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AVIAN SYSTEMATICS IN AUSTRALIA

G.J. INGRAM

ABSTRACT

Recent taxonomic literature on Australian birds is examined. This is shown to be heavily influenced by evolutionary systematics. It is argued that, while this school of thought is historically necessary, it is outmoded, and, if taxonomy is ever to be regarded as a science, procedures that are explicit and less intuitive should be favoured and encouraged. Numerical taxonomy and/or phylogenetic systematics are more operational. The biological species concept and the taxo-evolutionary concept of subspecies are appraised with special reference to allopatric populations and selection being more important in speciation than gene flow. Difficulties with the biological species concept are illustrated while the taxo-evolutionary concept is shown to be inadequate. The tendency towards an ingrown taxonomy concentrating on subspecies is identified in the literature, even though the future of the taxonomic discipline is undoubtedly in the much neglected field of higher taxa classification.

On the basis of these discussions, the recent RAOU checklist and interim checklist are considered. It is predicted that these lists will become quickly outdated by changes that will inevitably occur under the impact of more explicit taxonomic practice and procedures as they replace the traditional methods of evolutionary systematics. The lists do not, and should not, serve taxonomists beyond being a summary of information and list of synonymy at one particular point in time. In conclusion, it is recommended that the unique opportunity, offered by the publication of an interim list of passerines, be used to democratize the taxonomic process for the subsequent official checklist.

INTRODUCTION

I regard systematics as a curious mixture of pure science, and an applied science - an applied science in that one constructs a classification and that this classification is primarily for public consumption. The latter position is easily defended in the bird world where classifications and nomenclature are used by a large number of bird-watchers, conservationists, environmental

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consultants, physiologists, ethologists and ecologists. The former group, of course, far outnumbers the rest. I will refer to all of these groups as the ornithological masses.

In this paper, I will pursue three major issues:-

1. systematics must have procedures that are explicit with the intuitive content reduced to as little as possible i.e. methodologies that allow someone other than the person making the decision to be involved in the decision making process.
2. is the biological species concept (BSC) performing its function in the classification of discontinuities of organisms in nature? i.e. are reproductive isolating mechanisms (RIM) not a cause of speciation but a common and not universal result of it?
3. where real new species are rarely found there is a tendency to concentrate on subspecies and neglect higher taxa classification.

SCHOOLS OF SYSTEMATIC THOUGHT

Recent Australian avian literature was examined with reference to four schools of systematic thought. These were:- the pragmatic school, the evolutionary school, the numerical school, and the phylogenetic school. The Australian literature can be placed in the first two categories - pragmatic and evolutionary systematics. The significance of this identification can not be realized except on a world scale. A short summary and comparison of the different schools of systematic thought are given below to orientate the reader on that level.

The pragmatic school is difficult to define, but it is perhaps typified by Blackwelder (1967) who regards taxonomy as something a taxonomist does in his day to day working and not something he thinks about. It could be said that it is the oldest school of taxonomic thought. There is also an expressed aversion to the BSC because of the practical problems of its application. Tucker (1949:163) summed this up succinctly:-

"It is (as it should be) a biological definition and not a rule-of-thumb device for determining whether any given form is a species or a subspecies."

The problems associated with this school of thought are its poorly defined or absent methodology for making classificatory decisions and its absence of definitions for its categories. This highly implicit content gives it no claim to repeatability or objectivity, but this is not to say its classifications are not useful. Its strengths have been in getting information out of museums and private collections into the literature, bringing to notice several practical problems of classification, and, of course, getting on with the job.

Evolutionary systematics is a serious attempt at defining terms and categories as well as giving an empirical criterion for deciding what a species is. It grew out of the application of

the synthetic theory of evolution to systematics (Huxley 1940; Mayr 1942, 1963). As stated by Simpson (1961) a particular classification should not contradict phylogeny; which means it should be phylogenetically neutral (Hull 1964). There is also the goal that classification should express the degree of divergence (Mayr 1963, 1969).

The greatest problem associated with evolutionary systematics is again the lack of methods to accomplish its aims (Ashlock 1974; Brundin 1966; Hennig 1966, 1975; Nelson 1971, 1972a, b; Rosen 1974a, b; Sneath & Sokal 1973; Sokal & Sneath 1963). As both Mayr (1969) and Simpson (1961) have said, the decisions for a particular classification are mostly a matter of art, judgement and intuition. Serventy (1950: 263) writing on taxonomic trends in Australia concurred:-

"However an academic training is not essential for effective work in taxonomy. It is a field of biological endeavour which demands a sagacity which formal training alone cannot implant."

Ashlock (1974: 97) wrote with optimism of the future of this school of thought:-

"The valid criticism that no good methodology exists for evolutionary systematics will become invalid as soon as such a method is published. I would suggest that it is inevitable that such methods will appear within the next few years."

It still hasn't.

The other rather terminal difficulty associated with both the pragmatic and evolutionary school is that its proponents have a habit of eventually dying. Thus we are left with losses of massive amounts of data and no insight into their methods.

It was primarily in reaction to the above problems that the numerical school arose in revolutionary zeal. It did not accept these qualifications of the evolutionary method. Sokal & Sneath (1963) called for objectivity and repeatability and had the catch cry of operationalism. They argued that a classification should be completely objective and all characters be of equal weighting. Their demand of repeatability they partially satisfied with computer programs.

Phylogenetic systematics had no impact in the English speaking world till the mid-sixties, when Hennig's (1966) pioneering work was published in English. Its persuasive central goal goes something like this:- if we accept that evolution has occurred and is still occurring then there can be one and only one phylogeny. The argument goes that this is hierachial and is easily expressed by the logical structure the Linnean system (Brundin 1966).

