

term applied to many of the members of a very large bird family, Ploceidae, which may even be of polyphyletic origin. No more precise name than "weaver finch" is given, either in English or in Latin, in Segal's paper. It might be argued that reference can be made to earlier papers cited; this is true, although it should not be necessary. Checking one of Segal's cited references, that to Witschi's paper in *The Wilson Bulletin*, I find no less than four species of the family Ploceidae mentioned: the English sparrow (*Passer domesticus*), the paradise whydah (*Steganura paradisaea*), the red-billed weaver (*Quelea quelea*), and the orange weaver (*Euplectes orix*). Scientific names are given by Witschi for the last two only. The brief description given by Segal of his experimental birds ("the male bird assumes a bright yellow and black plumage . . .") fits none of the species mentioned by Witschi; the *Euplectes*, which comes closest, is described by Witschi as "black and orange red."

Segal's experimental bird is not identifiable from his description as published. In any case, it is almost certainly a member of a genus of the sort mentioned in the quotation from Mayr, Linsley, and Usinger, "with two, three, or more very similar species"—there are many such genera in the Ploceidae. The normal cycle of molts and plumages in wild birds may vary, even among closely related species, to the extent that it should be considered of fundamental importance to establish the identity of the experimental birds.

One further point. Segal mentions his "weaver finch" as being "readily available in the United States because large numbers are imported for sale to bird fanciers." Having had experience in recent years for the identification for zoos and aviaries of birds sold by importers, I would consider such birds to be extremely unreliable as standardized experimental animals. Birds of several genera are sold under the general term "weaver birds." A dealer may supply two shipments of what he honestly believes to be the same species to the experimenter; unfortunately, neither may be capable of recognizing the differences between closely-related species in a genus such as *Ploceus*, one of the largest genera of weavers; the consecutive shipments may well involve more than one species. Even within a single species the possibility arises of geographic variation in physiological characteristics, a well-known phenomenon in certain groups of birds and other animals. Very few importers of cage birds have any notion as to the precise area of Africa from which their weaver birds may be derived.

In the absence of an established breeding stock of experimental animals such as is now available for the golden ham-

ster or the budgerigar, it would appear to be of critical importance for nontaxonomic zoologists to secure authoritative taxonomic identifications of their subjects before publication of their results.

KENNETH C. PARKES

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I shall humbly agree that, in my description of "the weaver finch," it might have been pointed out more clearly that the so-called "yellow weaver" or "Napoleon weaver" (*Euplectes afra*) is the species that serves in the establishment of the standard of the discussed hormonal reaction.

In all other respects the criticisms of Parkes seem misdirected. Before writing a letter to the editor, he might have consulted not only the oldest but also the most recent summary by Witschi (reference 3 of my report). He would have found color photographs, of various species and reactions, adequate to satisfy anyone more particularly interested in the taxonomic and technical aspects of the feather reaction. He might then, also, have followed a reference to a readily available paper [E. Witschi, *Endocrinology* 27, 437 (1940)] in which *weaver finch test* and *weaver finch unit* are introduced as technical terms. In accordance with good usage, these terms are clearly defined, and the reactions of *E. afra* are compared with those of several other species.

It should be evident to all readers that my short report in *Science* of 13 Dec. 1957 was meant to be a contribution to endocrine physiology. Taxonomic considerations do not necessarily enter into the discussion of assay materials such as I have presented. All data refer to a single species. Moreover, each individual bird serves as its own control. The positive or negative reaction of a bar across a single regenerated feather is always compared with the tip and the base which grew before and after injected hormone substances were present in the systemic circulation. With this I do not deny that taxonomic and physiologic variation in plumage characters of finches is a subject of interest. Publication of a large assemblage of data that I have collected in Iowa City over more than twenty-five years is, in fact, in preparation.

Regarding the quotation from Mayr, Linsley, and Usinger, there can be little doubt but that these three experts mean exactly what they say: namely, that the experimental biologists have learned to appreciate physiologic traits and differences. If "most taxonomists" are wise to it as well, for whom, Dr. Parkes, was their message intended?

