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THE USE OF ELECTROCORTICAL ACTIVITY TO MONITOR HUMAN DECISION MAKING

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University of Illinois at Urbana-Champaign

Department of Psychology

THE USE OF ELECTROCORTICAL ACTIVITY TO MONITOR HUMAN DECISION MAKING

Semi-Annual Technical Report

Period Terminating February 1, 1974

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A. SUMMARY

1. Research Program Plan

The primary goal of this project is to develop and test techniques whereby a computer could obtain, on-line, information about the activities, intentions or degree of information processing of a human operator, through an analysis of this operator's brainwaves. Our plan derives from two main findings. We, and others, have shown that certain components of the human cortical evoked response, specifically the CNV and P300, are very sensitive to the subject's anticipations, intentions and information-processing activity with respect to stimuli. Secondly, we have been able to show that when cortical evoked responses are elicited under clearly distinct experimental conditions, it is possible to develop a discriminant function, using the step-wise discriminant analysis. This function can allow a determination of a subject's on-going response to stimuli specified in terms of the distinct conditions under which the discriminant function was developed. In other words given two situations which define a behavioral dimension we can develop a discriminant function that will allow us to place subject's responses on that behavioral dimension through the application of the discriminant function to his EEG activity. The research plan calls for the identification of such template conditions, the development of the appropriate discriminant functions in baseline experiments and then the creation of closed loops including a subject and a computer, where the computer is continuously evaluating the discriminant function and responding to the subject's varying states, as a function of the output of the discriminant function.

2. Accomplishments During the Reporting Period

The bulk of the equipment necessary for the performance of this project has been delivered or is in the final stages of delivery and is currently being installed in the laboratory. We have on hand the following major items:

a) Grass 12-channel polygraph acquired for the recording of the EEG equipment.

b) The PDP11/40 system including the two discs, the central processing unit and various accessories has been delivered on time by DEC. Regretably DEC has not fulfilled its commitment to deliver the display unit simultaneously with the computer. They informed us during the last week of November th t they must postpone delivery of the display device until the end of February 1974. It seemed advisable to accept this delay. We did, however, obtain from DEC a commitment to supply us with a software package for our PDP8/E priced at $700.00, at no charge, which would allow us to start pilot work in an interactive form through the PDP8/E until the display is delivered. We have also encountered various problems with the software actually delivered by DEC for use with the 11/40; however, these problems are generally under control and are normal to the installation of a new computer system.

c) A tape deck for the PDP11/40 has been delivered by Digidata and has been installed.

d) A line printer/plotter has been delivered by Gould Inc., and installation is under way. There are several minor hardware and software problems but again these are under control. I expect by mid-February to have the entire system fully operational and to be running experiments.

e) To allow an extension of our capacity to perform visual perception experiments we ordered from Siliconix Inc. a Tachistoscope for the controlled presentation of complex stimulus materials. This unit has been delivered and is functional and will be interefaced with the PDP11/40 within the next few days.

We have begun an extensive test of the feasibility of applying discriminant analysis to evoked response data for the purposes enumerated in Section 1 above using data from several previous experiments. We concentrated our efforts over the last three months on the analysis of sensory-threshold data using auditory stimuli and on the prediction of motor responses using the pre-motor electrocortical potentials.

In the auditory threshold experiment our data base consists of recordings from 10 subjects each of which having been presented series of auditory stimuli at successively increasing intensities. For each of these subjects we have developed a discriminant function using the data collected with one of the more intense stimulus presentations and we applied that function to the data obtained from that subject and the data collected from the other subjects. As we have found before, the discriminant function technique works very well within the confines of each of the subjects' own data. That is, for any one subject we could quite nicely determine the auditory threshold from discriminant function scores provided that we are using the discriminant function developed under this subject's training set. Most of our effort over the past two months has been concentrated on developing techniques for combining data from several subjects so that a normative discriminant function could be developed. We feel that the applicability of this approach to actual data monitoring would be considerably enhanced if cross-subject functions could be developed. We are trying a variety of techniques for combining the subject's data and a detailed report on this analysis of their results will be submitted separately as a manuscript prepared for publication. We anticipate completion of this analysis within the next reporting period.

The data base for the prediction of motor responses is rather large. We have 11 subjects squeezing a dynamometer with either their right or left hand on many different occasions at many different force levels; as we noted

before we can with considerable ease determine from the evoked response, averaged over the subjects, whether the data came from subjects using their right or their left hand. The determination can be based on data collected three or four hundred milliseconds prior to the actual response. In developing a discriminant function approach to these data we are now pooling all the data obtained from any one subject for all experimental series. We determine for each subject the two series, one with the right, one with the left hand response, which produced the largest difference and provides the best discrimination for the subject. This function is then applied for all the data obtained from this subject. These data are still being analyzed. However, the preliminary results are quite encouraging. Again, we hope to have a detailed report during the next reporting period.

This analysis has made us acutely aware of the need for a more efficient data management system. During the present recording period we have intensively explored the feasibility of implementing our data processing and management on the ARPA network. This search has been done within the framework of an overall plan by the biocybernetics project to develop data sharing and method sharing approaches on the ARPA network. As a consequence of the meeting, devoted to this problem held in Boston on December 13, 1973 we have been asked to intensively pursue the feasibility of implementing data management on Multics and to pursue the possibility of data processing at CCN. These tests are conducted using the data base of the auditory threshold experiments discussed above. At the present time we have acquired considerable experience in manipulating data at Multics using a simple data set we have sent over the network using a card deck and a tape of data has been successively read into Multics last week. We hope to have by the end of the next reporting period a functioning network interacting systems, serving us and other biocybernetics contracts.

B. INTRODUCTION TO PAPERS AND REPORTS

The reports and papers which constitute the bulk of this technical report discuss research conducted in this laboratory prior to the award of the ARPA contract. Yet, the work described is directly relevant to the research program under review. In fact, the primary findings and our approach to evoked potential research and the biocybernetic goal derive from the work described in these reports. We do therefore find it useful to include the papers in our first semi-annual report so that our future progress, if any, can be placed in the proper perspective.

Brief descriptions of each of the reports follow:

a. Donchin, E. Monitoring Human Decision Making Using Electrocortical Potentials.

This paper is the text of the presentation made to the first contractor's meeting of the Biocybernetics project. It presents a summary of the work performed in our laboratory with NSF and Office of Education support which provides the foundation on which we base the Biocybernetic application of the human evoked potential. The paper sets forth the program's goals and our research strategy.

b. Donchin, E. Measurement in AEP Studies.

This paper and the following two were presented before the 3rd International Congress on Slow Potentials on Behavior, held in Bristol, England. The proceedings of the conference are now in press and are scheduled to appear in 1975, (some two years after the originally scheduled publication date). While the research described was not supported by ARPA, E. Donchin's participation in the Congress was supported by ARPA. The report on Measurement in AEP Studies is the text of the remarks opening the Methodology Section of the Congress. They summarize the approach

we take to the study of event related potentials. The paper reviews the problem of quantification in brain-potential research and illustrates the manner in which Stepwise Discriminant analysis has been used in our laboratory. Some pitfalls are enumerated

c. Donchin, E. (Ed.) The Relationship Between P300 and the CNV: A Correspondence and an Experimental Demonstration.

