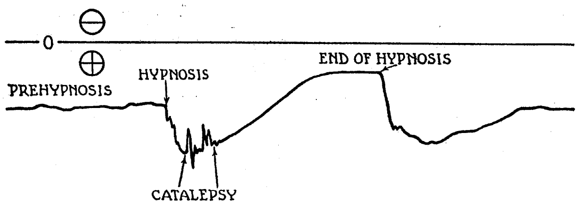

FIG. 1. Schematic d-c record of hypnosis.

behavior, utilizing the technique developed by Milton H. Erickson; e.g., as his hand rose, a subject would become sleepier until, finally, when it touched his face, he would close his eyes and sleep, signifying he was ready by returning the hand to his lap. Catalepsy, when used to induce hypnosis, sometimes produced marked emf changes (Fig. 1). When this occurred during the trance, or when the subject voluntarily raised an arm, minimal changes were recorded.

Depth of hypnosis, as measured electrometrically, does not seem to be correlated with ability to develop amnesia or other phenomena often necessary for a good therapeutic trance. Any disturbance of the hypnotic state could be detected immediately by changes in voltage and in configuration of the tracing. It is thus possible to measure objectively changes in depth of hypnosis.

A complete report of methodology and results will be published elsewhere.

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Comments and Commuications

Are Nonflying Wings Functionless?

In a recent able review of evidence that the so-called ratite birds had flying ancestors, Steiner (*Rev. Suisse de Zool.*, 1949, 56, 364) concluded with the remark (in German) that "the wing, made useless by the transition to a cursorial habitus, was reduced as a direct result of its high specialization, because it was no longer capable of taking on a new function (in *Rhea* and *Struthio* at most still used for display of the decorative wing feathers!)."

There is nothing new in the observation, but it is worth while to point out that even so highly specialized a structure as the bird wing is quite capable of taking on functions distinct from, although usually related to, that of aerial flight. The penguin wing is certainly a highly functional organ. Loss of ancestral power of flight in this case clearly involved change, not loss, of wing function (Simpson, G. G. *Bull. Amer. Mus. Nat. Hist.*, 1946, **87**, 1). The same is probably true of some or all of the distinctly cursorial flightless birds.

The rhea is among such truly cursorial birds, a running herbivore (and occasional insectivore) adapted to treeless plains where large predatory carnivores are present, and this also applies to the African ostriches. Long personal observation of wild Patagonian rheas in their natural habitat did not disclose any definite display of the wing feathers, although this may occur. Repeated observations did confirm the fact that the wings are not wholly passive and that they probably do serve a useful function. When running, the rheas keep the wings spread and seem definitely to balance themselves in this way, especially in the rather awkward maneuver of turning rapidly.

This true function for the reduced wings may well explain the rule that the wings are usually merely reduced and not lost altogether in cursorial birds. It is true that the wings were completely functionless and were lost externally in the moas of New Zealand and in *Aepyornis* and its allies in Madagascar. These birds, however, were probably not truly cursorial types, as suggested by their heavy, evidently slow proportions, their herbivorous habits, and their practical immunity from predation except by man, whose persecution was too late and too brief to induce cursorial adaptation. Among other extinct birds, the phororhacoids in South America, *Diatryma* in North America, and *Gastornis* in Europe all had cursorial proportions. All were associated with mammalian predators, and they were themselves probably running predators. All retained reduced but probably functional wings more or less as in the rheas.

It is a habit of thought to consider that changes associated with reduction in size and loss of a function, as in nonflying wings, are *ipso facto* degenerative. The modifications of the penguin wing can, however, be viewed as progressive specializations for their new function. May this not also be true of the rhea wing and analogous cases?

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The *Systema Naturae* of the Twentieth Century

If the International Zoological Congress, to meet in Copenhagen in 1953, decides to allow another interval of five years before its next meeting, the latter will fall in 1958. It will be 200 years after the issue of Linnaeus' tenth edition of his *Systema Naturae*, an anniversary which must not pass without a serious attempt being made to bring the ideas of this great naturalist a new step forward.

How it could be done is indicated by Karl P. Schmidt (*Science,* 1950, **111**, 235), who writes that we could create a *Systema Naturae* of the twentieth century by having the *Official List of Generic Names* brought up to date and efficiently extended.