Thus phylogenetic systematics would be highly critical (of the evolutionary school) because of its strict adherence to phylogeny but it is also critical because of its belief in rigorous definitions and methodologies (Crowson 1970; Farris, Kluge & Echaradt 1970; Nelson 1971, 1972a, b, 1973; Rosen 1974a, b).

Summing up:- both numerical and phylogenetic systematics regard the evolutionary school as historically necessary but outmoded. They themselves offer a greater degree of operationalism. What seems to be the basic plea behind both these schools of thought is a demand for decreasing the intuitive content in classificatory decisions such that there is a methodology and that this is explicit, thus allowing someone other than the person proposing the classification to be involved in his/her decision making procedures.

SPECIES

Over the seven years that I have been familiar with Australian bird literature, I have been often mystified by how some bird species periodically disappear and reappear in the literature. I could not understand why an empirical theory such as the biological species concept could not settle the issues once and for all. I would like to discuss this problem.

Historically there was a need to know what characterized the discontinuous clusters of organisms in nature - those clusters called species that naturalists had common names for, and taxonomists were putting binomials on. The introduction of the BSC was one of the most significant things to come out of the synthetic theory of evolution (Mayr 1942). It is usually defined thus:-

"species represent groups of interbreeding natural populations, reproductively isolated from other such groups." (Bush 1975: 339).

Its logical implication for the study of speciation, is to make it, to a considerable extent, a study of the genetics and evolution of RIM (Bush 1975). The biological species was thus regarded as the basic evolutionary unit, and in classification the only objective category (Mayr 1963).

The most criticism that has been levelled at the BSC has come from numerical taxonomists (Jardine & Sibson 1971; Sneath & Sokal 1973; Sokal & Crovello 1970; Sokal & Sneath 1963). It is best summed up by Kluge (1974: 17) in his revision of the Australian legless lizards.

"The probability of potential or actual interbreeding and the nature of the reproductive isolating mechanisms between any pair of previously recognized yugopodid species are unknown, and consequently the BSC cannot be applied realistically within this family..."

The argument goes:- in most cases the type of data needed is not available, and those taxonomists who ostensibly use the BSC can only predict interbreeding or isolation phenetically, so why not use phenetics in the first place? (Sokal & Crovello 1970).

As Hull (1970) points out, this doesn't compromise the empirical nature of the BSC, because there is no reason why phenetics should not be used to predict the probability of interbreeding. He further notes that even if the BSC is seriously compromised in the future a phenetic concept based on overall similarity of all characters is not necessarily the answer; because, if for no reason

than that no description has been provided of it (Hull 1969, 1970). Instead there are literally an infinite number of phenetic units, all of which have an equal right on the principles of numerical taxonomy to be called species.

But even with this warning in mind, there are serious challenges to the BSC that do warrant a re-examination of its claim that it is the basic evolutionary unit, and that it is the only real category in classification.

Ehrlich & Raven (1969) presented evidence to show that selection is so overwhelmingly important in speciation, that the occasional effects of gene flow can safely be ignored in the general evolutionary picture. They questioned whether reproductive isolating mechanisms were a case of speciation or a common and not universal result of it.

If this contention is borne out by additional investigations then the role of the BSC will have been fatally undermined and the synthetic theory of evolution will have to be modified accordingly (Hull 1970).

Sokal & Crovello (1970: 149) concurring with Ehrlich & Raven observed that possibly the BSC is more of a burden than a help in understanding evolution and they concluded-

"the phenetic species as normally described and whose definition may be improved by numerical taxonomy is the appropriate concept to be associated with the taxonomic category 'species', while the local population may be the most useful unit for evolutionary study."

This leads back to my previous comment - the BSC arose out of a need to understand the discontinuities that were observable in nature and that were being classified. What if we ask the question, "is the BSC still serving this purpose?" There are several points to make -

1. It has resulted in the discovery of species that are morphologically indistinguishable. In the case of some corn weevils the RIM can be traced to a difference in the ratios of two chemical compounds that make up a pheromone (Bush 1975) - something that could be due to one or two gene differences - while many phenetic differences can be much more genetically complicated. An analogous situation in Australian birds is the crows and ravens (Rowley 1967, 1970).
2. Large phenetic differences can be maintained in the face of gene flow. For example, the pardalotes and the sittellas (Macdonald 1969a, b).
3. The BSC does not give an answer to what to do with distinct allopatric populations. Since there is no way to naturally gather the data needed to decide whether these populations are reproductively isolated from similar populations, it would have to be done experimentally. The first problem is to get them to interbreed - a difficult task in any

circumstance. Even if they do have fertile progeny, the results are not conclusive. As Mayr (1963: 112) has stated, "the mere possibility of hybridization in captivity proves nothing as far as species status is concerned."

As Crowson (1970) has noted, there is no reason why geographically separated populations have to evolve RIM anyway. Several examples will help illustrate the confusion that results when the BSC is applied to distinct allopatric populations. Macdonald (1973) treated the Plumed pigeon (*Petrophassa plumifera*), the Red-plumed pigeon (*P. ferruginea*), the White-quilled rock pigeon (*P. albipennis*), the Chestnut-quilled rock pigeon (*P. rufipennis*), Rainbow lorikeet (*Trichoglossus moluccanus*), Red-collared lorikeet (*T. rubritorquis*) and the Naretha parrot (*Peephotus narethae*) as distinct species while Condon (1975) treated them as subspecies. Both authors treated the Golden-shouldered parrot (*P. chrysopterygius*) and the Hooded parrot (*P. dissimilis*) as distinct species while Storr (1973) treated them as subspecies. Storr gave the Atherton ground thrush (*Zoothera cuneata*) specific status but Schodde (1975) did not, and in the case of the Brown-backed honeyeater (*Ramsayornis modestus*), Schodde treated it as a full species while Storr saw it as a subspecies. The list could go on and on.

