oldest tenable names and the original spelling of generic, specific and subspecific terms are used, including those formed with the termination *ii* from the names of persons. We note, however, an occasional slip, as, for example, *Phœthornis guyi,* which should be written *Phœthornis guy,* to agree with the author's practise in the case of *Chætocercus heliodor.*

Recent ornithological work concerning the birds of America has been carefully collated, and the present catologue appears to be brought well up to date. It is doubtless worth while to note that since a number of the families included are peculiar to America, the present catalogue presents a complete list of the genera, species, and subspecies of Steatornithidæ, Todidæ, Momotidæ, Nyctibiidæ, and, most important of all, Trochilidæ.

There is only one illustration, the frontispiece in color, which depicts *Urochroma costaricensis* Cory. There is no index, but this is probably to be supplied at the end of Part II.

This work is unfortunately marred by many typographical errors in both scientific and common names. We are also sorry to see the the perpetuation of the vernacular name "nighthawk" for the species of the genera *Nyctiphrynus, Antiurus, Setopagis,* and *Nyctipolus,* for these do not belong to the same family as the true nighthawks (Chordeilidæ), but to the Caprimulgidæ, and are nearly related to the whip-poor-wills. Furthermore, there does not seem to be a satisfactory reason for continuing the use of *Antrostomus* instead of *Setochalcis* for the American whip-poor-wills, since there are many excellent characters, external as well as osteological, to separate them from *Antrostomus carolinensis.* Also, the American barn owls are clearly subspecies of the European *Tyto alba,* not of the South American *Tyto perlata.*

This catalogue is the first serious attempt at an enumeration of the birds of all America, and is so well done that it can not fail to have before it a long career of great usefulness.

Harry C. Oberholser

SPECIAL ARTICLES

PSYCHOLOGICAL RESEARCH IN AVIATION[1]

With the exception noted below, official research on psychological problems of aviation was conducted under the direction of the Medical Research Board, a branch of the Air Medical Service which (the A. M. S.) took charge of the medical, physiological and psychological problems concerning the behavior of the fliers. I began work at the Bureau of Mines Laboratory in October, 1917, with two assistants, and continued in charge of the Psychological Department until August 30, 1918, on which date I was removed from the Mineola Laboratory. During this period the psychological staff grew to (approximately) twenty-five officers and seventeen men (counting several who were commissioned shortly after my leaving, on my previous recommendations). The response of the members of the psychological profession to my calls for assistance was most liberal, and although we did not have a staff large enough to do effective work it was on account of the difficulty in securing commissions, not on account of lack of competent psychologists willing to make the sacrifice.

Work on tests of flying ability was commenced earlier, outside the Medical Board, by Professor Stratton, at first independently, and later under the Air Personnel. Upon my urgent recommendation, the board succeeded in having Professor (now Major) Stratton transferred to the Mineola Laboratory, bringing all the psychological research under medical control, and on my removal Major Stratton was placed in charge of the department.

The first work required of the board was the construction and standardization of a test for determining, if possible, the individual flier's ability to endure the partial asphyxia-

[1] A paper read before the Baltimore meeting of the American Psychological Association in joint session with Section H, American Association for the Advancement of Science, December 28, 1918. Authority to publish, with deletions, granted by Board of Publication, S. G. O.

Asterisks in the text indicate deletions.

tion incident to high altitudes. I entered this work with the understanding that as soon as such a test was completed I should have opportunity to investigate other (and in my opinion) more important psycho-physical problems. (I should not want it to be supposed that I was willing to disorganize my university work for the development of such a test merely.)

A summary of the work in the development of the psychological part of the "rating test" has been published (by order of the board) in the *Journal of the American Medical Association*. More detailed account of the experimental work involved in the perfecting of this test will be given later (I hope) in a technical journal (probably in *Psychobiology*). There is not time, of course to elaborate the details here. I may point out that we embarked on an unknown sea, and that we were able to get results in a relatively short time must be ascribed to the small but highly efficient staff I assembled in the early months of the work. After trying a wide range of standard psychophysical tests, and some we devised ourselves after usual patterns, with puzzling results, we hit upon the actual primary effects of asphyxia, and were then able to devise tests to fit.