This manuscript has two parts. The first is an edited version of an international "Teleconference" held through the facilities of the Postal Service and conducted in the hope that it will lead to the resolution of the vexing issue of the degree to which the two major endogenous components of the human evoked potential are independent. This correspondence lead to the development of an experimental design which, it was hoped, would allow the resolution of the issue. During a six week period, E. Donchin, and later Patricia Tueting and Walter Ritter developed the design at the laboratories of the Burden Neurological Institute in Bristol, England. The second part of this manuscript describes the experimental sessions conducted in Bristol. This study has been subsequently conducted in our laboratories, and the results published.

d. Donchin, E., Johnson, J., R., Herning, R., and Kutas, M. Covariation of the Magnitude of the CNV and P300 as a Function of the Subject's Task.

A brief report in which we show that the scalp distribution of the CNV and of P300 are different, and that the two components tend to vary in different manners as a function of variations in experimental conditions. Our preoccupation with the issue of CNV/P300 interactions derived from the fact that had it turned out that all components of the human evoked response vary in a global diffuse fashion their usefulness in

Biocybernetic communication would have been greatly reduced. As it turns out each of the EP components is exquisitely sensitive in a unique manner to the varieties of cognitive experience. These, and other, findings lead us to the present search for the vocabulary of the human event related potential.

C. PAPERS AND REPORTS

a. Donchin, E. Monitoring Human Decision Making Using Electrocortical Potentials. Paper presented before ARPA's contractors' meeting UCLA, March 9, 1973.

b. Donchin, E. Measurement in AEP Studies. In McCallum, W. C. and Knott, J. (Eds.) Proceedings of the 3rd International Congress on the CNV and Slow Potentials. Bristol, England, August, 1973 (in press).

c. Donchin, E. (Ed.) The Relationship Between P300 and the CNV: A Correspondence and an Experimental Demonstration. In McCallum, W. C. and Knott, J. (Eds.) Proceedings of the 3rd International Congress on the CNV and Slow Potentials. Bristol, England, August, 1973 (in press).

d. Donchin, E., Johnson, Jr., R., Herning, R., and Kutas, M. Covariation of the magnitude of the CNV and P300 as a Function of the Subject's Task. In McCallum, W. C. and Knott, J. (Eds.) Proceedings of the 3rd International Congress on the CNV and Slow Potentials. Bristol, England, August, 1973 (in press).

MONITORING HUMAN DECISION MAKING USING ELECTROCORTICAL POTENTIALS[1]

Emanuel Donchin

Department of Psychology

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Champaign, Ill. 61820

Paper presented before ARPA's contractors' meeting

UCLA, March 9, 1973

[1]The studies described in this manuscript were supported by the U.S. Office of Education (OEG 5-70-0030-508) and NSF (GB 3099). The studies were conducted in collaboration with John Rohrbaugh, Mike Kubovy, Marta Kutas Raymond Johnson and Ron Herning, and has been reported in detail elsewhere as indicated in the text. Preparation of the manuscript has been supported by the Advanced Research Projects Agency of the Department of Defense under Contract No. DAHC 15 73 C 0318.

It was only two or three days ago that I learned that I have to be here today, and not having come from my lab (I am spending a Sabbatical semester in Haifa, Israel) I could not prepare for you all the material I would have liked to present. So, bear with me while I try to make my case. I will make an assertion, illustrate the type of data which provide strong support for that assertion, and try to relate the assertion to the goals of this program as well as discuss some of our specific plans.

I am asserting that during the last five years, it has been convincingly demonstrated that certain components of the human Average Evoked Potential (AEP) represent endogenous cortical activity, elicited not by a stimulus but by the data-processing needs of the cortex. True, we often need a stimulus to synchronize cortical data processing with our averaging computers. However, the components I refer to represent intrinsic, rather than evoked, cortical activity.

Early work on the human evoked potentials (Katzman, 1964) was conditioned by the numerous studies of evoked potentials in the anesthetized animal to view the AEP primarily as an exogenous event. The cortex was viewed as a passive matrix in which input volleys triggered responses. "Late" AEP components, that could not be considered a direct response to input volleys, were assumed to represent the activity in circuitous pathways or reverberating circuits. The modulation of evoked response amplitude by the subject's alertness, or his general state, were assumed to reflect changes in cortical "excitability" (Donchin & Lindsley, 1965; Haider et al., 1964). All in all, the literature was guided by a view of the cortex which considered it a rather passive switching matrix, a view consistent

with the strongly Connectionistic view which dominated Psychology, and the Brain Sciences, in the early decades of this century.

This view is not adequate. I will not discuss here its logical and theoretical deficiencies, though these are becoming increasingly obvious (Quarton et al./, 1971, 1972). 1967; Schmitt, 1970 I will rather restrict myself to the implications of specific set of AEP reports which have appeared since 1965. These studies have demonstrated that two AEPs (recorded, say, at the vertex) elicited on two occasions by identical physical stimuli, from the same subject, under identical physical conditions can be quite different, their difference a function of the role of the eliciting stimuli in the task the subject is performing. The difference between the two AEPs will be in a positive-going component which has an approximate latency of 300 msec, and which I will call P300.

Let me illustrate with data obtained in a doctoral dissertation study, completed at the University of Illinois by Dr. John Rohrbaugh (Rohrbaugh, Donchin and Eriksen, 1972; Rohrbaugh, 1973). Rohrbaugh used a stimulus configuration such as is shown in Figure 1.

Figure 1

The subjects faced a matrix of plexi-glass cubes (Fig. 1). The cubes could be illuminated thru fiber-optics. Using different lamps and different bundles of fiber-optics we could illuminate any subset of these cubes. If all the cubes labeled "1" in the figure are illuminated, the subject sees a matrix of 9 squares. If we flashed all the cubes numbered "1" simultaneously with all the cubes numbered "2" the subject would, of course, see three horizontal lines. Alternately, if we illuminated the "1" group with the "3" group, the subject would see three vertical lines.

Note that one can generate either horizontal or vertical lines using different combinations of the 9-square matrices. If we illuminate one of these matrices, at random, and illuminate the second matrix 200 msec later, the subject cannot tell if he has seen horizontal or vertical lines until the second matrix has been flashed. Furthermore, neither the first nor the second matrix by itself provides sufficient information for determining whether the lines were vertical or horizontal. The subject must accept information contained in both matrices, integrate it over the 200 msec interval if he is to identify correctly the stimulus. Of course, if we inform the subject ahead of the trial which stimulus will be presented--he need not bother integrating the two matrices--all he needs to know is when the stimuli were presented.

Given this arrangement we ran the following experiment. Each of four subjects was presented with several series of paired stimuli. The series differed in that a specific interval (the "delay") between the two matrices (i.e. 0, 15, 50....450 msec) was used in each series. Each delay was used in two different series. In one, the two patterns alternated, that is, horizontal lines were followed by vertical lines, etc. In the other, the order of pattern presentation was randomized. Of course in an Alternating series (ALT) the subject knows precisely which stimulus will follow and therefore has no need to integrate the information in the matrices, a task he must perform during a random (RAN) series.

