge my statement that the mean clutch-size in the common swift both at Oxford and in Switzerland is well below the 'most efficient' clutch-size, this being the size that on average has yielded the largest number of surviving young. Lack outlines three suggestions as to how this situation could be accounted for on the basis of natural selection acting at the individual level.

The first concerns the cold, wet summers, when broods of two (derived from clutches of two or more) can turn out as well as or even a little better at Oxford than broods of three. In fact, such summers are in the minority, and the margin is at most rather small, so that any transient selective effect would be outweighed by the more substantial advantage to be gained in average and good years by starting off with a clutch of three. The breeding life of common swifts averages something like 4-5 years⁴; those that consistently lay three will, therefore, always tend to leave more offspring than those that lay two. In the alpine swift (*Apus melba*), studied at Solothurn, Switzerland⁵, a good many parallels were found to the common swift in breeding biology, including a distribution of clutch-sizes between 1 and 4 eggs; but the extensive data in this case do not reveal any parallel switch of advantage between good and bad years.

The second, that there is a selective advantage in reducing clutch-size as the season advances, is speculative with regard to the swifts at Oxford, not being supported by adequate evidence. In the alpine swift again, where there is a corresponding fall in the frequency of 3-egg layings as the season advances, the threes nevertheless appear to remain more productive than the twos, even after the latter have come to outnumber them⁷.

The third suggestion concerns the relationship established in the swifts at Oxford between the weather just prior to laying and the number of eggs laid. As Dr. Lack says, this appears to be a short-term physiological effect or phenotypic variation, modifying the innate fecundity-level. Natural selection could promote this kind of adaptability if it were hereditary and advantageous, but would be unlikely in so doing to have any effect on the basic fecundity level. Even in years when cold weather before laying depresses the average clutch-size, the threes that are laid can still be expected to do better than the twos.

I have emphasized elsewhere the fact that fertility in animals is widely influenced by contemporary events, particularly by changes in population density and economic conditions. In view of this, experiments of the kind reported by Perrins² seem to me to assume far too simple a conception of what is involved in determining breeding success. Environmental stress can condition such responses as the number of eggs laid, the tenacity of the parents in incubation and food-getting, the eviction or desertion of eggs or young, and so on; and where a pair of birds have become physiologically conditioned to making a reproductive effort of a certain magnitude it is hardly to be expected that they will automatically adjust themselves to a larger one, if the experimenter presents them with an extra chick. If they then fall down on the job, what valid conclusion can be drawn?

Finality on the difficult problems discussed here is not likely to be attained by a further exchange of correspondence in *Nature*, however stimulating. It can reasonably be concluded that there are unresolved difficulties about establishing Dr. Perrins's objections² to the concept of intergroup selection⁸, which is where the correspondence started. Whether the concept itself has suffered at all in the discussion it is for others to judge.

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⁷ Lack, D., and Arn, H., *Orn. Beobachter*, **44**, 188 (1947).

⁸ Wynne-Edwards, V. C., *Animal Dispersion in Relation to Social Behaviour* (Oliver and Boyd, Edinburgh and London, 1962).

⁹ Wynne-Edwards, V. C., *Nature*, **200**, 623 (1963).

A Hermit Crab New to Britain

It is of interest to record the occurrence on the shores of the Isles of Scilly of a hermit crab new to Britain. During the course of a collecting trip with my students to Porth Hellick, on the south-east corner of St. Mary's, in September of 1962 my attention was attracted by a small hermit crab with brilliant violet antennæ. Being unable to determine it to my satisfaction, I sent it to Dr. I. Gordon at the British Museum (Natural History), who identified it as a male *Catapaguroides timidus* (Roux), a species which she knew well in the Mediterranean. I have now taken a second male of the species, confirmed by Dr. R. W. Ingle, of the British Museum, this time from a bay near Gulf Rock at the south-west tip of the island of Bryher (Isles of Scilly). The two records, from widely separated localities among the islands and at an interval of two years, coupled with the fact that no systematic search has been undertaken in the interim, suggest that the species is established there.

The hitherto known localities are summarized by Bouvier¹ as the Atlantic at Roscoff and the Canaries,