dian amplitudes derived during these same behavioral trials. Reciprocals of the peak-to-peak amplitude have been plotted. The similarity in variation of the evoked potential amplitude and RT observed at 250 hz represents one of the clearer observations of this relationship. Curves at 8 khz represent average observations. For the 8-khz curves, both RT and the reciprocal of evoked potential amplitude increased rapidly at the next lower intensity (30 db). However, the amplitude of the evoked potential was reduced to a point where peak-to-peak amplitude could not be reliably measured with our procedure.

It has been suggested that the evoked potentials recorded in this investigation are "primary" evoked potentials. If this is indeed the case, and if the response reflects neural activity involved in elicitation of the behavioral response, one questions the lack of obvious changes in the latency of the cortical response with changes in the intensity of the stimulus. A possible explanation that may account for this is: observations on primary evoked responses have shown that most rapid latency changes occur at and near threshold levels of stimulation. At higher intensities latency changes are minimum. It may be that the intensities used in this investigation were at a level producing only minimum changes in latency. Latency dispersion as the activity continues through the synaptic system involved in this behavior would account for the latency changes observed in the reaction times.

Similar procedures have been used to demonstrate that when direct visual cortex stimulation was substituted for photic stimulation, behavioral response latency shifted by 30 to 39 msec. This behavioral response shift may be compared to the 15-msec response shift observed in the auditory system. Both latency shifts agree approximately with the latency of the peripherally evoked primary potential recorded differentially through the stimulating electrodes. This observation, plus the ease with which behavioral responses transfer from peripheral to central stimulation, suggest that in the monkey the cortex may function in the control of this behavioral response.

One of the primary advantages of the procedures we used is that they yield evoked potentials of sufficient stability to permit individual analysis and study, without the necessity of summing devices in unanesthetized animals. The potential appears to be the primary evoked potential; thus an extensive literature on its properties and the possible basis for its generation is available from neurophysiological investigations. These advantages apply to the stable and extensively studied reaction time response. The characteristic stability of these measures suggests that they are suitable for individual trial comparison. Furthermore, the relation between the temporal characteristics of the behavioral response and the temporal and amplitude characteristics of the neurophysiological event provides a meaningful basis for comparison and subsequent study of the relation of cortical function to behavior.

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13 September 1968; revised 15 November 1968

Avena magna: New Oat Species

An annual species of oats, described (*1*) as *Avena magna* Murphy and Terrell (sp. nova), is a tetraploid, $2n = 28$. It is related to the well-known hexaploid *A. sterilis* of the Mediterranean shores with a center of origin and dispersal in Asia Minor. The new species was found, by Zillinsky, near Rabat in Morocco, within the area of *A. sterilis*.

This is a very important discovery, since among the annual species of oats (*Avena* sect. *Avena*) there have long been known several species of a polyploid series: *A. strigosa*, $2n = 14$; *A. barbata*, $2n = 28$; and *A. fatua* and *A. sativa*, $2n = 42$.

In 1953 I found a strange form of oats near Sassari in Sardinia. At first I did not hesitate to refer it to *A. sterilis*, because it differed only slightly from this species, in a weaker habit and fewer and more sturdy spikes. These features did not justify referral to another form. However, I found it to have $2n = 28$ chromosomes, instead of $2n = 42$ typical of *A. sterilis*. Therefore, I pointed out (*2*) that a tetraploid biotype of *A. sterilis* occurs in Sardinia. The karyogram of this taxon was carefully analyzed. The chromosomes range in size from 5.9 to 2.6 microns, and the karyotype was found to include one pair with satellites, six pairs with a median centromere, and seven pairs with a submedian or subterminal centromere.

According to Murphy *et al.*, the karyotype of the new species shows two pairs with satellites, four pairs with a median centromere, and eight pairs with a submedian or subterminal centromere. The differences from my observations are small and caused by difficulties of observation, since the second pair of satellites is very small and I was in doubt as to inclusion of two pairs in the median or submedian category. I have no doubt that the plant studied in Sardinia is identical to that described by Murphy *et al.* from Morocco, both morphologically and karyologically.

I left Sardinia in 1956, and later botanists have not found the tetraploid there, probably because it is easily confused with the hexaploid *A. sterilis*.

Murphy *et al.* apparently were unaware of my publication, though the chromosome number was listed (*3*) in 1959. The apparently wider distribution of the tetraploid may be of some interest to cytotaxonomists.