Thus the BSC allows us to classify only one major type of discontinuity of organisms in nature. One could be cynical and say that classification is the practice of putting names on RIM.

SUBSPECIES

An interesting trend identifiable in Australia literature is the shift of emphasis to subspecies (Crome 1973; Ford 1970, 1974a, b, 1975; Parker 1972; Schodde & McKean 1973; Schodde & Mason 1976; Storr 1973). This tendency has been criticized in the recent literature (Crowson 1970; Selander 1971; Selander & Johnston 1967). Crowson (1970) has pointed out that the reaction of taxonomists specializing in groups in which real new species are rarely found has commonly been to pursue what has been called 'ingrown taxonomy' i.e. to concentrate their interests on infraspecific classification even though the future of their discipline is in the much neglected field of higher taxa classification.

Ford's (1974b) paper in *Emu* is the most recent attempt at hypothesizing an empirical concept of subspecies. This is the taxo-evolutionary concept, and according to it, a subspecies acquires its distinguishing taxonomic characters while isolated from other subspecies but may now be parapatric with them. Ford (p. 45) defines it as -

"a geographical aggregate of local populations that have undergone distinctive genetic differentiation in allopatry to a stage where it significantly differs taxonomically from other such subdivisions."

The two important criteria of the construct are "differentiation in allopatry" and "significantly differs taxonomically". Dealing with the latter first, how do we decide if the difference is significant taxonomically? We are not told, but it is this exact question that past subspecies concepts (e.g. the 75% rule

as in Mayr 1969) have tried to circumvent. Ford left the question begging, but decreased the instances of which it can be asked by imposing the allopatric condition.

The other objection to the taxo-evolutionary concept, however, is against the allopatric condition. It is apparently included to give the concept theoretical significance in terms of evolutionary theory, based on the correctness of the allopatric model of speciation as constructed by Mayr (1942, 1963) i.e. subspecies are probable species in the making if allopatric.

Mayr's allopatric model of speciation has now been rejected as the only model of evolving RIM (Bush 1975; Clarke 1966; Lewis 1966; Maynard-Smith 1966; Murray 1972; Slatkin 1974). If selection can be more important than gene flow, the small amounts of gene flow that do occur between distinctive parapatric populations can safely be ignored in the general evolutionary picture.

White *et al.* (1967) and White (1968) proposed the model of stasipatric speciation for the process that resulted in RIM being evolved under these conditions. Ford quoted Key's (1968) criticism of White's reasoning to reflect on the correctness of the stasipatric model. Bush (1975) analysed Key's objections in detail and rejected them. He has indicated the massive amount of literature showing that this type of speciation was quite common. Bush also discussed the evidence for sympatric speciation and concluded that this phenomenon is probably one of the most common of speciation processes.

From this evidence the taxo-evolutionary concept is apparently useless.

CONCLUSION

Hopefully now with the preceeding discussion I can home in on the recent RAOU checklist and interim checklist.

The RAOU checklist of non-passerines (Condon 1975) is a pragmatic taxonomic exercise. The ornithological masses have been left in a position where practically the only decision they can make is whether they except the list or not. The interim list of passerines (Schodde 1975) is a different kettle of fish. Schodde in the introduction has attempted to explain and discuss some of the decisions evident in the list and proposed for the official list. This situation where one can comment on a proposed checklist, and where one is invited to comment is certainly unique. The compilers should be thanked. Hopefully they have not become disillusioned with the reaction that is noticeable in the ornithological masses. This is a logical result of being allowed to comment and allowed to participate in the decision making process. It is also a result of the problems associated with evolutionary systematics. I will attempt to explain this last statement.

The principles of the synthetic theory of evolution and the new systematics permeated the ranks of the ornithological masses years ago. This, plus a naturalist's experience, has led to many more amateur critics of proposed classifications than there are professional taxonomists proposing them. These points, plus the high intuitive content and lack of rigorous methodology in evolutionary systematics (see also Disney 1976; McGill 1976),

give the ornithological masses every right in the world to comment, and even make decisions for a particular classification. Maybe the compilers should be acting as scrutineers searching for useful comments and classifications in addition to their own, i.e. democratize the taxonomic process for the subsequent checklist.

The question should also be asked, "whom do the Australian checklists serve?" Certainly not taxonomists beyond being a summary of information and list of synonymy at one particular point in time. From their point of view, some classifications must change with the input of new data (Cain 1958), and it is anyone's guess just how great the changes will be that will inevitably occur under the impact of more explicit taxonomic practice and procedures, as they replace the traditional methods of evolutionary systematics. The answer to the question of whom do checklists serve, must be those who have to use bird nomenclature for practical reasons and thus would like it to be stable. That is to realise there is a consumer demand for nomenclature and that there is probably an attractive commodity that pleases the masses the most.

Can I leave you with this question? Is this attractive commodity the general purpose classification (Berlin, Breedlove & Raven 1966) of the pheneticist, buffered from the inevitable changes that occur in the taxonomic world, and the biological species playing a minor part in them?