SHELDON J. SEGAL

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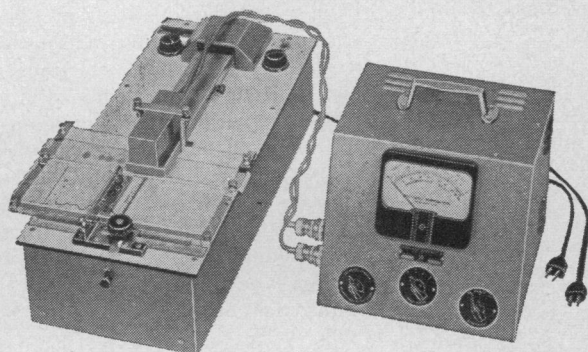
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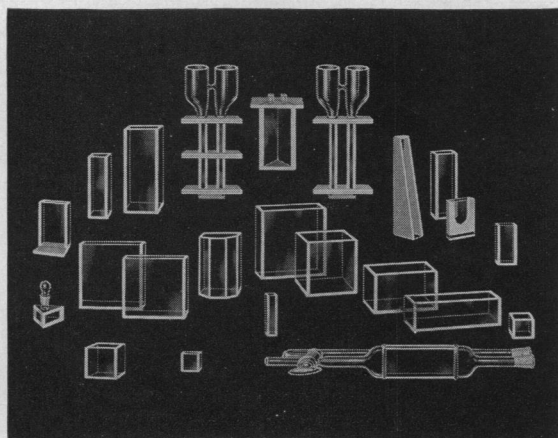
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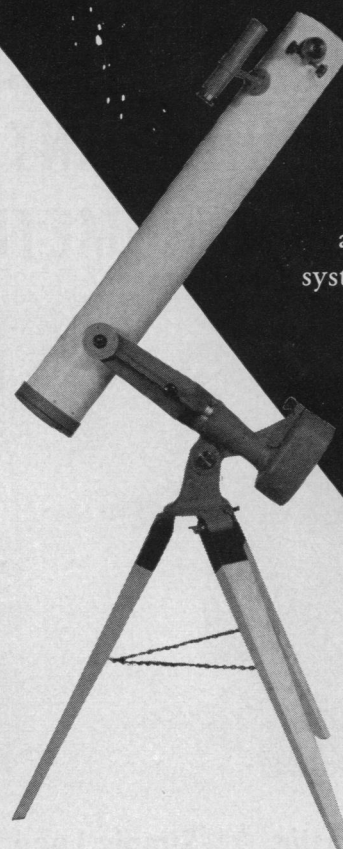
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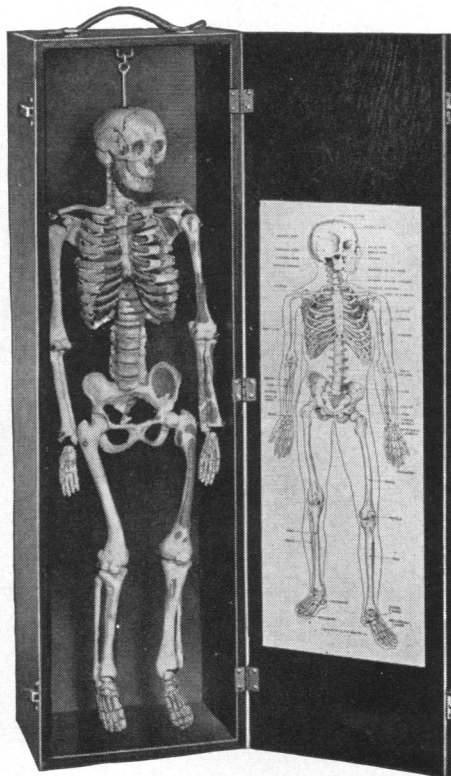
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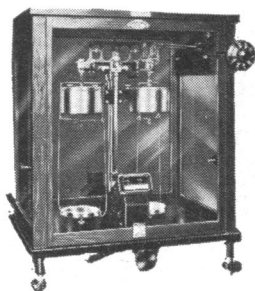
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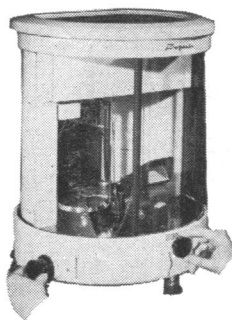
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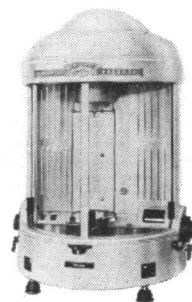
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