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24 May 1968; revised 26 August 1968

Professor Martinoli points out that the new tetraploid oat species of Murphy *et al.* (*1*) had been observed previously from Sardinia. However, another considerably more serious flaw in their discussion is that Murphy *et al.* split out a new species from the Linnaean taxon *Avena sterilis* without typifying the latter and only assuming, but not proving, that it is a hexaploid. Most chromosome numbers from what has been called *A. sterilis* have been counted on eastern Mediterranean material, or on cultivated plants of unknown origin. The Linnaean species was, however, described from Spain, and no reports of chromosome numbers are known from Spain. When the tetraploid was discovered, herbarium studies might have shown its occurrence in Spain, and then the authors could have determined what the Linnaean type, if available, represents. If identical with the hexaploid as assumed, this is fine, but if its identity cannot be established with certainty, a new type-specimen must be selected, preferably the hexaploid if it occurs in Spain. The next step ought to have been a study of all previous descriptions of species of this group and a search for them in herbaria of southwestern Europe and northern Africa, before describing the taxon as new. There is a possibility that it is identical to, say, *A. algeriensis* Trabut or another of the species described in profusion by Old World botanists. It is unlikely that European taxonomists have failed to observe taxa which we now find differ in chromosome number, despite the fact that authors of manuals may have incorporated them in collective species, especially grasses.

It is likely that the name *A. magna* will soon be relegated to synonymy caused by the same ignorance of European material and taxonomical methods as when *Hordeum glaucum* was redescribed as *H. Stebbinsii*, or *Rumex stenophyllus* was renamed *R. alluvius*, both in America. Care is no less important in taxonomy than is a logical species concept. Nevertheless, the article by Murphy *et al.* may be a stimulus to further research on the taxonomy of *Avena* by modern specialists within its area of distribution.

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3 September 1968

The taxonomic part of the paper by Murphy *et al.* (*1*) was entirely my responsibility, and I am glad to have the opportunity to reply to Dr. Martinoli and Dr. Löve (*2*). Nowhere in Dr. Martinoli's letter does he give adequate morphological evidence that his collection was the taxon we described. His main evidence is that they have the same chromosome number and similar karyotypes. After learning of Dr. Martinoli's paper (*3*), which was admittedly overlooked, Dr. Murphy made an effort to obtain material of the Sardinian collection. Dr. Martinoli wrote to the U.S. Department of Agriculture on 21 July 1968 that he had no material from his collection but hoped to obtain material in the future (*4*). Until a new collection can be studied, it is not possible to determine whether the two taxa are the same or not.

Dr. Löve, first, uncritically accepts Martinoli's letter as adequate evidence that the two taxa are the same. Next, he states that we "split out a new species from the Linnaean taxon *Avena sterilis* without typifying the latter." The fact is, however, that we described *A. magna* as a distinct species in its own right, not as a segregate from *A. sterilis*. We said that *A. magna* resembled *A. sterilis* more than it did any other species. A careful study of our comparison discloses that the two taxa differ in several characters. Moreover, *A. magna* is not just a morphological extreme within the total variability of *A. sterilis* sens. lat., but is set off by distinct morphological discontinuities. Combinations of characters are found in each that do not occur in the other. This suggests entire linkage groups not common to both.

Avena sterilis is typified by two Linnaean specimens, microfiches of which I have seen. I am certain that their identity will not affect the status of *A. magna*. This problem might be solved by a taxonomic revision of *Avena* (*5*).

We investigated the present usage of the name *A. sterilis* on the basis of literature, available herbarium specimens, and collections introduced for agricultural research. In certain European and two recent North African floras there was no indication that any taxon was recognized which resembled *A. magna*. The nearest morphological approach was *A. sterilis* subsp. *macrocarpa* including its variant forms recognized in Malzew's monograph (*6*) (cited in our paper).

Trabut stated that *Avena algeriensis* Trabut is a cultivated form of *A. sterilis*, and his illustration shows it to be quite different from *A. magna* (*7*). There is no way to guarantee that *A. magna* has not been previously described under another name without studying all of the many old descriptions of new taxa as well as the type specimens (presumably scattered all over Europe). Anyone undertaking this would certainly go on and also complete the other activities necessary for a taxonomic revision.

I agree that not enough is known about chromosome number in *Avena sterilis* sens. lat. to insure that it is hexaploid throughout. It is necessary to sample a species widely before one can be sure of its chromosome number(s). However, it is likely to be some time before such data are accumulated.

Dr. Löve suggests that a search of North African herbaria would have been desirable. This presumes an ideal situation, however. While doing a taxonomic revision of the European and Asian genus, *Lolium*, I attempted to borrow specimens from four North African herbaria. Three herbaria did not reply, while one refused. This was in contrast to generally good cooperation from European herbaria.

We took a well-calculated risk based on careful although necessarily limited research that *Avena magna* would stand up under future scrutiny. If, for one reason or another, *A. magna* is relegated to synonymy, as Dr. Löve thinks likely, we are willing to accept this. However, so far, there is no evidence at all that this is necessary. Dr. Löve would, I think, have had us complete a taxonomic revision of the genus *Avena* before publishing *A. magna*. If he demands the same of all botanists describing new species in insufficiently known genera, he will have many more letters to write.

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8. H. C. Murphy, the senior author (*1*), died 20 July 1968, while on an official trip.

15 November 1968