SHORT COMMUNICATIONS

EFFECT OF A CATASTROPHIC HAILSTORM ON BIRD POPULATIONS

Some ecologists, notably Andrewartha and Birch (1954) and Andrewartha (1961) in Australia, have argued strongly against theories involving the regulation of animal populations only through density-dependent means. Such arguments stress the importance of density-independent regulation and emphasize, if often only inferentially, environmental catastrophes such as frost, drought and fire. Others (e.g. Lack 1954, 1966) have strongly opposed such theories, arguing that catastrophic effects must really be unimportant. Because catastrophes can rarely be foreseen, probably few ecologists have had the experience of observing their direct effects on populations in the field, and hence may tend to assume their insignificance.

I had such first-hand experience on the evening of 17 November 1970 at Dyer's Lagoon, 2 km west of Laidley, Queensland. I had previously chosen an area for studying Noisy Miners *Myzantha melanocephala* on the more heavily wooded western side of the roughly triangular lake. The lake is a natural catchment with a low earth dam at its northern end. I had known it only when the water was low and when it was at most about 500 m long, covering an area of roughly 11 hectares. Being very shallow with much exposed mud, it normally abounds with swans, ducks and waders. Most of the surrounding land has been cleared of trees, but a few savanna-like stands of living and dead, ring-barked, acacias and eucalypts remain in places. My study-area comprises about 30 ha of the densest timber. Some larger trees have been removed, and those remaining range in height from 9 to 33 m. All the area is or has been grazed by cattle.

At 18:25 a deluge of rain issued from pitch-black clouds across a pale-green sky. Almost immediately hail followed, lasting thirty minutes. In flashes of lightning striking nearby, I estimated the diameters of hailstones at 15 to 40 mm. The rain and hail, driven hard from the south-west, was accompanied by gusty winds. Rain continued until 19:40, when nearby roads in low places were impassibly inundated, and in higher places covered with divots, branches and other debris washed from adjacent fields and bush. There was little more rain, and the official registration at Laidley was 83 mm (328 points).

When I emerged next morning at first light, the lake was full; a rise of roughly 1.5 m had extended its surface to a length of 1.2 km and area of 30 ha. The surface in bays on the western side was completely covered with the thick brown muck of leaf-litter and floating debris blown in by a steady south-easterly wind. I inspected the study-area and found that most litter had been washed away, many large boughs had been blown down, and at least eighteen trees had been struck by lightning, leaving jagged splintered stumps. (According to Professor S. A. Prentice, Department of Electrical Engineering, University of Queensland, such a concentration of lightning strikes is most unusual, and far exceeds that of any local record.) The ground was covered thinly with fresh green leaves, but only in the open were crowns of trees largely defoliated. In sheltered places, masses of hailstones covered with litter reached depths of 15 cm. Their diameters ranged from 10 to 20 mm. The *Courier-Mail*, Brisbane (23 November: 11), described this as the most violent storm in fifty years and printed a photograph of hail one and a quarter metres (four feet) deep and *still present* on 22 November.

Five known active nests of Noisy Miners were destroyed by the storm. All were badly flattened or smashed. But no dead or injured miners were found, and most colour-banded members of territorial groups were accounted for.

At 11:00 I noticed a few dead Black-winged Stilts *Himantopus himantopus* drifting towards shore. Along the western shore and revealed offshore through binoculars I counted forty-one dead Stilts. Obviously not all dead birds were found. No dead bird of any other species was seen, although dotterels and sandpipers would have been easily overlooked in the floating muck. I counted seventy-eight live Stilts on the now grassy edges of the lake. Other species present, but in much lower numbers than a week before, included Black Swan *Cygnus atratus*, Black Duck *Anas superciliosa*, Grey Teal *A. gibberifrons*, Black-fronted and Red-kneed Dotterels *Charadrius melanops* and *C. cinctus*, and Marsh Sandpiper *Tringa stagnatilis*.

On 4 November, Mr R. J. Hadley of Goodna had camped with me at the lake and censused waders. He counted 200 Stilts, then much occupied with courtship and agonistic displays. On 11 November, I counted 194. Hence the overnight destruction of forty-one birds represents a conservative 20 per cent, a substantial mortality in this local population.

Why should Stilts be more susceptible to destruction? These birds are active much of the night at this lake. They are also, by far, the most numerous wader. If they had taken flight during the hailstorm, perhaps in response to thunder or lightning or to escape the hail, they would have been quickly smashed into the water if not killed or injured in flight. They may then have perished through drowning or exposure.