These primary effects, we found, are not on any special mechanism or division of the nervous system (I except of course the cardiovascular and respiratory mechanisms), but are upon the *integration* of the system, and are evidenced in the decrease in sensory-motor coordination, and in range and sustention of attention. The so-called "higher mental processes" are affected in so far as they depend on attention and coordination, and no further. For example, a man may be able to make accurate observations visually, up to the time he can no longer "keep his attention on the task" (that is, if diplopia does not interfere), and record them, until his records become undecipherable, and also be able to remember these observations with normal accuracy. These findings I consider of great importance for future psychological work.

The functioning of "attention peaks," as we have called brief spurts of normal attention and coordination, is one of the important practical findings. Even with large actual deterioration of the patient's mental ability, he is able to bring himself back to his usual level of efficiency for a brief period, if given the appropriate mental stimulus. In this way, ordinary methods of testing, entirely fail to show the patient's real condition. In my own case, for example, I become diplopic regularly in the early light stages of asphyxiation, but the usual oculist's test shows no diplopia until the asphyxia has reached a much more serious stage, because the presentation of the test object "pulls me together," and for the few seconds required I coordinate properly, relapsing into diplopia as soon as the test is over. The importance of these attention peaks in working on fatigue, bad ventilation, and drug effects, is too obvious to need further emphasis.

The work of the psychologists, as will be readily imagined, was necessarily extended over a wider scope than the devising of the fundamental psychological part of the "rating test." Such disagreeable tasks as securing a system of work under which visitors were excluded from the room where the patients were being tested, protecting the patient from distraction and excitement by careless remarks of those belonging to the combined medico-physio-ophtho-psychological group of examiners, and similar routine precautions against invalidating work, the psychologists were of course forced to assume. This produced friction at first, as many of the men beginning the testing work were unaccustomed to research work, but they soon grasped the situation, and excellent cooperation was secured with the men in the other departments engaged upon these tests.

Into the discussion of the test as a whole there is not time to enter far. As applied in the field, men were rated chiefly on the psychological findings, the cardiovascular, respiration and ophthalmological findings figuring in minor degree. Although the blood-pressure observations were introduced by the psychologists, and carried on by us until the physiologists were convinced of their value, we were

in no wise responsible for their use as a factor in rating, having introduced the observations simply as a protection to the patient, and for the securing of experimental data.

Owing to the necessity of hurrying the test to application as soon as it was standardized, we were not able to carry on investigations to determine the ultimate significance of the test.

Sets of experiments were several times started in cooperation with the physiological department, to determine the significance, but the need of the officers carrying them on for field work in applying the test necessitated their discontinuance. It is certain that the test gives a relative measure of the individuals' ability at the time tested, to withstand asphyxiation under the conditions of the test —that is, the individuals can be given a just serial order of rating, or grouped in classes according to ability. It is to be hoped that there will be data available some time on the points of the significance of these relative ratings for absolute altitudes and durations, and as to their dependence on temporary psycho-physical condition of the individuals, and also as to the influence of temperature, wind-stimulation, nitrogen tension and mental excitement.

The research done in the development of the rating test laid a foundation for the development of tests of psycho-physical condition or "fitness," in which aviators are understood to show variations having serious bearing on their work and safety. (The slang term "staleness" is commonly applied to the condition of an aviator when he is markedly unfit to fly for psychological causes.) The development of such tests is relatively simpler than the task actually accomplished in the asphyxiation test, but the limitations of time, space and staff prevented definite accomplishment along this line, although substantial progress was made.

Towards the construction of such tests a tactual discrimination test devised by Captain Sparrow is an interesting contribution. Among the hopeful means also is the controlled association reaction. The solution lies, however, according to my belief, not in single tests but in methodical combinations as in the rating test.

There was no opportunity to investigate the important psychological factors summed up in the term "morale of the flier." These problems were committed to the "flight surgeons," * * * .

