seem to be two ways around this: disturbance could be eliminated by making observations at a distance, or the "cost" of the method could be measured in controlled experiments, provided that an identical and independent measure of nesting success were used for both the experimental and control areas.

My major concern with this paper, however, is that it may be misinterpreted. Just because a study causes mortality does not mean that it necessarily has an effect on the population of the species being studied. Unless this is clearly pointed out, refuge managers and state and federal officials may quite understandably react to reports of scientist-induced mortality by restricting or denying access to colonies or species just when these are in need of careful and sympathetic study.

The Auk and other journals should routinely expect their authors to demonstrate the absence or at least the probable magnitude of investigator-induced mortality in productivity studies. Investigators unable or unwilling to do do this may find it more sensible to use methods for which the biases are already known (e.g. Tremblay and Ellison 1979, Auk 96: 364). An additional benefit of this might be greater standardization of methods of measuring reproductive success, which would allow more confidence in comparisons between different studies. *Received 27 March 1979, accepted 22 May 1979.*

Author's Response

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We welcome Duffy's valuable comments on our paper and on disturbance studies in general. Here we reply to three specific points before adding our own general comments. First, Duffy asks if the intense destruction of eggs and young documented on Île-aux-Pommes in a late-nesting (July) colony of Double-crested Cormorants (*Phalacrocorax auritus*) can be considered representative of the species? Probably not. Drent et al. (1964, Can. Field-Naturalist 78: 208–263) found, however, that human disturbance caused similar losses in July among Double-crested Cormorants who laid late clutches. In fact, these authors also reported that early visits in May induced such high predation losses that the investigators were finally forced to evaluate reproductive success by telescope from a blind.

Second, Duffy correctly cites our data showing no difference in mean clutch size or mean brood size in experimentals vs. controls on one island. These means, however, were based only on nests with eggs or young. Losses of total clutches or total broods were not considered. Thus the means are not necessarily a valid test for disturbance effects.

Third, Duffy notes that only one of four comparisons on Gros Pèlerin showed a greater percentage of empty nests in a disturbed site and that even for that comparison it is possible that late-nesters dismantled empty nests in the control. This criticism is justified. But even if one were to assume that each late-nesting couple dismantled an empty nest in the control, there would still be an excess of about 10% empty nests in the experimental on the ground in 1975 (Table 3). Moreover, there were more young outside their nests in the control than in the experimental in 1975, and these young were not allocated to any specific nests (footnotes, Table 3). This artificially increased the proportion of empty nests registered for the control. Thus we believe there is good evidence that nest failure was relatively high in the disturbed ground colony in 1975. We think it important also to recall that the disturbance effects recorded in the four experimentals corresponded with the intensity of disturbance and the susceptibility of the birds to disturbance. The two ground colonies were disturbed more than the two tree colonies. Eggs and young were not manipulated in the tree colonies and all adults were not forced from nests. Cormorants seemed more susceptible to disturbance in 1975 than in 1976. It is not surprising, therefore, that the ground colony in 1975 manifested two effects of disturbance, many empty nests and few late nests. The tree colony in 1975 also had few late nests. In 1976, the ground colony had few late nests, but no disturbance effects were noted in the tree colony.

We would like, however, to stress that 1975 may have been an exceptional year on Gros Pèlerin. A long-term study might reveal that Double-crested Cormorants on Gros Pèlerin are rarely as susceptible to disturbance as they were in 1975. Snow (1960, Ibis 102: 554–575) felt that her activities in a colony of Shags (*Phalacrocorax aristotelis*) induced a high predation rate on eggs in only 1 of 4 years when adults did not brood closely, perhaps because of food shortage.

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The main weakness of our study was our ignorance of what really happened in the infrequently visited controls. For example, we do not know how many nests failed and were dismantled in the controls, and thus we cannot quantify the effects of disturbance. This precludes an accurate calculation of fledging success per breeding pair, an important parameter in most population models. We would like to see someone evaluate disturbance effects in colonial nesting birds by comparing a disturbed and a completely undisturbed colony for which one had recorded as many reproductive events as possible. Such a study of cormorants would be feasible in some situations by recording data for the control from cliffs or towers.

We agree with Duffy that, even if investigator effects are shown in a study of Double-crested Cormorant nesting success, one cannot assume that there will be any significant effect on subsequent breeding densities. The loss of eggs or young may be compensated for by replacement eggs in partially depleted clutches, renesting when the entire clutch or brood is lost, increased survival rate of young remaining in a nest or in the autumn population, and nesting by pairs who may have otherwise been socially inhibited from nesting. Our concern is not that investigators necessarily have any effect on breeding densities, but rather that researchers may sometimes report productivity data that are biased by their activities. This bias will usually be in the direction of underestimating the breeding success that would have occurred in the absence of the investigators. An unfortunate consequence in some of these instances may be to blame predators rather than human-induced predation for the low breeding success. In other circumstances researchers may overestimate breeding success by initiating their study late in the breeding cycle to avoid disturbance during the critical periods of laying and early incubation. Here early nest failures may go unrecorded. The problem of the relationship between stage of development of nests when first observed and hatching rate is discussed by Mayfield (1975, Wilson Bull. 87: 456–466). Breeding success may also be overestimated by assuming that all late nests represent renesting, whereas, in fact, some of the late nests may simply be those of pairs laying for the first time. Inflated estimates of nesting success may mask effects of investigator disturbance and may prevent the detection of adverse environmental influences on reproduction.