GLOSSARY

- Allopatry.** Of populations or species occupying mutually exclusive geographical areas. These areas are separated by geographical gaps.
- Binomial.** In the system of nomenclature first standardized by Linnaeus where the scientific name of an organism is designated by both the generic and specific name (e.g. *Platycercus adscitus* for the Pale-headed rosella). A trinomial is when the sub-species name is included.
- Biological species concept.** Defines species as groups of interbreeding natural populations reproductively isolated from other such groups.
- Classification.** The activity of recognizing groups and incorporating these into a rational hierarchial system in which each group has a unique place. See also the Linnean System.
- Empirical theory.** A theory which can be tested by proposing ways in which it can be refuted.
- Evolutionary Systematics.** This school of taxonomic thought grew out of the application of the synthetic theory of evolution to taxonomy. It applies the biological species concept, and believes that a classification should express phylogeny and the degree of divergence.

- Linnean system.** A system of categorical ranks for taxa where each category except the lowest includes one or more subordinate categories. The main ranks recognized are Phylum, Class, Order, Family, Genus, Species. (e.g. Pale-headed rosella. Phylum, Chordata; Class, Aves; Order, Psittaciformes; Family, Psittacidae; Genus, *Platyercus*; Species, *adscitus*).
- Morphology.** The form or structure.
- Nomenclature.** A system of names for denoting classificatory groups.
- Numerical systematics.** This school of taxonomic thought grew out of a reaction to the highly implicit methods of evolutionary systematics. They demanded objectivity and repeatability in their methods and specifically excluded evolutionary theory. They satisfied many of their demands with the use of computers.
- Operationalism.** This was a philosophical attempt to exclude theoretical terms from science by defining scientific data in terms of sense-data and operations. These days, terms like 'operational' usually mean that statements and hypotheses about nature can be tested by observation and experiment.
- Parapatry.** Of populations or species whose ranges are geographically contiguous and non-overlapping.
- Phenetics.** Separating out categories from the perceived variation in nature based strictly on the degree of overall similarity. The latter is usually calculated by the summation of similarities in many individual characters.
- Phylogenetic systematics.** This school of taxonomic thought believes in rigorously incorporating phylogeny in their classifications. They do this by strict definitions of their categories and methods. These days the distinction between phylogenetic and numerical systematics has become blurred where phylogenetic methods have been incorporated into numerical techniques.
- Phylogeny.** The study of the history of the lines of evolution in a group of organisms.
- Reproductive isolating mechanism (RIM).** Biological properties of individuals that prevent successful interbreeding with individuals from other populations (e.g. recognition of the song of the male by the female during breeding).
- Stasipatric speciation.** When two populations undergo speciation

- while in geographical contact (parapatric) and with some degree of gene flow occurring.
- Sympatry.** The occurrence of two or more populations of species in the same area.
- Synonymy.** A tabulated list of all the names a particular taxon has been known by.
- Systematics.** The body of theory underlying classification.
- Taxo-evolutionary concept of subspecies.** It defines a subspecies as a geographical aggregate of local populations that have undergone distinctive genetic differentiation in allopatry to a stage where it significantly differs taxonomically from other such subdivisions.
- Taxon (plural, taxa).** A taxonomic group that is sufficiently distinct to be distinguished by name, and ranked in a definite category (higher taxa are those categories above the species level, e.g. genus, family etc.). See also the Linnean System.
- Taxonomy.** See Systematics.

REFERENCES

- Ashlock, P.D. 1974. The uses of cladistics. *Ann. Rev. Ecol. Syst.* 5:81-99.
- Berlin, B., Breedlove, D.E. and Raven, D.H. 1966. Folk taxonomies and biological classification. *Science* 154:273-5.
- Blackwelder, R.E. 1967. *Taxonomy*. Wiley: New York.
- Brundin, L. 1966. Transantarctic relationships and their significance, as evidenced by chironomid midges. *K. svenska Vetensk - Akad. Handl.*, ser. 4, vol. 11, no. 1.
- Bush, G.L. 1975. Modes of animal speciation. *Ann. Rev. Ecol. Syst.* 6: 339-64.
- Cain, A.J. 1958. Logic and memory in Linnaeus's system of taxonomy. *Proc. Linn. Soc. Lond.* 169:144-63.
- Clark, B. 1966. The evolution of morpho-ratio clines. *Am. Nat.* 100:389-462.
- Condon, H.J. 1975. Checklist of the birds of Australia. Part 1. non-passerines. RAOU: Melbourne.
- Crome, F.H.J. 1973. The relationships of the Helmeted honeyeater and Yellow-tufted honeyeaters. *Emu.* 73:12-8.
- Crowson, R.A. 1970. *Classification and biology*. Heinemann Educational Books: London.
- Disney, H.J. de S. 1976. Interim list of Australian songbirds, by R. Schodde. RAOU Newsletter. 27:3.
- Ehrlich, P. and Raven, P.H. 1969. Differentiation of populations. *Science.* 165:1228-32.
- Farris, J.S., Kluge, A.G. and Echaradt, M.J. 1970. A numerical approach to phylogenetic systematics. *Syst. Zool.* 19:172-89.
- Ford, J. 1970. Variation in the *Sericornis frontalis maculatus* sub-species group. *Emu.* 70:168-72.
- Ford, J. 1974a. Taxonomic significance of some hybrid and aberrant-plumaged quail-thrushes. *Emu.* 74:80-90.
- Ford, J. 1974b. Concepts of subspecies and hybrid zones, and their application in Australian ornithology. *Emu.* 74:113-23.