A note by Vestjens (1971) is of interest because it suggests that violent hailstorms, although isolated, may be fairly frequent at certain times of the year. He described an even more violent storm in New South Wales eight days before my own observations. Vestjens recorded severely injured or dead birds of several species. This perhaps supports my suggestion that Stilts suffered because of their nocturnal activity. Most birds in my area, unlike those in Vestjens's, would have just gone to roost before the storm struck. D'Ombrain (1938) also described, although not first-hand, the destruction of large numbers of Black Ducks and several other species in a hailstorm near Maitland, NSW, late in December. This, the most severe of several within a fortnight, left hail on the ground to depths of two metres. In these accounts, birds inhabiting open country or edges of swamps or lakes seem, reasonably enough, most prone to injury or destruction. And Hindwood (1964) investigated a remarkable incident in which a Spine-tailed Swift *Hirundapus caudacutus* was apparently killed in flight by colliding with a hailstone.

Although catastrophic events as isolated and instantaneous as these have such obviously devastating effects on local bird populations, it is difficult to ascribe to them any far-reaching significance in the ecology or regulation of populations of birds such as Stilts or Noisy Miners in south-eastern Queensland or probably elsewhere.

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11 May 1971.

EXTENSION OF RANGE OF THE BRUSH BRONZEWING

On 5 October 1968 with Messrs M. Streeter and P. Disher, I camped in stunted mallee-country thirty-four km south of Pinnaroo and about sixteen km west of the Victorian border. In conversation, Mr Streeter mentioned that he considered a useful character for differentiating the two bronzewings in flight was the tail, which was shorter in the Brush Bronzewing *Phaps elegans* than in the Common Bronzewing *Phaps chalcoptera.* That evening a bronzewing, flushed from a roadside pool, appeared to have a short tail and it was thought that this invalidated his theory because the area seemed well outside the known range of the Brush Bronzewing.

However, in the same area on 17–18 October 1970, this time with Messrs M. Streeter, D. Maxwell and E. Milne, a Brush Bronzewing was seen on a nest containing two eggs, built on top of a *Triodia* bush sheltered by some stunted mallee. The bird was identified as *elegans* by its slate-blue breast, rufous sides of neck and creamy buff forehead. The Brush Bronzewing was not observed at water on this occasion although the Common Bronzewing was fairly numerous.

Condon (1969, Handlist of the Birds of South Australia) gives no precise range for the Brush Bronzewing but says: 'not venturing far into the drier mallee'. Wheeler (1966, Handlist of the Birds of Victoria) shows the range of the species in south-western Victoria probably not north of the Grampians, where it was recorded by Austin (May 1963, Bird Observer), and definitely excluded it from the Mallee District, though his distributional map suggests that it may also not occur in the Wimmera District. McGill (1960, Handlist of the Birds of New South Wales) gives an essentially coastal distribution for the species in that State.

The occurrence of the Brush Bronzewing so close to the Victorian border suggested that it might also occur in adjacent areas of that State, as is supported by an old record of *elegans* from north-western Victoria by Howe and Tregellas (1914, Emu 14: 76) who recorded a bird with two chicks about two weeks old in 'porcupine and small mallee'. The precise locality is difficult to determine but was probably about sixteen km north of the present Carina siding, which in turn is eight km west of Murrayville.

Mr W. R. Wheeler (*in litt.*) informed me that both species of bronzewing had recently been recorded at Bunn's Bore, about midway between Pinnaroo and Bordertown and some thirty km south of our camp, by Messrs T. Hunt and R. Kenyon, who kindly supplied the following information: 'Brush Bronzewings were first positively identified on 3 November 1970 when a pair drank at a small pool of water at 08:00. They were in full sunlight and were observed from 25 m with 7 × 35 binoculars. At about 18:30 on the same day four Brush Bronzewings and four Common Bronzewings were observed drinking at the edge of the bore. The following evening two Brush and three Common were drinking. When both species were seen together the most striking feature was the much fatter appearance of the Common, particularly around the belly. In comparison the Brush appeared very sleek birds. The "marbled" back of the Common and the rust coloured marking of the neck of the Brush were also very prominent.'

Thus *P. elegans* is probably well represented in the desert-country between Pinnaroo and Bordertown, its range possibly being controlled by the availability of water. Mr Hunt and myself consider that *P. elegans* will also be found to inhabit those parts of the Big Desert in Victoria where water is available. Hunt and Kenyon (1970, Australian Bird Watcher 3: 222–226) did not record bronzewings in the Big Desert in 1969, but there appeared to be no surface water in the area covered.