Some important observations were made on psychological causes of accidents (although no research was done). From accidents in the low pressure chamber, and from study of air acidents, it was made evident that the painful stimulation of air pressure inward on the ear drum, may (aside from any lesion of the membrane) produce mental incompetence, and apparent unconsciousness. Such stimulation occurs at times when exceptionally rapid descents are made: sudden increase on the external pressure actually tending to prevent on account of the physics of the Eustachian tube, the opening of the tube to equalize the pressure. Unconsciousness may also occur, as the asphyxiation work makes obvious, from the lack of oxygen, but these cases present features different from the above, the descent improving the situation since it increases the oxygen supply. It was at first thought that the sudden increase in oxygen-supply to a patient who had nearly reached the limits of his endurance, might make his condition temporarily worse, producing an "oxygen intoxication," so-called. The work on the rating test however shows conclusively that no such effect occurs; the effect of giving oxygen is beneficial, whatever the patient's degree of asphyxiation, although on account of the latent period (the time required for the carrying of oxygen from the alveolar air to the nervous system) the *symptoms* may increase for a few seconds.

Of the work carried on by Captain Stratton, I shall not speak, as I imagine he would prefer to report it himself. Moreover, my only function in regard to it was to secure its transfer from the Air Service to the Medical Service, thinking thereby to give him increased facilities.

* * * concerning the nystagmus test for aviation candidates, the Psychology Depart-

ment was authorized by the Officer in Charge of the Laboratory, and later by the board, to conduct experiments and secure data bearing on the question * * * . The specific point on which we commenced work was whether the duration of nystagmus after rotation is an index of the sensitiveness of the mechanisms for appreciating motion and maintaining equilibrium. Other questions raised outside of the laboratory, *e. g.*, as to how far the semicircular canals constituted the mechanism involved, or whether they were the sole mechanism, seeming to us to have no practical bearing on the issue.

Our first attack on the problem consisted in observations on circus performers, vaudeville stunters, and dancers, in the effort to determine the nystagmus-reactions of these individuals of demonstrated high ability in equilibration. These observations were started by Captain G. R. Wells, and after being discontinued because of the necessity of sending him to a field station, were taken up by Captain Bentley. The results of these observations indicated the necessity of attacking the problem of practise-effects by controlled experiments. I consulted a well-known dancing teacher in New York City, who advised a daily practise period of half an hour, prophesying striking results in two weeks.

The experiment was carried out by Captain Bentley, on five men, who showed "normal" nystagmus the first day, when tested by Captain Wales, who had had experience in conducting the nystagmus test on examining boards. This observation was checked by Captain Bentley by more exact methods. Each man was given turning at the "standard" rate—ten turns in twenty seconds—first in one direction, then after a brief resting period in the opposite direction, and so on, for approximately half an hour. The total time of turning during the half hour was between three and four minutes. At the end of each set of ten turns, the nystagmus duration was recorded by an admirable method devised by Bentley. Practise was given as near daily as possible, but was interrupted by Sundays and military duties.

The nystagmus duration showed a reduction from day to day, following the form of the usual practise curves derived from other experiments on learning and habituation. In a short time all the men had durations far below the official lower limits for passing candidates (16 seconds) and one showed no nystagmus at all. The equilibration of these men was certainly no poorer than when they showed the "normal" nystagmus at the beginning.

An interesting field for further work on nystagmus was opened up by these investigations, which however had gone far enough to settle the point of practical military importance. It would have been well to ascertain the effect of varying the rate of rotation, * * * , and as I and the other men concerned in the experiments were shortly transferred from duty with the Medical Research Board, the investigation lapsed at that point.

The tests * * * which were carried on in airplanes upon deaf subjects and others, we had nothing to do with. * * *

KNIGHT DUNLAP

THE JOHNS HOPKINS UNIVERSITY

THE MATHEMATICAL ASSOCIATION OF AMERICA

THE third annual meeting of the association was held at the University of Chicago on Friday, December 27, 1918, in conjunction with the annual meeting of the American Mathematical Society. Eighty-six were in attendance at the sessions.

The first session was devoted to the subject: "Deductions from war-time experiences with respect to the teaching of mathematics." This was a conference participated in by representatives of colleges and universities in which the Students' Army Training Corps were located, including R. P. Baker, University of Iowa; W. D. Cairns, Oberlin College; A. R. Crathorne, University of Illinois; D. R. Curtiss, Northwestern University; W. B. Ford, University of Michigan; A. M. Kenyon, Purdue University, and H. E. Slaught, University of Chicago. These men reported upon the methods used and the probable lessons to be gained for the future of collegiate mathematics. Professor D. R. Curtiss also gave an account of "An experiment in supervised study" this being followed by