The results are shown, for four subjects, on Fig. 2. Each of the

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Figure 2

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traces shown is an AEP elicited by the presentation of one pair of matrices. The columns are aligned on the second matrix, as indicated by the vertical

line. The time of first matrix presentation is indicated by the small cross hatches. Data are presented for the Vertex and for the Occipital electrode. Note that each pair of superimposed AEPs were obtained under identical physical conditions, except that the solid lines were obtained when the patterns alternated, the dashed lines were obtained when the patterns were presented in a random order.

It is clear that the two AEPs in a pair differ. In each case the RAN series AEP is characterized by a large, positive going component, which is either absent, or is very small in the ALT AEP. It is this component which we call P300. Several of the important features of P300 are clearly revealed by Rohrbaugh's experiment.

First--note that P300 is elicited only on the RAN series, in spite of the fact that the two AEPs in each trace pair were obtained under identical physical conditions. Second--P300 is associated in each case with the second of the two matrices, the one which allows the subject to decide which of the stimuli was presented. The first matrix, in spite of the fact that it "delivers" task relevant information to the subject, is not associated with a P300. Third--note that the early components of the RAN and ALT AEPs are quite similar. It has been suggested (Karlin, 1970), that the differences in P300 are due to differences in the generalized arousal state of the subject rather than to specific cognitive data processing activity, and that the enhancement of P300 is merely a matter of a more aroused cortex responding to stimuli (a "Big Bang" interpretation of cortical activity). However, had we been observing the effects of general arousal, a similar effect should have been observed in all components of the AEP. As the effect is restricted to a specific component, it does not

seem reasonable to attribute it to diffuse, generalized, arousal.

Thus P300 appears to be a component of the AEP which is enhanced, or in fact elicited, when, and if, the subject needs to, and does, perform a cognitive act of some sort. As I asserted above, P300 is an evoked potential component which is associated with intracortically generated, intrinsic, data processing activities. The strongest evidence we have for the endogenous nature of P300 comes from several reports in which P300 has been elicited in the absence of a stimulus. Rohrbaugh's study is important in that it demonstrates the dependence of P300 on the subject's final decision, and in that it rejects the "arousal" hypothesis.

Within the context of the project ARPA is supporting, the important point is that in P300 we have a relatively easy to record, objective, indicator of thought processes. In an attempt to develop close coupled man-machine systems, this is of enormous potential significance.

It seems to me that two conditions must be met if we are to realize this potential. We must have a better understanding, or a more precise definition, of the psychological variables which determine P300, and we must have a good method for quantifying the data. (Another set of problems has to do with the physiology and the functional significance of P300, but these I shall ignore in the present discussion.)

I tend to think of the first problem as analogous to the problem of mapping receptive fields. In such mapping we have a physiological response, and a "space" in which are included all conceivable circumstances which affect this response. This space is scanned systematically, and the response is measured for all points in the space. The subspace which includes all the points which affect the appearance and amplitude of the physiological response is the receptive field. For visual space the

technique is straightforward. For cognitive space, that collection of circumstances which tend to affect P300, the procedure is, of course, far more complex. Unlike visual space, "cognitive" space is difficult to define or measure. Yet the analogy suggests a research strategy which I shall illustrate with one of our recent studies which will provide a framework within which I can describe our approach to the quantification of the data.

Recall that in Rohrbaugh's study, by comparing a RAN with an ALT series we could dramatically demonstrate the enhancement of P300. This manipulation, first described by Sutton et al. (1965) is indeed quite powerful. In the experiment I shall now describe (Donchin et al., in press) we assumed that these two conditions define the extremes of a hypothetical dimension, and we tried to develop points intermediate on the dimension by varying the rules which generate the sequence of stimuli in the series.

Our subjects were presented on each trial with a tone followed 1400 msec later by a flash which illuminated either the letter A or the letter B. The subject's task was to predict before the tone which stimulus (A or B) will follow. There were five such series, and they differed in the way in which the sequence of As and Bs were determined. Two series were of course the RAN and the ALT sequence described above. A somewhat different condition is created if we use a perfectly predictable series, in which a sequence of nine trials is repeated, for example, AABABBAAB, AABABBAAB, etc. Now, if the subject knows the unit sequence, and he knows what his position is in the series, he can predict the next trial's stimulus. In one of our series such a predictable sequence was used, and the subject was allowed to memorize the sequence before beginning the recording--this we

call our LRN series. In our PAT series, the subject was told that there is a pattern in the sequence, but he was to deduce it from the presentation sequence. Finally, we used a "shafty" series (SHF) where we presented the subject with a random series and told him that he had to find a sequence in the data. Of course, he never found it, yet he searched for it.

It turns out that the amplitude of P300 associated with each of the 5 series can be ordered according to P300 amplitude, ranging from RAN to ALT thru SHF, PAT and LRN in that order. Figure 3 shows the data from one subject in support of this statement. However, subject's AEP waveforms

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Figure 3

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are notoriously variable and we need a technique for objectively quantifying the data.

We used for this purpose the program BMD07M, a Stepwise Discriminant Analysis (SWDA) program developed at UCLA (Dixon, 1970). We have recently conducted a detailed simulation of AEP data whose goal was to assess the efficacy of BMD07M in analyzing such data.

In Figure 4 you can see the simulation scheme. We synthesized evoked-

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Figure 4

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potential-like signals by summing five damped sinusoids and mixed them with "noise" generated by an autoregressive process having precisely defined characteristics (it was a first order autoregressive process). By taking two different "AEPs" with clearly defined differences, and mixing each with 100 realizations of the noise--we simulate an AEP experiment. In Figure 5 we show "AEPs" obtained by averaging the synthesized data. These

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Figure 5

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curves are based on 80 points each, and are thus in effect points in an 80 dimensional space. A discriminant function (DF) is a linear transformation of these points which projects them from the 80-dimensional space to a line. The DF is that specific linear combination which maximizes the separation in the space between the two data swarms (Tatsuoka, 1971). "Stepwise" discriminant analysis assumes that we need not bother with all 80 points as there is much redundancy in the data. It finds a small subset of the points such that a DF based on the subset will yield the largest possible separation between the two groups. We proposed in the past (and the simulation results confirm this proposal) that the points selected by the program will indicate the components of the AEP which carry the difference between the experimental conditions.

For the data in Figure 5, the SWDA selected the points indicated by the cross lines. The program defines the coefficients of the linear combination and computes the value of the combination for each of our raw data vectors. This value, then, is a score on the dimension which optimizes the difference between the two groups. It is these scores that we have lately found very useful (Donchin & Herning, in preparation).