Chisholm (1946, Emu 46: 173) said in a general way that both bronzewings were fairly common in the mallee, but, as his article referred to a much wider area than Wheeler's (*op. cit.*) Mallee District, it is possible that his remark stemmed from experience in the vicinity of Maryborough where both species are known to occur. Howe made many trips to the north-western Victorian mallee and he and his associates published numerous papers on birds of the area in the EMU during the first half of this century, yet mentioned the Brush Bronzewing once only (*loc. cit.*). Records of some species in these papers seem to have since been disregarded, as being doubtful, and that of the Brush Bronzewing may have been treated similarly. However, our recent observations of the species in similar habitat not far south indicate that the identification from about sixteen km north of Carina was probably correct. This locality would then become the northernmost limit of the known range of the species in western Victoria and nearby South Australia.

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AN UNSUCCESSFUL SEARCH FOR THE SOLOMON ISLANDS CROWNED PIGEON

The Solomon Islands Crowned Pigeon *Microgoura meeki* is known only by six* skins and an egg collected by Meek on Choiseul in January 1904. Using unpublished information in Meek's letters to Tring Museum and in the 1927 and 1929 journals of the Whitney South Sea Expedition, I discussed Meek's three camps on Choiseul and presented evidence that the specimens of *Microgoura* had been secured by Meek's party at Choiseul Bay (Parker 1967a and b). Although the Whitney Expedition failed to find *Microgoura* during two visits to the island, I thought that a further search would be justified.

From 13 February to 12 March 1968 I visited Choiseul and the nearby islands of Rob Roy and Wagina in search of this aberrant and elusive ground pigeon. Based on Wagina I circumnavigated Choiseul by motor-launch, calling at the coastal villages to enquire about the bird. I also visited by canoe several of the tiny islands lying between Wagina and Choiseul.

I have described elsewhere Meek's route round Choiseul in 1903–4 (Parker 1967b). Verification of the site of one of Meek's three camps on Choiseul (the first) as Tambo Tambo Island was forthcoming from two very old men of Varangga village on the opposite mainland, Semi Vazarabatu and Joel Vanamola, who told me that as boys they had actually worked for Meek during his visit. They recalled that he had anchored the *Hecla* offshore at Tambo Tambo, collected birds and butterflies along the nearby Tarapa River and dynamited fish for the villagers.

Choiseul Bay, almost certainly where Meek's party obtained the six extant specimens of *Microgoura meeki*, is full of tiny islands, which in Meek's day were covered with the graceful *zaru* tree, a species of *Casuarina*. In 1919–20, however, these islets were cleared of all but their mangrove fringes by Burns

* Meek, in a letter to Hartert from Gizo dated 18 January 1904, wrote that he had collected six specimens. Rothschild and Hartert (1905) however stated: 'Mr Meek sent seven specimens, of which six are in the Tring Museum.' Of these, one is now in the British Museum (Natural History) and five are in the American Museum of Natural History. If there was a seventh specimen, Rothschild may well have given it to some royal friend, as seems to have been his wont.

Philp for planting coconut palms, and now present the sorry aspect of neglected plantations choked with low secondary growth. The opposite coast was similarly abused. Meek would not have been the first naturalist to visit Choiseul Bay. During her visit to the Solomon Islands in 1882–4 HMS *Lark* anchored there, and her officers shot a number of pigeons, *Caloenas nicobarica* and *Ducula*, which were dissected by the ship's surgeon, H. B. Guppy, who made notes on the structure of their gizzards (Guppy 1887).

Only the older men of Choiseul to whom I spoke recalled *Microgoura*, usually from areas along the sheltered southern coast. All who knew the bird said that they had not seen it for many years and that cats, introduced to keep down rats, had finished the species. Dogs used on pig-hunts would also catch them. The most recent record I obtained was from the inhabitants of Sasamungga on the Kolambangara River (apparently a former stronghold of *Microgoura*) of a small roost seen during World War II, i.e. in the early 1940s. This would confirm the presence of *Microgoura* in the Kolambangara basin at the time of the Whitney Expedition's search, when in spite of native advice and help the expedition failed to find it. From other parts of the southern coast it seems to have disappeared earlier, during the 1920s and 1930s, if one may place any reliance on native estimation of the passage of time. Its absence from the area of Choiseul Bay by 1927–9, when Beck and Hamlin of the Whitney Expedition called there, may have been because of the development of coconut plantations and the almost complete removal of the original vegetation.

My chief hope had been to find *Microgoura* on the swampy, almost undisturbed, islands of Rob Roy and Wagina, the former uninhabited, the latter inhabited only since the Gilbertese settlement of 1964. I failed to find any trace of the bird there. The Gilbertese, who did not recognize my picture of *Microgoura*, remarked that they had found feral cats common on Wagina when they arrived.

Information that I got from the Choiseulese concerning the habits of *Microgoura* tallied with that given to members of the Whitney Expedition in 1929. The bird was widely known as *kukuru-ni-lua*, was usually terrestrial and roosted in small groups on low branches, which sites could be found by the piles of droppings below. Its call was imitated as a low 'c-r-r-ooo, cr-ooo, cr-o-o-o' (possibly Hamlin's 'low trilling sound'). It lived in lowland, often swampy, forest, but not in mangroves.