- Ford, J. 1975. Systematics and hybridization of Figbirds *Sphecothebes*. Emu 75:163-71.
- Hennig, W. 1966. Phylogenetic systematics. University of Illinois Press: Urbana.
- Hennig, W. 1975. "Cladistic analysis or cladistic classification?": a reply to Ernst Mayr. Syst. Zool. 24:244-56.
- Hull, D.L. 1964. Consistency and monophyly. Syst. Zool. 13:1-11.
- Hull, D.L. 1969. The natural system and the species problem. In C.G. Sibly (ed.) Systematic biology Publ. 1962 Nat. Acad. Sci.
- Hull, D.L. 1970. Contemporary systematic philosophies. Ann. Rev. Ecol. Syst. 1:19-54.
- Huxley, J. (ed.) 1940. The new systematics. Oxford University Press: London.
- Jardine, N. and Sibson, R. 1971. Mathematical taxonomy. Wiley: London.
- Key, K.H.L. 1968. The concept of stasipatric speciation. Syst. Zool. 17: 14-22.
- Kluge, A.G. 1974. A taxonomic revision of the lizard family Pygopodidae. Misc. Publ. Mus. Zool. Uni. Mich. 147.
- Lewis, H. 1966. Speciation in flowering plants. Science. 152:167-72.
- McGill, A.R. 1976. Interim list of Australian Songbirds-Passerines, by R. Schodde. Aust. Bird Bander 14: 80-2.
- Macdonald, J.D. 1969a. Hybridization in *Pardalotus*. Emu 69:41-4.
- Macdonald, J.D. 1969b. Notes on the taxonomy of *Neositta*. Emu 69:169-74.
- Macdonald, J.D. 1973. Birds of Australia. Reed: Sydney.
- Mayr, E. 1942. Systematics and the origin of species. Columbia University Press: New York.
- Mayr, E. 1963. Animal species and evolution. Harvard University Press: Cambridge.
- Mayr, E. 1969. Principles of systematic zoology. McGraw-Hill: New York.
- Maynard-Smith, J. 1966. Sympatric speciation. Am. Nat. 100:637-50.
- Murray, J. 1972. Genetic diversity and natural selection. Hafner: New York.
- Nelson, G.J. 1971. "Cladism" as a philosophy of classification. Syst. Zool. 20:373-6.
- Nelson, G.J. 1972a. Phylogenetic relationships and classification. Syst. Zool. 21:227-31.
- Nelson, G.J. 1972b. Comments on Hennig's "Phylogenetic Systematics" and its influence on ichthyology. Syst. Zool. 21:364-74.
- Nelson, G.J. 1973. Classification as an expression of phylogenetic relationships. Syst. Zool. 22:244-59.
- Parker, S.A. 1972. Remarks on distribution and taxonomy of the grass wrens, *Amytornis textilis*, *modestus* and *purnelli*. Emu 72:157-66.
- Rosen, D.E. 1974a. Review: Classification and biology by R.A. Crowson. Syst. Zool. 23:141-3.
- Rosen, D.E. 1974b. Cladism or gradism?: a reply to Ernst Mayr. Syst. Zool. 23:446-51.
- Rowley, I. 1967. A fourth species of Australian corvid. Emu 66:191-210.
- Rowley, I. 1970. The genus *Corvus* (Aves: Corvidae) in Australia. CSIRO Wildl. Res. 15:27-71.
- Schodde, R. 1975. Interim list of Australian songbirds. RAOU: Melbourne.
- Schodde, R. and McKean, J.L. 1973. Distribution, taxonomy and evolution of the gardener bowerbirds *Amblyornis* spp. in eastern New Guinea with descriptions of a new subspecies. Emu 73:51-60.
- Schodde, R. and Mason, I.J. 1976. A new subspecies of *Colluricincla megarhyncha*, Quoy and Gaimard, from Northern Territory. Emu 76:109-14.
- Serventy, D.L. 1950. Taxonomic trends in Australian ornithology - with special reference to the Work of Gregory Mathews. Emu 49:257-67.
- Selander, R.K. 1971. Systematics and speciation in birds. In D.S. Farmer and J.R. King (eds.) Avian biology vol. 1. Academic Press: New York.
- Selander, R.K. and Johnston, R.F. 1967. Evolution in the House Sparrow: intrapopulation variation in North America. Condor 69:217-258.
- Simpson, G.G. 1961. Principles of animal taxonomy. Columbia University Press: London.

- Sneath, P.H.A. and Sokal, R.R. 1973. Numerical taxonomy. W.H. Freeman and Co.: San Francisco.
- Slatkin, M. 1974. Cascading speciation. Nature. 252:701-2.
- Sokal, R.R. and Crovello, T.J. 1970. The biological species concept: a critical evaluation. Amer. Nat. 104:127-53.
- Sokal, R.R. and Sneath, P.H.A. 1963. Principles of numerical taxonomy. W.H. Freeman and Co.: San Francisco.
- Storr, G.M. 1973. List of Queensland birds. Spec. Publs. West Aust. Mus. 5:1-177.
- Tucker, B.W. 1949. Species and subspecies: a review for the general ornithologist. Br. Birds 42:129-34, 161-74, 193-205.
- White, M.J.D. 1968. Models of speciation. Science. 159-1065-70.
- White, M.J.D., Blackith, R.E., Blackith, R.M. and Cheney, J. 1967. Cytogenetics of the *Viatica* group of morabine grasshoppers. 1. The "coastal" species. Aust. J. Zool. 15:262-302.

