corn. This double recessive combination completely resembled sweet seeds in contour and could only be determined by subsequent breeding tests. The iodine method has been of value also in separating the waxy from the starchy seeds in hybrids with the large-seeded Cuzco variety where heretofore classification has been impossible owing to the looseness of the carbohydrate particles.

The fact must not be overlooked, however, that the identification of the carbohydrate as erythrodextrin does not furnish a complete explanation of the physical properties of waxy endosperm since like starch this carbohydrate can be found in an extremely floury condition.

The chief criticism of this latest contribution of Weatherwax to the literature of maize and it applies equally well to several of his previous papers has been epitomized aptly by East in his phrase "blending of the didactic and the dogmatic" but this manner of presentation of course is purely personal and should not detract from the observed facts.

A more serious defect and one also not unique in this paper on erythrodextrin is a tendency to magnify the importance of minor discoveries.

This tendency is well illustrated by the extended discussion of the genetic difficulties met with in sweet-starchy maize hybrids—difficulties which hardly can be solved by the discovery that waxy endosperm turns red in a solution of iodine—and the discussion of this subject might well have been postponed until a true chemical solution could be offered.

Appreciating that there was to be a delay in publication Dr. Weatherwax kindly furnished us with a copy of his manuscript early last May and at that time his statement that "the sporadic appearance of sweet corn as a mutant from starchy corn in many parts of America is known" was questioned. Since he has not found it expedient to qualify this statement it seems desirable to record that as far as we are aware no such mutations are known to geneticists. In the thousands of guarded pollinations that have been been made in starchy maize varieties in the past decade not a single instance of a mutation to sweet seeds has been reported. While the value of the discovery of the red reaction of waxy endosperm in iodine solution can not be affected by the truth or falsity of this observation, it seems unfortunate that such a statement should be published under circumstances likely to afford it credence.

J. H. KEMPTON

BUREAU OF PLANT INDUSTRY

THE PERSONAL EQUATION AND REACTION TIMES

THE question whether personal equation in star transits is a matter of reaction time may be one that has been somewhat confused by not using the respective terms with the same significance.

But perhaps the real difficulty has been in the interpretation of the particular form of personal equation in this case. The usual understanding of recorded chronograph transits is based on the explanation given in books on descriptive astronomy, in which the observer is represented as tapping the key in coincidence with the instant when a star is on the wire, as nearly as he can do so. The flying bird and a sportsman, armed with a gun, is the popular illustration. Thus for a Mallard, dropping into decoys, we allow a lead of a yard, while for the Black Brant, commonly called a goose in California, we lead ten feet, because he is flying high and his size makes his real speed very deceptive.

My own habit of observing, however, based upon early consultation with such masters in the "old astronomy" as Benjamin Gould and Lewis Boss, has been to tap the key when I see the star on the wire. Thus every record is late and consciously late, but the advantage is that accidental deviations from uniformity of record are presumed to be smaller than they would be in efforts to allow for the motion of the stars as they approach the wire, each with a special velocity.

The systematic error thus introduced is a part of the personal equation and would be of importance in the determination of absolute clock corrections and in longitude work. It is eliminated in the measures of right ascensions where the observed clock correction and clock rate depend upon observations of standard stars. We are there concerned in making the accumulated accidental errors of observation as small as possible.

My own understanding of the reaction time in this process is the elapsed interval between

the instant when the eye perceives the star to be on the wire, and the record of that perception by the finger with the key. Any estimate of coincidence based on judgment of speed or time interval does not enter. This estimate would appear to be the "synchronization of reactions and anticipated stimuli." Or, is an agreement to be prevented by the obstacle of a mutual understanding of familiar phenomena with a misunderstanding of the words in which they are described. In this freely translated "classical definition" of psychological descriptions I certainly do not wish to be understood as concurring.

Without pretense of undue modesty in the matter the conclusion established in my note in SCIENCE was not presumed to be a novel one in the domain of psychology. But the form of the test appears to be new, the comparison of the hourly rate of a clock at night with the daily rate at the epoch; and then only new as regards its completeness and the elimination of systematic errors as fully as possible. The tabulated results are not an exposition of the personal equation nor of reactions. They give the mean deviations of my personal equation from uniformity, and while fatigue is acknowledged to be existent it has introduced no mean deviation as large as the one-hundredth of a second in the intervals of two hours each. If these results, in their completeness and precision, contribute to the establishment of facts that are of interest to psychologists, that seems to be a matter of our mutual benefit.

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We find in the psychological laboratory that there are two quite distinguishable conditions for reaction to a visual stimulus. Under one condition, which we have conventionally described as "simple reaction," the stimulus appears suddenly, at a moment which the reactor cannot predict, although he is given a "warning signal" preceding the stimulus by the period of from 1¼ to 1¾ seconds, understanding fully that the foreperiod is thus variable. The reactor's attention is thus approximately at its best during the short period of time within which the stimulus may actually appear. Under such conditions the average value of the reaction time should be from 130 to 160 *sigma*, varying in accordance with factors such as the type of individual, the amount of previous practice, etc. The average may, under certain conditions, have a value somewhat greater, but if over 180 *sigma*, we at once suspect serious error in the technique of obtaining and measuring the reactions, and even under the best conditions, with experienced reactors, an average of less than 120 *sigma* is unusual. The distribution of the individual reactions is, however, wide and a mean variation of not over 10 per cent. of the average is considered a safe measure of reliability. On this account, from 100 to 500 reactions are required to establish a valid average for a given reactor under given conditions.