Two informants, one from Sasamungga and one from Kiala, told me that stones were often found in the gizzard of *Microgoura*. According to Chalmers and Gill (1885: 317) stones are also found in the gizzard of the related New Guinea crowned pigeons, *Goura*: 'Inside the gizzard of each Goura pigeon there is a good-sized pebble much prized by the natives as a charm against spear thrusts and club blows.' One of my Choiseulese informants described the gizzard-stone of *Microgoura* as being of gold, thereby signifying perhaps that such stones were regarded as valuable. Guppy (1887: 323–325), dissecting many specimens of the Nicobar Pigeon *Caloenas nicobarica* in the Solomon Islands, invariably found in the extremely muscular gizzard a quartz pebble about 12 mm in diameter. The strong gizzard and stone together enabled *Caloenas* to crush such hard seeds as those of *Adenanthera pavonina*, which Guppy (1906: 159–160) considered required for their fracture a blow with a hammer. Guppy found that the fruit pigeons he got (*Ducula*, probably mostly *D. pistrinaria*) had a comparatively thin-walled unmodified gizzard; they digested the fruit pulp and ejected the hard seeds and kernels.

Meek (1913: 187) was told by natives that *Microgoura* occurred also on Santa Isabel and Malaita. The Whitney Expedition searched both islands in the 1920s without finding the bird. Ever hopeful, I spent from 26 January to 7 February 1968 on the western coast of Malaita making enquiries among the natives. I found that no one knew of *Microgoura* at all, though people sometimes at first confused my coloured picture of that species with such crested birds as *Aviceda subcristata* (*kito* or *ito*, the Crested Hawk) and *Reinwardtoena crassirostris* (*gwalolē*, the Crested Pigeon).

Although one cannot say even now that *Microgoura meeki* is extinct, the likelihood of its survival is small. The feral cat, its reported predator, has been widespread in the Solomon Islands for several decades.

My peregrinations in the Solomon Islands were financed in part by a grant from the Frank M. Chapman Fund of the American Museum of Natural History, New York. To Mr E. Palmer of Gizo, to Mr S. Kondovaru, Chief Clerk of Choiseul, and to Umea and Airam of Wagina I extend my warmest thanks for their hospitality, their boats and their services as interpreters.

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CONTINENTAL DRIFT AND THE HISTORY OF THE AUSTRALIAN BIRD FAUNA

Building of landbridges was a favourite sport of biogeographers during the first three decades of the century (and earlier). Even the most remote atoll of the Pacific was connected with the nearest mainland by such bridge-builders as Crampton or Skottsberg. When the worst of that fashion was over, theories of continental drift became popular, theories which made one fatal mistake: they placed the time of the drifting (and preceding land-connexions) as recently as the late Tertiary and Pleistocene. Their opponents had little trouble proving that these assumptions could not be reconciled at all with the observed patterns of distribution. It was during this period that I published a series of papers (Mayr 1944a and b, 1951) in which I tried to show that the composition of the Australian bird fauna could be explained in terms of the existing constellation of continents. I contended that all Australian bird taxa, the relatives of which (outside Australia) can be determined, could have colonized Australia from Asia, having used the Indonesian islands as stepping stones. I furthermore stated (Mayr 1952) that these findings did not disprove drift, provided the break-up of Gondwanaland had taken place before the evolution of the existing families and genera of birds, that is before the middle of the Jurassic.

A great deal has happened in geology in the twenty-five years since these papers were written. Continental drift is now firmly established, and the nature of the argument has changed. The questions now asked by the geologist are: Which of the now separated land masses were once connected? When did the separation occur? How rapidly did the land masses drift apart? And consequently, what distances of open water separated them 30, 60, 90, 120, 150 and 180 million years ago? The new understanding of palaeogeology makes it mandatory that the zoogeographic interpretations be re-examined. This was done for Australian birds by Serventy (1971) in an analysis in which he asks which components of the Australian bird fauna might have arrived by ways other than the Indonesian stepping stones. Are some of them so old that they might be remnants of the old Gondwanaland fauna that predates the break-up of the old Gondwana continent? Have others arrived in Australia when Africa, Antarctica or South America were still much nearer to each other than they are today and thus within reach of an avian colonist that could jump moderate water-gaps, but not those of such width as the oceans now isolating Australia? It was the object of Serventy's analysis to identify those components of the Australian bird fauna that might qualify for such intercontinental colonizations, preceding the traditional colonization from Asia to Australia via Indonesia.