In Figure 6 we present the results of a simulation where we synthesized

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Figure 6

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the signal shown in the top row then mixed it with 100 realizations of the noise. The ensemble average of this set is shown on the left. The ensemble average of 100 noise-alone records is shown on the right. We generated nine more sets of the "signal + noise", in each of which the signal is identical to the one shown in the top row, except that we successively reduce

signal amplitude by multiplying it by successively smaller fractions. As signal amplitude is reduced (the noise being held constant), the ensemble average is reduced till it is, by visual inspection, virtually indiscriminable from the ensemble average of the noise set. What we have simulated, then, is a "threshold" experiment, in which the subject is presented with successively smaller intensity stimuli, each evoking a smaller AEP, till the stimulus intensity no longer evoking an AEP is found.

We develop a DF using the two data sets in the top row as our "template" set. We compute the value of the function for the two traces in this top row. These two values are of course, as far apart as they can be, given the original data. In Figure 7 we plot the values thus obtained by applying

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Figure 7

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the DF to the other 8 sets, against the amplitude of the signal. Clearly, the value of the computed DF is a function of the amplitude of the signal. This, of course, is not surprising, we synthesized the data to obtain this result. The logic is, however, important. Given a template using two AEPs which are clearly different--we develop a DF which allows us to evaluate the degree to which <u>other</u> AEPs differ--in terms of the dimensions defined by our template set. Let me emphasize--what I propose is that AEP differences should not be evaluated in absolute terms, but rather in terms of other AEPs, whose differences are known to us on an a priori basis.

We conducted, incidentally, a study in which we tried to duplicate this result with real data. Subjects were presented with flashes of decreasi intensity and the AEPs are shown in Figure 8, each elicited by a flash of

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Figure 8

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the duration indicated. We also show the average computed over the 500 msec just preceding the flash (as good an estimate of noise-alone as we can get in this context). The scores of the respective AEPs on the DF are shown in Figure 9. The DF values track the duration of the flash. In other words we can use the scores on the DF to infer the duration of the flash! You

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Figure 9

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will note, I am sure, that we are dealing with averages, not with single trials. (We can, of course, apply the DF to the raw records to see if the results make sense. Work of this nature is now in progress in our lab.)

We can now return to P300. Recall the 5 conditions of the experiment I described. Two of the conditions (RAN, ALT) represented what we felt were extremes on a dimension. There were three additional conditions. We used the RAN-ALT data as a template for developing a DF on the basis of which to evaluate the rest of the data. The results are shown in Figure 10.

Figure 10

Again, we plot the DF scores against "experimental conditions". Note the striking similarity between the functions obtained in all 10 subjects. For each subject, the five AEPs are ranked on a dimension defined by the RAN and ALT AEPs. Figure 10 includes interesting control data. When we apply the vertex DF to electroculographic data, no orderly relations are observed thus we cannot account for the vertex data in terms of eye movements. In the column labeled Reaction Time sessions we plot the results obtained by applying the vertex DF to data obtained during sessions which were identical in all respects to the previously described sessions except that the subjects

were instructed to respond to an A by pressing one switch and to a B by pressing another switch. Response speed and response accuracy were stressed during these sessions. Clearly, the differences in P300 we observe during Guess sessions disappear when such a response requirement is imposed on the subject. (This finding, of course, presents a problem of explanation which I shall ignore in this discussion.)

In summary then we, and others, have shown that P300 is an endogenous electrocortical component. Its amplitude reflects, in a rather complex manner the intrinsic cortical processes associated with cognitive activity. Some investigators tend to discuss P300 as if it represents the output of a cortical processor (e.g. a "mismatch detector"). This is a view which I do not feel to be supported by the presently available data. It seems to me safer, and more in line with the available evidence, to consider P300 an index of the useage of a general purpose cortical processor (or subroutine) which is invoked by a variety of cortical programs when its services are required. The P300 can thus be considered a "utilization" measure rather than as the output of a processor.

This view implies that we are more likely to understand the functional significance of P300 if we concentrate on attempts to define the nature of the processing common to all P300 evoking situations. The situation is analogous to one in which a special purpose processor has just been added to a computer. You are told that this processor is transparent to the existing software. You are not told what the processor is in fact doing, however, a utilization meter is available and you can therefore systematically run programs whose nature and role are known. Is there a method whereby one can determine the nature of the processor?

Consider for example the possibility that this processor is either a Floating Point Processor (FPP) or a Display Processor or a Memory-Management Processor. Each of these processors will be invoked by a wide class of programs. Thus, for example Floating Point computations may be called for by a statistical analysis program, or by a program designed to synthesize digital filters and by payroll program. It might not be invoked at all by a program designed to produce a KWIC index of a bibliography. A display processor may be invoked by any program requiring a display, while a Memory-Management processor may be called by all programs whenever they are loaded, to determine the core locations available for storage.

The distinction between the ultimate task of a program and the processing capabilities it invokes while accomplishing its task is important. Most attempts to assess the functional significance of P300 were in terms of the task assigned to the subject. This leads to a search for the common features of the disparate tasks which have shown to affect P300 amplitude. What I am suggesting is that the tasks may be quite different and yet have much in common in terms of the processing needs they invoke. To cite one example--the role of "stimulus uncertainty" has been stressed in the P300 literature. Many of the experiments in which P300 has been demonstrated have used the strategem of varying the probability of the stimuli to enhance P300. If these data lead one to the conclusion that P300 reflects the activity of a mismatch detector, one is hard put to explain why in the Reaction Time experiment described above, an enhanced P300 was associated with stimuli concerning which the subject has no uncertainty. Equally puzzling is the consistent finding that a large P300 is associated with "Hit" trials in a signal detection task but not with false alarms or correct rejections. Hillyard's

proposal that P300 is associated with "affirmative decisions" is more in line with the view proposed here in that it stresses the specific processing task rather than the final outcome of the subject's reaction to the stimulus.

To return to our computer analogy, one method for determining whether our unknown processor is either of the three candidate processors is to design an experiment in which we run a variety of programs which systematically call for differing amounts of floating point processing, display controlling, etc. If we design the experiment cleverly enough, that is, if we feed the computer with programs which systematically and quantitatively call for differing amounts of useage of the different processors, we ought to be able to decide, by plotting the processor's utilization as a function of the processing needs of the programs, what the "nature" of the processor is. (A memory management processor illustrates the possibility that all programs might call upon a processor at specific points in their history, and therefore that it may be necessary to determine in some cases not simply the overall utilization of the processor by various programs but also the specific phase of the program at which the processor is invoked.)

It should be clear from this analogy that one must have hypotheses concerning the nature of the processor before one can proceed to evaluate it. But, given an intelligent hypothesis, it should be possible to make an intelligent choice between alternate possibilities. A similar strategy can be employed in attempts to evaluate P300. It is necessary to develop tasks which differ in the degree to which they require different processing needs of the subject, and then to attempt to determine the extent to which P300 amplitude covaries with these needs.

A hypothesis we intend to evaluate in the immediate future is that P300 is involved in memory search procedures. We will use a task developed

by Sternberg which requires subjects to make specific judgments, which under different experimental conditions will differ only in the amount of memory-search they call for. In this way we can determine the extent to which memory-search is involved in the processor indexed by P300.