The second condition for reaction (which is found in the so-called "complication experiment") is that in which the reactor sees the stimulus-pattern changing at a uniform rate towards an expected stage at which it constitutes the "stimulus." For example, he sees the star approaching the cross-wire in a transit instrument, or a pointer approaching a given mark on a dial; and the coincidence of star and cross-wire, or pointer and mark, constitutes the "stimulus." Under this condition, two different types of reaction are obtained: (*a*) When the reactor attempts to make his reaction *synchronous* with the stimulus pattern. In this case the reaction may actually follow or may precede the stimulus-pattern when seeming synchronous, but the positive or negative error usually varies from a few *sigma* to 30 *sigma*. (*b*) When the reactor may definitely attempt to make his reaction *follow* the stimulus pattern. Even in this case, although the reactor may succeed in making his "error" positive, it is usually relatively small; that is to say, only a fraction of the normal "simple reaction" time. In other words, the conditions here are those described under the first type (*a*) but with a subjective "set," which makes the error positive. This, however, is only one type of condition which will give a positive rather than a negative error in reactions of the "complication" type. It is not necessary that the estimation of the course of change in the stimulus-pattern should be consciously made. In fact, the actual stimulus-

pattern for any reaction under such circumstances involves a temporal factor whether the reactor analyzes the temporal factor or not.

Whether a certain group of reactions to a visual stimulus are of the "simple reaction" or the "complication" type can usually be decided from the average value, even where the conditions of reaction are not fully described. If the average is over 120 *sigma,* the reactions were mostly of the "simple reaction" type, although (if we do not know all the conditions) we can not be assured that some reactions of the other type may not have been mixed in. If, however, the average is under 50 *sigma,* we can be certain that the reactions were practically all of the "complication" type, since the "simple reaction" will seldom occur if conditions are such that the "complication" reaction can occur, and is occurring in the majority of cases.

With these facts in view, it seems to me that there is no room for confusion between the conceptions of the psychologist and those of the astronomers.

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ÆROBIC?

DR. W. W. KEEN'S objection to the word "erobic" (SCIENCE, March 23, p. 360) is perfectly valid. However, the substitute he proposes is equally objectionable. The Greek word is ἀήρ. Like most of our Greek words this comes to us through the Latin transcription, in this case aër, formerly written with a dieresis. Now that the dieresis is no longer in vogue, some confusion is apt to occur. At any rate, the word is a dissyllable and not a diphthong. Dr. Keen would not propose the use of a diphthong in the words aerate, aerial, aeronaut, etc.

ARTHUR W. DOX
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TO THE EDITOR OF SCIENCE—Dr. Dox surely has misread my letter in SCIENCE protesting against "erobic." I urged the retention of the "aer" as a disyllable. Personally I should use the diacritical mark to indicate the proper pronunciation.

The dropping of all accents in French by the newspapers during the war is wrecking their pronunciation, *e.g.,* "Poincaré" is often pronounced "Poin-car" or "Poin-care" and "communiqué" is distorted into "communeek" or even "communeek-quee."

If Dr. Dox will consult the Oxford Dictionary he will find that "disyllable" with one "s" is preferable to double s and that "dipthong" is an obsolete form of "diphthong" which in Greek is spelled with φ and θ (ph and th).

W. W. KEEN.

QUOTATIONS

PARK CONSERVATION

No one can justly charge the American Association for the Advancement of Science with flightiness, or suspect it of being swept off its feet through unpractical sentimentalism. Its strong plea to the governments and people of the United States and Canada that the great wild parks of both countries be more completely safeguarded against commercial exploitation of every form will, therefore, command attention and respect. The resolutions that the association has now issued upon this subject would seem to indicate that it recognizes the threatened encroachment of power developments within the national parks of both countries. Its appeal is not addressed to this particular menace alone, however, but calls attention to the fact that the parks are in many cases open to incursion by those who regard their various natural resources with covetous eyes. Gradually the public is coming to understand that the national parks are by no means as proof against materialistic raids as had been supposed. This call for a greater degree of protection, coming as it does from one of the most distinguished scientific organizations in the world, will tend to emphasize the situation and carry conviction as to its economic importance.

It will not be suspected that this association is unsympathetic with the endeavors of those who are legitimately developing the resources of the two countries. There is more than an implication in its resolutions, though, that it would be poor business to allow the material resources of the parks to be utilized except in those cases where a compelling and unquestionable public necessity exists. To safeguard the parks more thoroughly against unnecessary utilitarian developments, the association recom-