I feel that Linnaeus might have signed the following words of my regretted friend Cyril Crossland, when he wrote in a letter printed for circulation among systematists during the war (1940):

> Systematists do not work for themselves alone, indeed if it were so their work would be of the most useless sort, and a waste of public money. It is, in fact, in every way the reverse. It is a part, and a fundamental part, of the great whole of biology. On this foundation rises a vast superstructure of such things as ecology, evolution, genetics, agriculture and medicine, which without it, are a chaos of loose stones. Species workers only exist in order that others may be able, with the least possible loss of time and labour, to find out all that is known about the relations of a species to its fellows, its structure, distribution, and habitat. If they put difficulties in the way they stultify their own work and their own existences.

This is the way any systematist should feel the responsibility of his task.

The ultimate aim of the *International Rules of Zoological Nomenclature* should be to have an unchangeable system of names of all animals. Everybody will agree that such a system is very far off, if nothing is done to bring it nearer.

When 1758 was fixed as the starting point of zoological nomenclature, it was quite an arbitrary date. It did away with many older authors and gave credit to Linnaeus in a way he himself probably never thought of; this was when it was decided that a generic or specific name should be followed by the author's name. A widespread mental disease among systematists was the deplorable result, the *Mihilisme.* In many cases this disease can only be cured by an operation; this has been done by placing the systematic names on the *Official List* (O.L.)—and with good results, as far as the O.L. is known, but this is hardly beyond the narrow circles of systematists. To be of any real value, it must be considerably extended and comprise a few thousands of names, selected from textbooks on physiology, embryology, ecology, etc. These names should form the *Systema Naturae* of the twentieth century, a book to be found in any biological laboratory. Any name used in reference to this book should be cited by adding "O.L." beside the author's name; e.g., *Gadus* Linnaeus 1758 (O.L.). Nobody would have difficulty in checking the exact significance of the name, and long lists of synonymic names could be avoided.

In view of the existence of the Vanity Fair of the Mihilists, I dare only dream about the day when we can write *Gadus* O.L. 1958, and the still more remote time when plain *Gadus* cannot be misunderstood because all names will have passed onto the O.L.

By that time all Mihilists will have been forgotten, while we will remember the outstanding systematists in the same way we remember outstanding physiologists, anatomists, etc., and for the same reason—their contribution to the structure of the great building of biology.

So let us have the *Systema Naturae* of the twentieth century.

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Book Reviews

Numerical Solutions of Differential Equations. H. Levy and E. A. Baggott. New York: Dover Publs., 1950. 238 pp. $3.00.

This is an American edition of a volume first published in England in 1934 under the title *Numerical Studies in Differential Equations.* Except for the title itself and the omission of Volume I from the cover, the present American printing and the original English text are identical in content. Everything was reproduced verbatim, including the preface which states among other things:

> This, the first volume, concerns itself only with the actual solution of ordinary differential equations and the numerical examination of many of their properties. The determination of Characteristic Numbers (*Eigenwerte*) and the investigation of Orthogonal Properties in general are, however, omitted. These will be included in Volume II, since such properties are primarily of importance in connection with the practical solution of partial differential equations.

As far as the reviewer knows, no such second volume has ever appeared in print. It is indeed regrettable that the authors of this fine little volume on the numerical solution of ordinary differential equations have not been able to carry out their plans of writing a similar text for the numerical solution of partial differential equations—a field which, owing to recent developments in automatic high-speed computing machines, is rapidly becoming of vital importance in engineering and applied mathematics, and for which not a single text is yet in existence.

Since this book has been reviewed previously on a number of occasions, the reviewer will confine himself to a brief outline of its contents. The first two chapters deal entirely with graphical methods for the solution of ordinary differential equations. Certain geometrical properties that are useful in the application of graphical methods are discussed, and the solutions of a number of typical equations are carried out in detail.

Chapters III, IV, and V are devoted to the numerical solution of differential equations both by expansion in infinite series and by finite difference methods. Although the authors stress the application of these methods to special cases, which are carried out in detail and described with great clarity, these procedures are of general usefulness. Thus the Adams–Bashforth method, which is a general method for the numerical integration of differential equations, is taken up in a subsection