In an extension of the experiment described above we intend to analyze in much greater detail the manner in which P300 amplitude depends on the sequence generating rules of stimulus series. We will use Restle's recent analysis of the effect of various sequence-generating rules on the subject's behavior to assess the relationship between these and P300. The advantage of Restle's approach as a framework for our own studies lies in their provision of a well-studied set of behavioral data against which P300 data can be evaluated.

We also intend to use the two-matrix approach developed by Rohrbaugh to further evaluate P300. This situation is particularly useful in that it allows us to assign to each of the two matrices in a sequence differential informational value and to determine how shifts in this informational value affect P300 amplitude. In this, as in the above cases, we shall focus a considerable part of our effort on the development of the discriminant analysis approach to on-line monitoring of the subject's behavioral state.

Specifically our strategy would be to try and perfect a normative DF which will measure on-line the degree to which a stimulus has elicited a P300. This DF will be developed using situations which we, and others, have shown to elicit large P300 differences. This DF will then be applied in other experiments. Through a comparison of behavioral and electrophysiological data we hope to be able to develop a technique where the computer could decide, on a trial-by-trial basis, whether a given stimulus elicited a P300. Even now such a determination can be interpreted as indicating that the

subject is processing the information delivered by the stimulus. As we perfect our understanding of P300 such determinations could be interpreted in a more specific form.

We also intend to follow up in some detail on a research strategy employed in several of our earlier studies. In these studies we presented subjects with a stimulus configuration in which a variety of different aspects varied simultaneously, though asynchronously. We have shown (Donchin & Cohen, 1967; Donchin & Smith, 1970; Smith et al., 1971) that only changes in that aspect of the stimulus on which the subject was focusing attention elicited a large P300. One could infer therefore from P300, to which of a complex of stimuli a subject was paying attention. In the previous work we could vary only two different stimulus features at a time. In the present project we will have the capability to vary several such aspects of the display. We could thus determine the degree to which P300 can be used to determine to which aspects of a complex information display a subject is "paying attention" to at any time. We envisage an arrangement whereby a computer generating complex information displays could assess the degree to which the subject is accessing information the computer "considers" relevant. It will also have presumably the ability to prod the subject, ever so gently, to look where he ought to.

In summary then, our goals within the framework of this project are to develop simultaneously our understanding of P300 and its utilization as a basic monitoring tool in man-machine interactions. The understanding will depend on the development of experiments in which monotonic changes in the complexity of cognitive tasks are related to P300 amplitude in an attempt to determine its psychological determinants. To obtain a monitoring technique

we will develop a normative DF, or a series of normative DFs, each defining a dimension of cognitive processing, and each used to locate a subject's present cognitive activity on the dimensions in question.

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Figure legends

Fig. 1. Stimulus matrix used for generating stimuli in Rohrbaugh's (1973) study. Each square indicates a plexiglass cube that could be illuminated. All squares labeled with the same number were flashed simultaneously. (From Rohrbaugh 1973)

Fig. 2. Average evoked potentials from four subjects. Each trace represents one evoked response associated with the presentation of a pair of 9 square matrices, using the stimuli of Fig. 1. The pairs are separated by the interval in msec in the center column. The traces are aligned on the second matrix (vertical line) and are the presentation of the first matrix as indicated by the arrow. Solid lines - horizontal and vertical lines presented in RAndom sequence. Dotted lines. Horizontal and vertical lines alternated. Two left column recorded from occiput. The two right columns from vertex. Link vertical calibration-10 microvolts. Horizontal calibration 500 msec. Negativity up. (From Rohrbaugh, 1973).

Fig. 3. Average evoked responses recorded from one subject during five conditions. In each condition a tone was presented at the second vertical line (from the left) and either an A or a B were flashed 1400 msec later (at the third line). The fourth line identifies the interval 300 msec after the flash. The subject had to guess prior to the tone which letter will be flashed. The five conditions differed in the rules which determined the sequence of the letters and in the information the subject had concerning these rules. See text for details.

Fig. 4. Same vectors generated by the simulation study. As labeled the vectors shown represent a signal vector, realizations of the noise process and a summation of the signal and the noise.

Fig. 5. Two "average evoked responses" each based on averaging an ensemble of 100 synthesized signal+noise vectors. The cross lines indicate the location of the variables selected for discriminating between the two vectors.

Fig. 6. Synthesized AEPs used for simulating an experiment on the effects of stimulus intensity on the evoked responses. The upper two traces represent a signal vector and a corresponding noise vector. Similar noise vectors are shown in the right column. Successive traces in the left column represent thesuccessive reductions in the amplitude of the signal.

Fig. 7. The results of applying SWDA to the data of Fig. 6. The two top traces in that figure were used as a template for computing the Discriminant Function. The values of this function applied to all the other synthetic AEPs are plotted against the corresponding signal amplitude.

Fig. 8. Average evoked potentials recorded from one subject using flashes of increasingly briefer duration as indicated in the center column. The right column presents averaged EEG activity for the period just prior to stimulus presentation. The right column the activity just following stimulus presentation. The two top traces were used to compute the discriminant function.

Fig. 9. The scores of all traces from Fig. 8 on the discriminant function developed on the basis of the records associated with the 12.8 msec stimulus. The discriminant scores are plotted against flash duration.

Fig. 10. For each of ten subjects a discriminant function was developed using the data obtained in the ALT and RAN trials and applied to the data obtained in the other three conditions. The left column plots the results for the 10 subjects during a "guess" condition, that is when the subject predicts which flash he will receive. The right column presents the discriminant scores for the same function applied to data obtained when the subject is responding to the flashes. See text for details. (From Donchin et al. in press).