Serventy does not question that the majority, indeed the vast majority, of Australian birds arrived from Asia by island-hopping. The question he raises is whether or not there is an old element in Australia that arrived by a different route. This is for at least two reasons a completely legitimate question. One is that the climatic situation in the Mesozoic and earlier Tertiary was quite different from what it is today. Even Antarctica was inhabitable in those days and could have served as a stepping stone for immigrants, even if it did not have at that time a direct land connexion with Australia. The other reason is that the distances between the southern continents were much smaller in the first 50–100 million years after the Triassic (or early Jurassic) break-up of Gondwanaland than today. During that period birds could have jumped water-gaps that now form total barriers.

Both of these possibilities raise a series of new questions. The first of these is when exactly did Australia break away from its Gondwanaland partners, Africa and Antarctica? The recently discovered Antarctic beds rich in fossil reptiles and amphibians which have a fauna almost identical with a corresponding one of South Africa are of lower Triassic age. A very close connexion must have existed at that time. The evidence for later connexions is not so good although Antarctica seems to have had an equable climate well into the Tertiary. The presence of marine deposits of Jurassic age between some of the continents makes connexion after the middle of the Jurassic highly improbable.

The second problem is the age of the avian orders and families. *Archaeopteryx* is still very reptilian, and the branching of the birds from the reptiles could not have happened very much earlier than the

time of *Archaeopteryx*, hardly earlier than the middle Jurassic, let us say 145 million years ago. It is evident from this figure that the exact timing of the break-up of the Gondwana block is of crucial importance. Unfortunately, this date is still highly controversial, some authors placing it as late as the early Tertiary, others into the earliest Jurassic. Because some non-passerine families go back about seventy million years, this date is indeed crucial.

It is evident then that the last word cannot be said until there is more information available on the geological chronology.

What can be done, however, at this time is to take another look at those bird taxa that might have reached Australia by another route than by island-hopping from Asia.

The Ratites are, of course, the foremost candidates for this category, as pointed out by Serventy. However, in spite of the evidence provided by Bock (1963), Sibley (1960) and Meise (1963) doubts still linger. The distribution pattern (two living and some extinct species South America, none North America, one species Africa, one or two species (extinct) Asia, none south-eastern Asia and islands, several species Madagascar, three species New Guinea, two species Australia, and many species New Zealand) is quite odd. Why have the Ratites failed to speciate more extensively in South America, Africa and Australia, considering their speciation in Madagascar and New Zealand? Are purely ecological reasons responsible for the prevention of radiation on the continents? The possibility that the similarities between the ratite genera are due to convergence is still not yet entirely ruled out. The new techniques of protein analysis will presumably resolve these doubts within the next ten or fifteen years.

The *Forpus* (*Amoropsittaca*) group of South American parrots represents a second candidate for a non-Asiatic faunal connexion. Indeed, if they are as closely related to the Australian Platycercines, as it would appear from Brereton's (1963) work, an Asiatic connexion would seem most unlikely. The absence of true Platycercines in the Papuan Region and the weak representation by related tribes in both the Papuan and the Oriental Regions confirm this conclusion. The similarity of the Australian and South American genera would suggest to me that the separation could not have taken place much further back than the Oligocene, at the most the Eocene. Whether the colonization took place via Antarctica or via some of the archipelagos of the Pacific that were then greatly enlarged cannot be decided until we know more than we do now about the distribution of land in the early Tertiary. As several students of the Polynesian avifauna have pointed out, some parrot genera, such as *Cyanorhamphus* and *Vini*, are excellent trans-oceanic colonists. The *Forpus* group might actually have reached South America at a time when the water-gaps were not greatly narrower than they are now.

A relict genus like *Rhynochetos* (New Caledonia) is not a good test for zoogeographical theories, and I shall say nothing about it. The four families of Suboscines of the Australian Region (Pittidae, Menuridae, Atrichornithidae, and Acanthisittidae) would be important if they formed a single assemblage with the South American Suboscines. This, however, is indicated neither by the morphology of their syrinx (Ames 1971) nor by their egg-white proteins (Sibley 1970). The Acanthisittidae seem to be an old relict element (perhaps related to the Pittidae), and the Menuridae-Atrichornithidae are not too distant from the true Oscines.

Little can be said about the two New Zealand oscine 'families', Callaeidae and Turnagridae. Because evolution of isolated species often progresses much more rapidly than that of widespread species rich in individuals, it is by no means certain that these are 'old' families.

The flamingos (Phoenicopteridae), which Miller (1963) proved to have formerly existed in Australia in three genera and four species, are so widespread and like many other waterbirds known for their extensive colonizing flights that they would seem poorly qualified as evidence for Gondwanan connexions. Still, it is noteworthy that they achieved a particularly great diversity in the three southern continents (South America, Africa, Australia).