MR. G.J. INGRAM, Queensland Museum, Brisbane, Queensland, 4000.

FEEDING BEHAVIOUR OF SCALY-BREADED LORIKEETS

TIM HAMLEY

ABSTRACT

A study of feeding methods and related behaviour in the Scaly-breasted lorikeet, *Trichoglossus chlorolepidotus* provides evidence of territorial behaviour in a normally flocking bird. The relationship of this behaviour with the resource in use at the time is suggested, and questions of permanence of this behaviour are briefly discussed.

INTRODUCTION

The Scaly-breasted lorikeet, *Trichoglossus chlorolepidotus*, has a brush tongue and is known to feed on pollen and nectar as well as flowers, fruits, berries and seeds (Forshaw 1973). This species also feeds on ripening sorghum crops (Lavery 1970). Apart from observations on the feeding of the Purple-crowned lorikeet, (*Glossopsitta porphyrocephala*) by Churchill and Christensen (1970) there is little information available on the feeding behaviour of lorikeets. Scaly-breasted lorikeets roosted near my home during the flowering period of several umbrella trees (*Brassia actinophylla*) and gave me an opportunity to observe their feeding and social behaviour.

METHODS

Observations were made from the verandah of my home in Toowong (Brisbane) for eight days from 5 March 1977, then four more days of observation were made from the roof to give an unobscured view of the lorikeets. Observations from the roof were carried out from 1200 to about 1800 hours, at which time roosting had generally taken place. I used a pair of 7X35 wide angle binoculars.

The birds were watched closely on one occasion while I climbed a ladder to the roof. My activity seemed to have no effect on them even though I was only about 3 m away. These lorikeets may have been accustomed to human presence.

During the observation times the weather was fine and warm (27°C - 30°C) and only occasionally cloudy.

RESULTS

Feeding behaviour

Two different feeding methods were observed. The first consisted of a slower, more deliberate action, where the beak was held wide open and the tongue, pointed, was thrust deep into the flower of the umbrella tree. This method of feeding was employed almost exclusively during the first few days of observation and small grains, presumed to be pollen, could be seen on the hairs of the lorikeet's tongue. The average duration of this method was between one and two seconds per flower. The second method of feeding was much more rapid, (covering two to three flowers per second) and consisted of placing the upper

surface of the tongue in a bent position on the flower. This method was most common after the fourth day and was coincident with an increase in the number of bees present around the flower stems, and the presence of what was assumed to be nectar clearly visible on the surface of the flower. At this time the birds appeared to be feeding mainly on nectar. The relatively large amount of nectar exuded by the flowers presumably strongly attracted lorikeets to umbrella trees when they are in bloom, and contributes to the social organisation that is discussed below.

Intra Specific Agonistic Behaviour

I have classified agonistic behaviour under the names "squabbling" and "fighting", more as a result of differences in the outcomes than of the behaviour itself. "Fighting behaviour" consists of one bird chasing another and directing pecks at it. Often the wings remained folded and a slight red patch of plumage on the lesser wing coverts appeared to be more evident during agonistic behaviour. This behaviour appears to be highly ritualized, and in more than 20 fights I did not observe one where attempted pecks actually made contact. Fighting behaviour was one-sided and ended in one of the birds involved being chased away from the observation area. "Squabbling", although seemingly containing the same movements, differed in that both birds pecked at each other and although they might move from branch to branch, or even tree to tree, the encounter did not end with one of the birds leaving the area. Both types of agonistic behaviour were accompanied by a call in which the elements seemed more rapid and of a higher pitch than normal.

Inter Specific Agonistic Behaviour

The only interspecific agonistic behaviour observed occurred with Noisy miners, (*Manorina melanocephala*). Here again, squabbling and fighting can be differentiated on the part of the lorikeet, the difference being that the miners never retaliated, but either moved away or left the area completely.

Spacing and Social Organization

The spacing patterns and social behaviour of the lorikeets can best be described by considering a typical day. Generally, the lorikeets left their roost in a eucalypt tree approximately 150 m from the observation area by about 0600 hours. Many dispersed immediately but as many as 20 were observed spread over the three umbrella trees with the number gradually decreasing until none were left at 0700 hours. The observation area then remained free from lorikeets until between 1200 and 1400 hours. Usually at about 1200 hours two lorikeets would be heard calling in the observation area. Due to the lack of sexual dimorphism I was unable to sex the birds, but I shall refer to them as a pair.

The pair divided their time between the three umbrella trees in the area and a large South African tulip tree (*Spathodea campanulata*). There seemed to be discernible pattern or preference as to which umbrella tree the pair used. The pair spent more than 90% of the feeding time together on the same stand of flowers. Time in

an umbrella tree varied between 1 and 6 minutes, with more than 70% of this time being spent on feeding. These feeding intervals were interspersed with sitting and preening in the dense canopy of the South African tulip tree. These periods varied in length, on one occasion lasting for 35 minutes.

From 1500 to 1600 hours fighting behaviour was strongly evident whenever either a conspecific or a Noisy miner entered the area, even to the extent of intercepting and chasing the "intruder" while still on the wing. After all such interactions, the original pair remained in the area; the intruder or intruders having been chased away.

Between 1600 and 1700 hours the frequency of fighting behaviour decreased and squabbling behaviour increased with the result that there might be 3 pairs of lorikeets in the observation area although usually feeding on different umbrella trees. During this time the amount of time spent in the umbrella trees feeding increased until the birds rarely landed in the South African tulip tree. The presence of Noisy miners generally increased at this time, and although they were still met with fighting behaviour most of the time, the lorikeets often exhibited squabbling behaviour towards them.