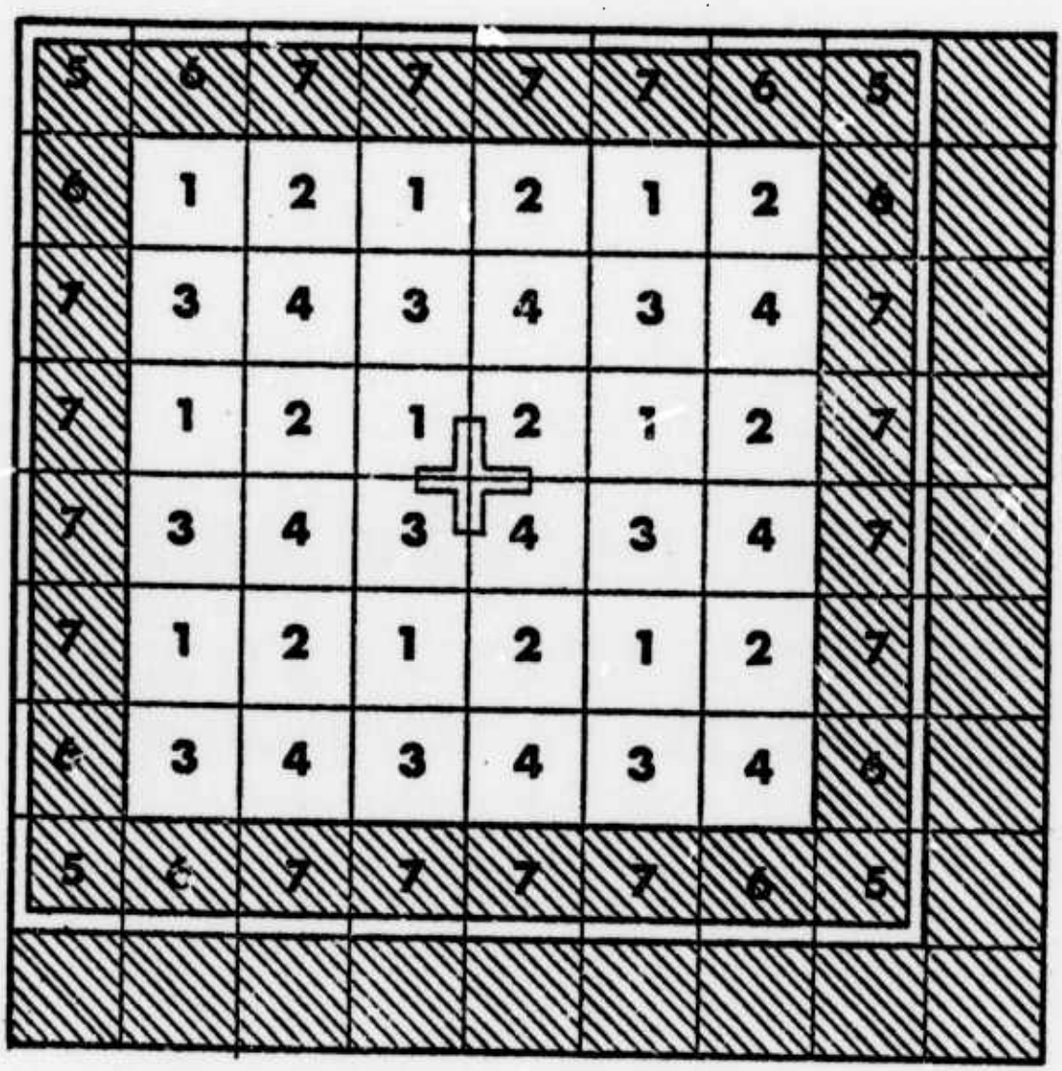
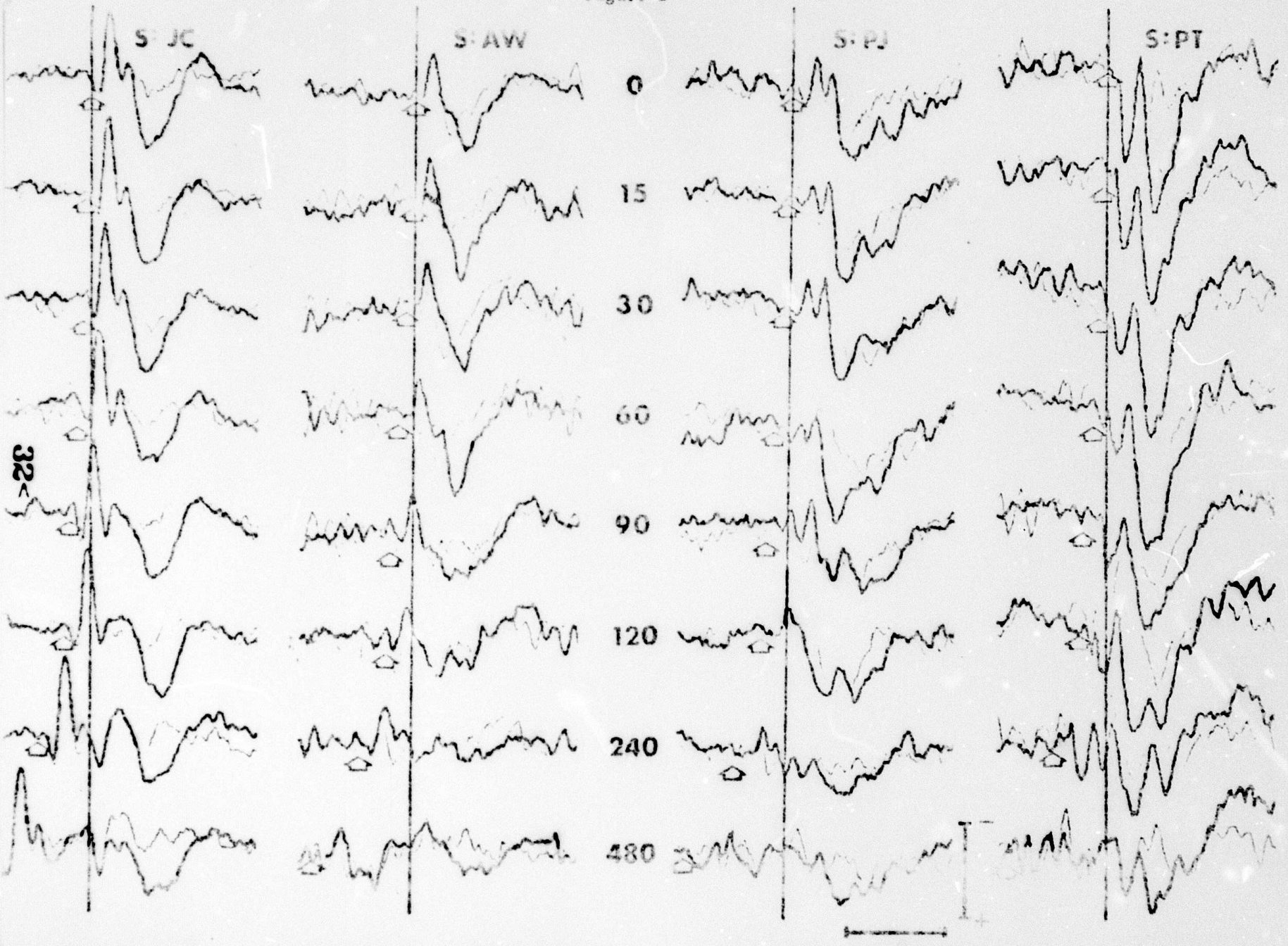

5 6 7 7 7 7 6 5
6 1 2 1 2 1 2 6
7 3 4 3 4 3 4 7
7 1 2 1 2 1 2 7
7 3 4 3 4 3 4 7
7 1 2 1 2 1 2 7
6 3 4 3 4 3 4 6
5 6 7 7 7 7 6 5