The taxa that I have mentioned just about comprise the names of 'the older elements of the present Australasian bird fauna [that might] have reached Australia via Africa and Antarctica during the earlier stages of fragmentation of Gondwanaland' (Serventy 1971). The example of the flamingos, formerly richly represented in Australia but exterminated during the climatic vicissitudes of the Pleistocene, induces caution. There might have been old Gondwanan elements in Australia which also became extinct, as did the flamingos. We shall never know unless fossils are found. Unfortunately, the chances for that are greatest among large-sized waterbirds that are least helpful for our purposes because they have actively engaged in trans-oceanic colonizations even during the times when the oceans had acquired their present width.

It hardly needs stressing after the foregoing discussion that the evidence for an old Gondwanan element in the bird fauna of Australia is slight, if not non-existent. Further research on the proteins (and possibly other characters) of the Ratites will soon provide decisive evidence for their relations. The fossil record may provide additional information

on the former distribution of genera of flamingos. Finally, a more reliable mapping of the distribution of land-masses (and now submerged archipelagos) during early and middle Tertiary may allow a more positive reconstruction of the probable route of colonization of the South American *Forpus* group.

This much is certain, however, that the long-standing thesis that Australia received nearly all of its bird life from south-eastern Asia through island-hopping is still fully valid. Only further research can elucidate whether (and if so, which) other routes were used in the Mesozoic and early Tertiary.

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BREEDING OF THE MAGPIE GOOSE AT THE SERENDIP WILDLIFE RESEARCH STATION

The Magpie Goose *Anseranas semipalmata* has been recorded as breeding in captivity in at least two places, the San Diego Zoo (Delacour 1954) and Slimbridge (Johnsgard 1961), where isolated pairs have bred several times. In contrast this note describes the breeding of a pair in a captive flock.

A flock of Magpie Geese was established at the Serendip Wildlife Research Station, Lara, Victoria, in 1964 when 15 birds were transferred from CSIRO, Canberra; 102 birds were added to the flock in 1965. In 1968 the flock totalled 99 birds, consisting of 34 males and 65 females, aged 4–5 years. The flock was kept in a pen of 12,000 m^2 (with a pond of 2,000 m^2), and all birds were pinioned. Before 1968 no breeding was noted, although several platforms of grass were built on dry land in November 1966 and March 1967.

On 12 November 1968 one pair was observed building a nest, which they completed. The nest was constructed of short vegetable material, including Ryegrass *Lolium perenne*, clover *Trifolium* spp and Spikerush *Eleocharis* spp. The nest was constructed 6 m from the nearest permanent water, in the centre of several dense clumps of dock *Rumex* spp. The pen was flooded at intervals of three weeks from late September onwards, and surface water often lay round the nest-site for short periods.

Laying began on 14 November, and an egg was laid on 16, 18, 19, 21, 25 and 29 November. The dimensions of the eggs were within the range given by Frith (1967), and their mean weight was 108 g (range 95–115 g). Eggs 1–3 and 7 were placed in an incubator. After Egg 6 was lost on 17 December, Eggs 4 and 5 were also placed in the incubator. All eggs removed from the nest were replaced by either eggs of the Cape Barren Goose (from the population at Serendip), or large eggs of the domestic hen. On 22 December Eggs 4 and 5 were returned to the nest because the goslings could be heard inside the eggs, and both hatched on 23 December. On the morning of 24 December the female parent was found dead near the nest, and one of the two recently hatched goslings was found dead in the nest. The live gosling was tended by the male until midday on 24 December, when the male deserted and joined the flock. This gosling was immediately placed in a brooder. A post-mortem and other investigations failed to show why the female or the gosling died, or how the egg was lost from the nest.

Incubation of Eggs 1–3 began on 22 November (at 37.5 °C dry bulb, 30–31 °C wet bulb). Egg 7 was placed in the incubator on 29 November, and Eggs 4 and 5 on 17 December. On 22 December Egg 1 hatched in the incubator, and this was the only fertile egg of those retained in the incubator.

The two surviving goslings, a male from Egg 4 or 5 in the nest and a female from Egg 1 in the incubator, developed satisfactorily. They could be

readily sexed by the seventh week. The female had reached full development of the wing by the seventh week, and could fly. The male bird was approximately one week behind the female in development of the feathers.

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DESTRUCTION OF NEST-CONTENTS BY CUCKOOS

Readers may have noticed an editorial comment on Mr Fien's (1970) note regarding the behaviour of a Horsfield Bronze Cuckoo *Chrysococcyx basalis* at the nest of a Red-backed Wren *Malurus melanocephalus*, to the effect that similar predation is also known for the European Cuckoo *Cuculus canorus*. The comment was prompted by remembrance of an event that I witnessed as a child and by belief that destruction of nests by cuckoos was recorded in the literature. Indeed, Jourdain in Witherby *et al.* (1938) said that 'there is good evidence that the Cuckoo occasionally destroys nests with young of possible fosterers: by this means replacement layings become available for parasitizing'.