After 1700 hours and until the birds left the umbrella trees to roost, both the number of birds and the intensity of feeding increased markedly. Squabbling behaviour, although common, became less intense and rarely involved greater movement than from one branch to another. If Noisy miners were present, they were often seemingly ignored and fighting behaviour was rarely directed towards them. At this time, there could be up to 20 birds feeding in the umbrella trees and calls from the roost area indicated that many more birds were gathering there. Observation of the roost area showed much squabbling which deminished rapidly at sun set.

DISCUSSION

The Scaly-breasted lorikeets I observed displayed territorial behaviour. No differentiation of individual birds was possible, so I do not know whether the same pair was present each day or whether different pairs occupied it on different days. There is also doubt about whether the same flock was in the roost area consistently. However, based on the observations of flock size and consistency of behaviour, this is quite likely. If this is the case, we are left with what appears to be transient territoriality waning through the afternoon. There is also a commensurate waning in agonistic defence of this area. The fact that a similar organization does not appear to exist in other feeding situations would seem to suggest that perhaps this territorial aspect of the lorikeets behaviour is related to the extent and availability of the food resource of the umbrella trees. For example, thousands of lorikeets can be seen feeding at Currumbin at "feeding time" with very little fighting behaviour evident. Similarly near the observation area, flocks feed in various eucalypt trees; and it appears that the pairs of individuals still tend to feed close together.

It would be useful to mark birds and see if they take up a similar territorial attitude in other areas with respect to this roost area, and if so, if there is any observable hierarchical structure in the way territories are allocated.

The use of a red patch of plumage on the lesser wing coverts during agonistic behaviour seems to be an extension of the red plumage found underneath the wing and could function in a similar way to the yellow eye patch of the Noisy miners (D. Dow, pers comm).

The observed waning of agonistic behaviour as roosting time approached can be seen as a necessary prerequisite if the birds are going to be able to flock together at night. The parallel waning of agonistic behaviour towards Noisy miners produces some unanswered questions.

REFERENCES

- Churchill, D.M. and Christensen, P., 1970. Observations on pollen harvesting by Brush-tongued lorikeets. *Aust. J. Zool.* 18: 427-437.
- Forshaw, J.M. 1973. *Parrots of the world.* Lansdowne, Melbourne.
- Lavery, N.J. 1970. Sorghum damage by lorikeets. *Qld. Agric. J.* 96: 785-786.

MR. T. HAMLEY, 2/16 Norwood Street, Toowoong, Queensland, 4066.

ROOSTING OF TREE-CREEPERS

RICHARD NOSKE

INTRODUCTION

There is very little published information on the roosting habits of Australian birds, and the tree-creepers (Climacteridae) are no exception. Batey (1907) disturbed a Brown tree-creeper (*Climacteris picummus*) from its roost-site in the burnt-out butt of a live tree. He suggested that the bird had clung to the substrate in an upright position. More recently, Mackness (1976) discovered a White-throated tree-creeper (*C. leucophaea*) sleeping in a horizontal position (presumably upside-down), clinging to the roof of a cave. He regarded this choice of roost-site as fairly atypical. Over the past eighteen months, I have been comparing the behaviour and ecology of three species of tree-creeper the Brown, White-throated, and Red-browed (*C. erythrops*). The study has been conducted mainly at Wollomombi Falls Reserve, an area of tall eucalypt woodland 40 km east of Armidale, N.S.W. Brown tree-creepers were also observed in a paddock at Swan Vale (36 km east of Inverell, N.S.W.), and in gidgee (*Acacia cambagei*) woodland, 53 km west of Cunnamulla, Qld.

Each day in the field I followed birds at dusk in an attempt to find their roosts. Not all of these attempts have been successful, as the birds are often still active when it is dark. Frequently, after perching motionless on a substrate for several minutes, they would fly swiftly and suddenly to a distant roost.

RESULTS

Data on eighteen tree-creeper roosts are summarized in Table 1. Visits to roost-sites, subsequent to their discovery, proved that birds do not always occupy the same site. In addition, once a bird was flushed from its roost, it would rarely return that night, indicating a second site was being used. One male White-throated tree-creeper used a third roosting site. A female White-throated tree-creeper slept in the corner of two outside walls of my weatherboard home, 3 km north of Armidale, on 1 April 1977. It was resting vertically on the ledge of a plank under the roof gutter, about 4 m from the ground. This site has not been used again.

At Wollomombi, one fairly exposed roost-trees (Table 1:2) has been used by three different birds. On 20 September 1976 I found an adult female White-throated tree-creeper sleeping 2.5 m from the ground on the northern edge of a vertical burnt-out tree-stump, about 6 m high. I did not revisit the site until 17 February 1977, when an immature female was found at 4.5 m, on the southern edge. This bird later moved to the lower position used by the former bird. In seventeen revisits to the stump, I found it unoccupied only twice. On another occasion a Brown tree-creeper was found roosting in a crevice, (Table 1:7) but a young White-throated tree-creeper occupied the site the

TABLE 1

Details of roost-sites of tree-creepers, at Wollomombi Falls (WF), Swan Vale (SV), and Cunnamulla (CN). Some observations refer to alternative roost-sites of the same individual. Trees were classified as predominately dead or alive, sites as either enclosed hollows (H), or partly or wholly exposed crevices (C). All dead trees were eucalypts, except 13 and 14 which refer to *Acacia cambagei*. Live trees 1 and 5 were *Eucalyptus blakelyi*, and 15 was *E. conica*.