Figure 1

Figure 2



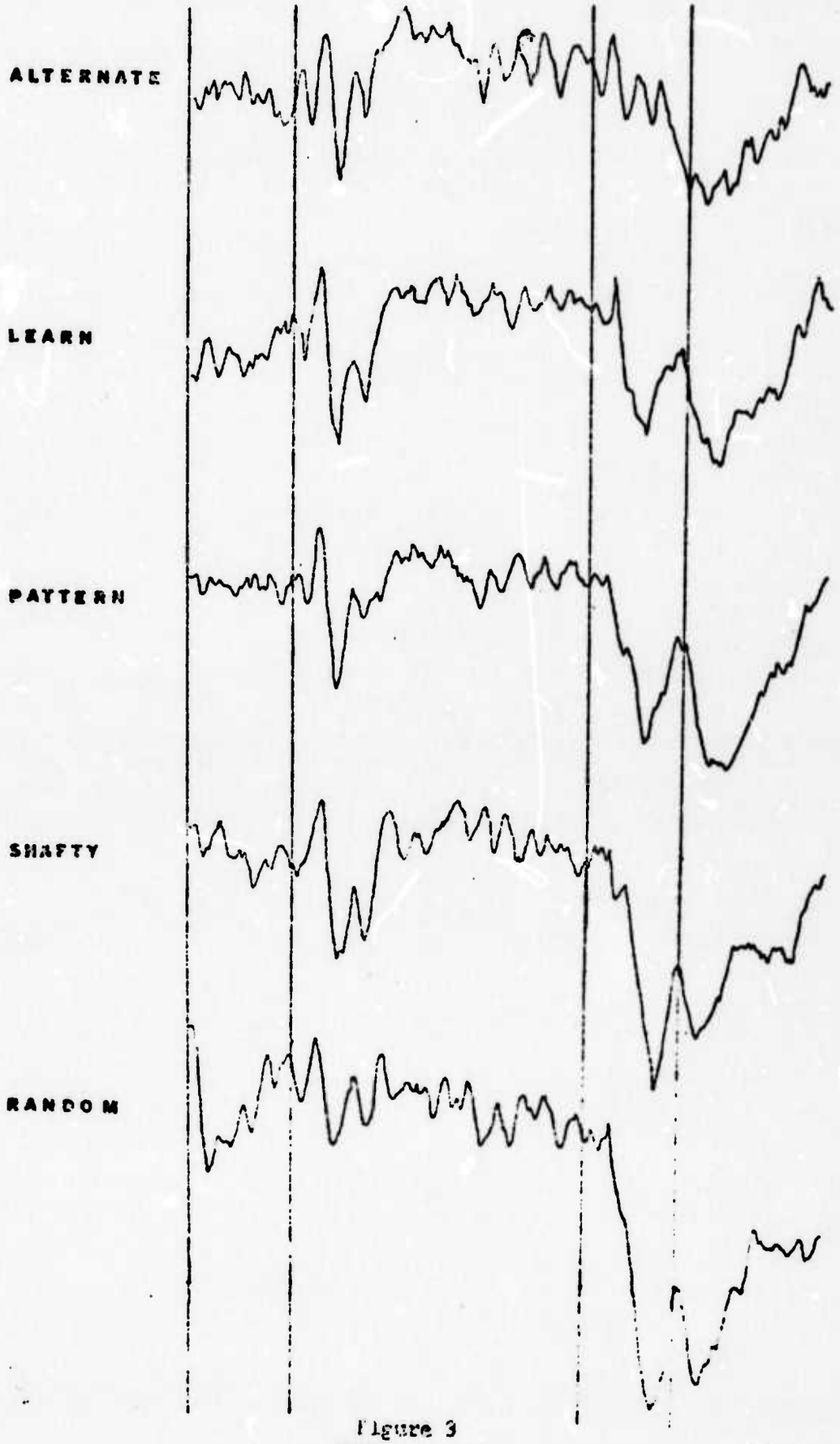


Figure 3

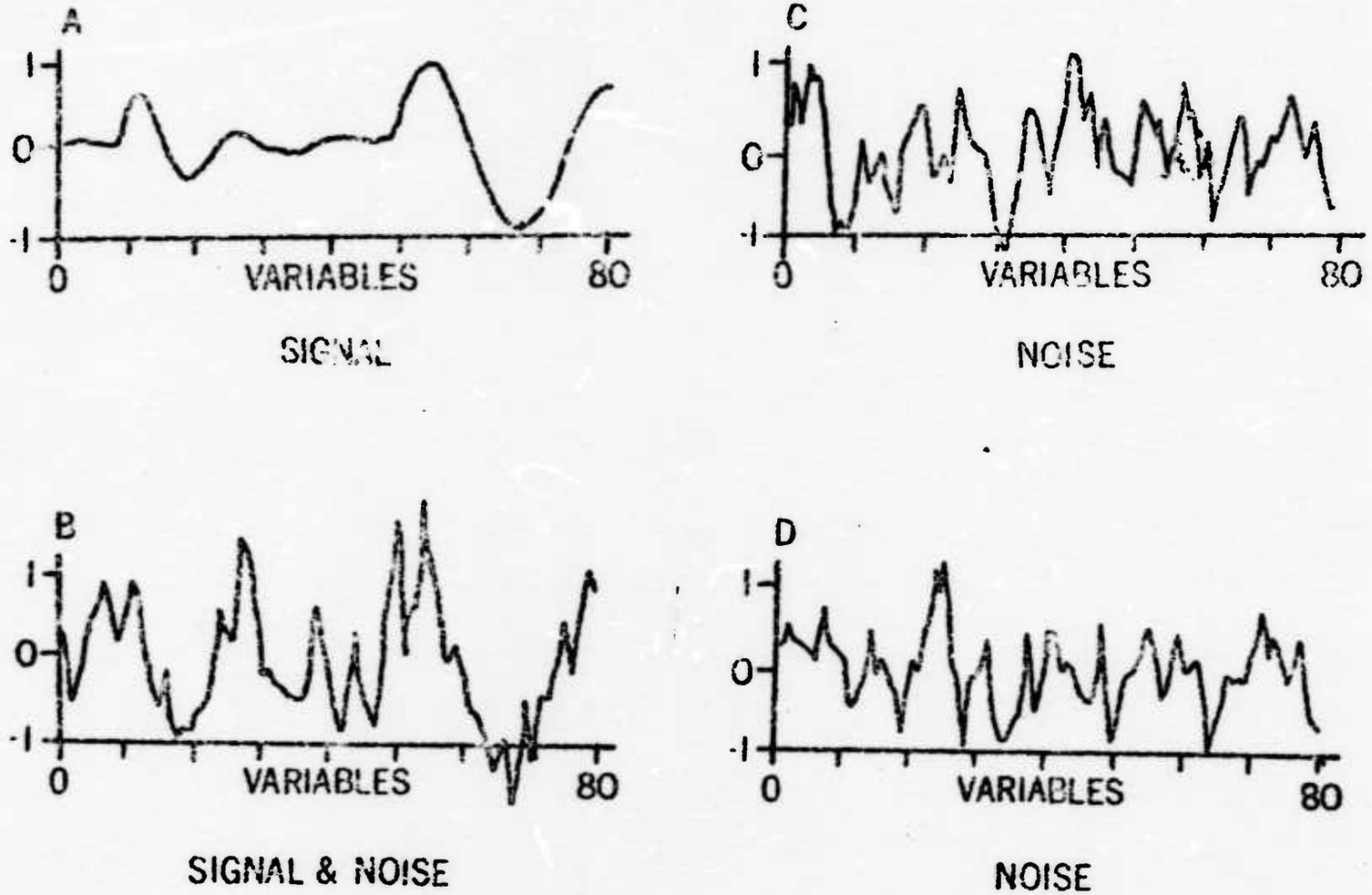

A
1
0
-1
0
VARIABLES
80
SIGNAL
C
1
0
-1
0
VARIABLES
80
NOISE
B
1
0
-1
0
VARIABLES
80
SIGNAL & NOISE
D
1
0
-1
0
VARIABLES
80
NOISE