However, it is doubtful what this evidence is. Stuart Baker (1942), who discussed cuckoos comprehensively, made no mention of destruction of fosterers' young by any species of cuckoo. He mentioned the destruction of whole clutches without personal experience of it and sounded sceptical, though he wrote that 'Chance has, I think, quite satisfactorily proved it to occur'. But Chance did not record it in *The Cuckoo's Secret* (1922), though he may have done so in *The Truth about the Cuckoo* (1940), which I have not been able to consult. Bannerman (1955) in a long article on *C. canorus* did not mention such destruction, nor did Southern writing on parasitism in *A New Dictionary of Birds* (1964). Dr Friedmann (*in litt.*) informed me that he did 'not recall any instance of similar behaviour by any of the small glossy cuckoos'.

The literature on cuckoos is vast and scattered. I have not been able to search it exhaustively or even adequately. Yet, among the references given in *Aves* (Zool. Rec.) from Volume 87 (1950) onwards I have found no mention of destruction of fosterers' young in those papers available to me, and judge that few, if any, of those not available to me hold out much hope of mentioning the occurrence. Indeed, I found only two interesting, and almost relevant, records in my search. One was by Chalk (1950), who described carefully how a Golden Bronze Cuckoo *C. lucidus* removed from the nest of a Yellow-tailed Thornbill *Acanthiza chrysorrhoa* one egg of the Thornbill and one of its own species, leaving the nest empty. This account, incidentally, shows that the Cuckoo can, and does, enter domed nests, so that it need not always, perhaps ever, introduce its egg in its bill. The other account (Wright 1955) was of the destruction of three eggs in the nest of a Dunnock *Prunella modularis* by *C. canorus*, but was chiefly interesting because the editors had commented that this was 'further evidence that the Cuckoo occasionally destroys the eggs *or young* [my italics] of possible fosterers'.

I conclude that the habit has been recorded seldom and published in obscure journals because otherwise Baker, Bannerman and Southern would not have missed it; or that it has been noted only privately and not published, which might account for Jourdain's inexact reference, probably copied by the editors of *British Birds*. In any case there must have been few observations of the occurrence in the last twenty years, and it seems worth recalling the incident of my childhood. It depends basically on observation and identification by my father, Walter Marchant, who was a competent field-naturalist and who died in 1951. My mother, still alive aged 88, and my sister, three years older than I, both confirm that it happened: so the evidence is perhaps better than what the soldier said. It happened when we were living at Weston-under-Lizard, near Shifnal, Shropshire, England, in the spring of 1918, when I was five and a half years old. A Pied Wagtail *Motacilla alba* had a nest in Ivy *Hedera helix* growing on a wall running at a right angle to the French windows of our dining room, and the nest was less than ten metres from the windows. I have a photograph of the place. One Sunday while we were having lunch and the windows were open, a Cuckoo *C. canorus* came and put its head into the cavity where the nest was. To my father's annoyance, at that point I dis-

turbed the bird, which flew away carrying one of the small young Wagtails in its bill and perched in a large Sycamore *Acer pseudoplatanus* about thirty metres away. There it knocked the young Wagtail against a branch repeatedly, but I cannot remember whether it swallowed it. Nor unfortunately do I know the exact contents of the Wagtail's nest before, or its history after, the Cuckoo's visit.

My father, who was not in touch with the ornithologists of the day and unaware of the few ornithological journals then published, wrote to the English weekly magazine *The Field* about the incident. His letter was not published, but it was certainly received because in *The Field* of 11 May 1918 the following answer to a correspondent appeared: 'W.M. (Shifnal). Such an incident as you describe has been previously reported, but we are inclined to think that the malefactor was a Sparrowhawk, and not a Cuckoo, the latter not being a rapacious bird, but preying solely upon insects. The general similarity of these two birds in size, colour and flight has led to their being often confounded.' My mother particularly remembers my father's scorn for the editor, who was apparently not aware that if the malefactor had been a Sparrowhawk *Accipiter nisus* the incident would have been equally remarkable; my father from his experience of Sparrowhawks for falconry and from general observation knew perfectly well that they never carry their prey in their bills. He minded much less the implication that he could not distinguish a hawk from a handsaw at ten metres.