* Not occupied on subsequent visits. 1 Bird found dead.
2 Fallen dead branch suspended in a live tree.

No.	Sex	Tree	Height (m)			Locality	Date of discovery
			Tree	Roost	Site		
White-throated tree-creeper							
1	F	live	18	4	C	WF	10 April- 30 July 76* 1
2	F	dead	6	2.5,4.5	C	WF	20 Sept 76
2	F	dead	6	2.5,4.5	C	WF	20 Sept 76
3	F	dead ²	12	5.5	C	WF	24 Feb 77*
4	F	dead	6	3.5	C	WF	13 May 77
5	M	(live	14	4	C	WF	6 July 77
		(live	11	6	H	WF	7 July 77
Brown tree-creeper							
6	?	dead	12	5.5	H	WF	31 Mar 76*
7	?	dead	6	1	C	WF	24 Feb 77*
8	F	dead	10	7.5	H	WF	12 May 77
9	?	dead	7	5.5	H	SV	22 May 77
10	?	(dead	4	?	H	SV	22 May 77
		(dead	6	3.5	H	SV	23 May 77
11	?	dead	5	4.5	H	SV	25 June 77*
12	?	(dead	7	4.5	H	SV	27 June 77
		(dead	6	5	H	SV	27 June 77
13	?	dead	6	1.5	C	CN	6 June 77
14	F	dead	5	3	C	CN	9 June 77
Red-browed tree-creeper							
15	M	live	15	7	H	WF	27 Feb 77

following night.

All birds appeared to sleep in a more or less vertical position, although in many cases, the nature of the site prevented me from seeing the bird's precise orientation. The White-throated tree-creeper sleeps with its head twisted backward, and hidden in the ruffled feathers of the back. A similar sleeping posture has been described for one species of South American woodcreeper (Dendrocolaptidae) (Oniki 1970); a family which resembles Climacteridae in many aspects of behaviour.

As White-throated tree-creepers prefer to forage singly (Noske 1976), it is not surprising that the sexes sleep separately. On the other hand, many gregarious species roost communally; some Australian examples being wood-swallows and miners (Rowley 1974), sittellas (McGill 1967), and babblers (King 1975). It is somewhat surprising then, that Brown tree-creepers, which are frequently encountered in groups of three to six individuals, also choose to sleep singly. In the case of one pair at Wollomombi, the two roost-trees were separated by a distance of about 300 m, yet during the day the two birds rarely foraged more than 50 m apart. Similarly, it is interesting to note that at least one Red-browed tree-creeper, a species which usually forages in groups of three or four birds (Noske 1976), roosted alone.

In summary, tree-creepers appear to roost mostly in either hollow trunks and limbs, or cavities formed by peeling or protruding bark on trees. Occasionally, more exposed situations are sought. The apparent preference shown by Brown tree-creepers for dead substrates, reflects the species' fondness for habitats where dead timber predominates. All three species apparently roost hanging in a vertical position, and sleep singly (although there is little information on the Red-browed tree-creeper at this stage). Most birds use more than one roost-site.

REFERENCES

- Batey, I. 1907. On fifteen thousand acres: its birdlife sixty years ago. *Emu* 7: 8.
- King, B. 1975. Communal nesting by the Grey-crowned babbler *Pomatostomus temporalis*. *Emu* 74: 310.
- McGill, A.R. 1967. Roosting of the Black-capped sittella. *Emu* 66: 388.
- Mackness, B. 1976. White-throated treecreeper roosting in a cave. *Sunbird* 7: 48.
- Noske, R. 1976. Niche differentiation and behaviour in 3 sympatric tree-creepers. B.Sc. Honours Thesis. University of New England, Armidale, N.S.W.
- Oniki, Y. 1970. Roosting behaviour of three species of woodcreepers (Dendrocolaptidae) in Brazil. *Condor* 72: 233.
- Rowley, I. 1974. *Bird life*. Collins: Sydney.

A SIGHTING OF THE FIG PARROT IN SOUTH-EAST QUEENSLAND

BRUCE CORFE

In the mid-morning of 14 December 1976, I visited Komarun Lookout in Lamington National Park, approximately 12 km from O'Reilly's guest house. The lookout is in a cleared and elevated area which gives a view over the surrounding rainforest. Conditions were cloudy and bright and I was using 10x50 binoculars.

Some loud calls were heard coming from a large solitary fig tree (*Ficus* sp.) in the centre of the clearing. On moving closer to the tree I disturbed two small parrots which flew rapidly to an emergent hoop pine (*Araucaria cunninghamii*) in the nearby rainforest. I was able to view the birds from approximately 10 m and immediately recognised them as Fig parrots. For the previous six months, I had resided in Cairns and had frequently observed the Fig parrot (*Opsittia diophthalma*) at close range.

The small size, and dumpy, almost tail-less silhouette, distinctive "yyit-yyit" call, and rapid, direct flight were all diagnostic of Fig parrots. I was able to obtain a further brief view of the head of one bird from about 40 m, before both flew off calling loudly. The colouring of the head was bright green with a brilliant blue forehead and a large bright orange cheek patch, extending well behind the eye. Although the sightings were only brief, I felt quite certain that the birds were Fig parrots, (*Opsittia diophthalma*).

Because there is a paucity of records and general information for this species in south-east Queensland, this easily accessible location could be worth visiting for future observations on these birds.

MR. B. CORFE, 6/71 Cremorne Road, Cremorne, N.S.W. 2090.
Present address: 74 Pennygate Road, Spalding, Lincolnshire, England.