Though undoubtedly rare, this destructiveness by cuckoos is puzzling. Baker, somewhat diffidently, suggested that the eating of whole clutches might have developed from an appetite for eggs that had been acquired in the ordinary course of removing and perhaps eating a fosterer's egg when the cuckoo laid its own. This is plausible, if unlikely, and at least would mean that the habit would be acquired by individuals only occasionally, would not spread by inheritance and probably would remain rare; but it would not even be plausible for the eating of young birds, if indeed cuckoos eat them, because there is no reason to suppose that cuckoos would ever have the chance to acquire the taste for such unlikely food in the normal course of their behaviour. Jourdain's explanation seems more acceptable, and at least for some gentes of European Cuckoo may not have the teleological implication that it at first seems to have.

Cuckoos arrive in Britain in the second half of April. Females settle into their territories and are ready to lay about the middle of May. Chance's (1922) well-studied Cuckoo apparently never laid before 13 May in three years. They lay about fifteen eggs each season at intervals of forty-eight hours, so that their laying period is rather well defined between the middle of May and the end of June. The gentes that parasitize Meadow Pipits *Anthus pratensis* and Reed Warblers *Acrocephalus scirpaceus* probably have little difficulty in finding enough nests at suitable stages for their own eggs because the hosts generally nest in contiguous territories in open places where several pairs can be watched at the same time from a vantage point. The Reed Warblers start to lay in late May. Most first clutches of Meadow Pipits are probably complete early in May or even at the end of April before female Cuckoos are ready to lay, but the species is so common on moors, heaths and grasslands that there would be no shortage of replacement and second clutches after mid-May. Chance (1922) had six to nine pairs of Meadow Pipits breeding in an area of about twelve hectares where he studied his Cuckoo in 1918–20. On the other hand the gentes that parasitize Pied Wagtails and Dunnocks may have problems. Both species of host tend to nest solitarily in gardens, hedges, woods, on buildings and so on, where a Cuckoo would probably have to watch each pair separately to find its nest. In an area of a normal English village like Weston covering, say, 200–300 hectares there would probably not be as many as nine pairs of Pied Wagtails, but there would be many more Dunnocks. Moreover, both these hosts start their first clutches in April, the Dunnock earlier than the Wagtail, so that generally one may expect that first clutches will not be available for the Cuckoo, which then must rely on replacements and second clutches. Even a bird presumably can find only a limited number of nests in a given time, and the Cuckoo has only its period of egg-production plus a few days before the start of laying in which to search. I suppose, therefore, that members of the gens parasitizing Pied Wagtails, and perhaps of that parasitizing Dunnocks, may sometimes be hard put to find enough suitable nests of the proper host. In these circumstances Cuckoos that find the right nest at the wrong stage for laying might destroy the contents in much the same way as other species have been known occasionally to destroy their own nests when some eggs have been removed or the nests tampered with in other ways. Cuckoos that, thus accidentally in the first place, provided themselves with replacement nests would perhaps be the more successful members of their gens and in this way the habit could become established.

It is hard to say whether Australian bronze cuckoos are affected by the same considerations because we do not know enough about them. Neither of the two species that I have found breeding, *Chrysococcyx basalis* and *C. lucidus*, seems to be so specific in its choice of fosterer as *Cuculus canorus*. *C. lucidus* is one of the few parasitic cuckoos that

seem to have made no attempt at egg-mimicry; its eggs are totally unlike those of the species of *Acanthiza* and *Gerygone* in whose nests I have found them. *C. basalis*, however, by the colour of its eggs is presumably adapted to parasitize species of *Malurus*, *Acanthiza* and *Gerygone*, and could have differentiated into gentes parasitizing particular species. The number of species acting as egg-hosts for both of these cuckoos is large (Friedmann 1968); but probably the cuckoos used many of these hosts because they could not find suitable nests of their customary fosterer, as Jensen and Jensen (1969) suggest for African species. Much of Baker's (1942) evidence showed that if a cuckoo deposits its egg in the nest of a species to which it is probably not adapted the nest is deserted or the cuckoo's egg is destroyed. In any case, even if many of the recorded hosts have been used only accidentally, these bronze cuckoos seem to have a wider choice of host than does a gens of *C. canorus*, unless of course they too have different gentes; in *C. basalis* these would be hard to recognize by egg-miniicry alone. Further, neither bronze cuckoo seems to have such a short period of laying as *C. canorus*. North (1912) took fresh eggs of *C. basalis* near Sydney from the middle of July to the end of January. Data in the RAOU Nest Record Scheme from ACT give laying dates of *basalis* between 4 October and 20 January. Eggs of *basalis* in the H. L. White collection in the National Museum of Victoria were taken in south-eastern Australia between 18 August and 23 March, and those of *lucidus* between 5 August and 28 December, though most eggs of both species were taken in September, October and November. As far as we can say at present, these and perhaps other Australian bronze cuckoos seem to have less need and urgency to find the right nest at the right time for their own eggs than have gentes of *C. canorus*, and therefore may be less likely to be driven to depredation, but clearly we need to know much more about them in every respect.

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