Figure 4

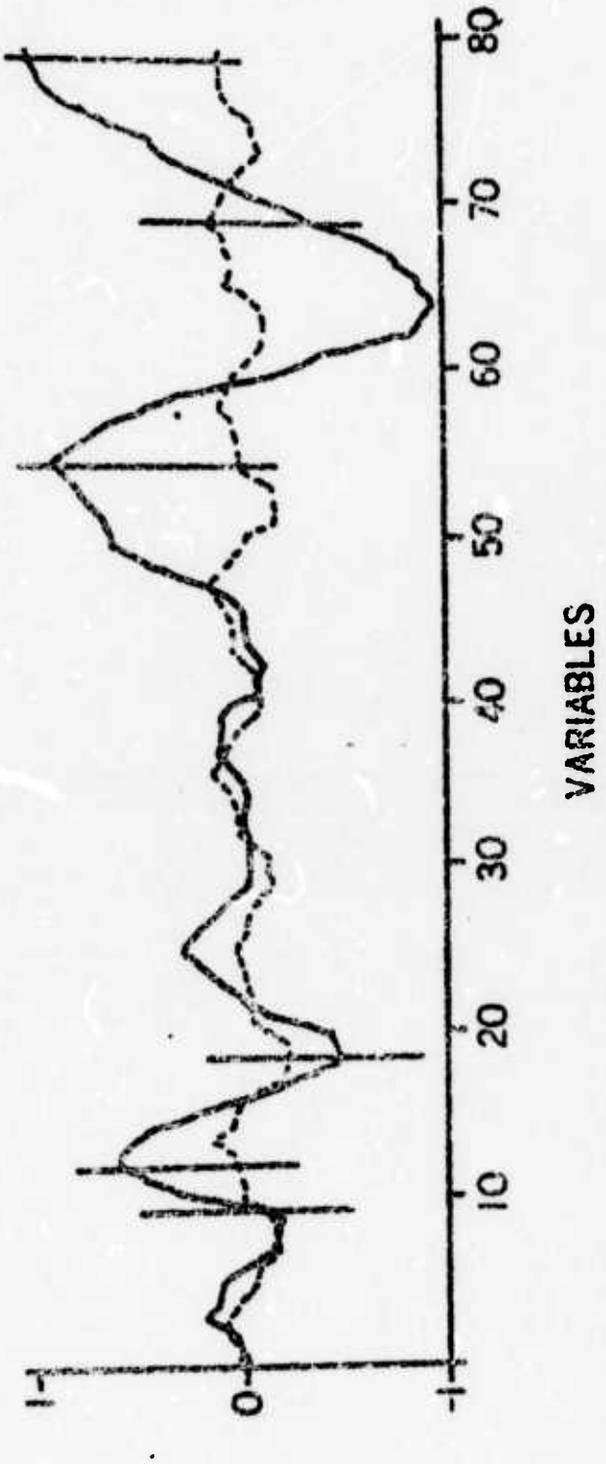


Figure 5

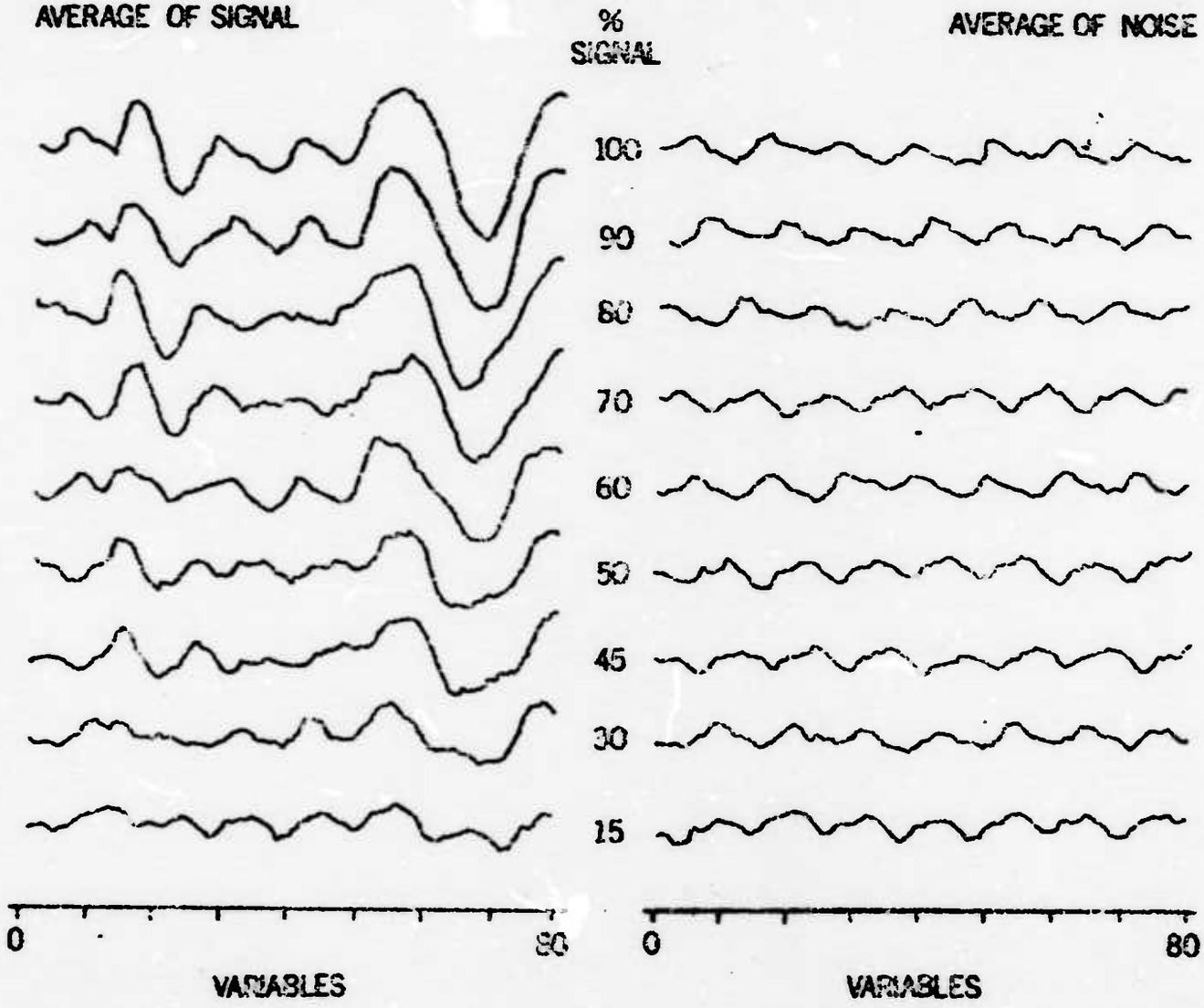

AVERAGE OF SIGNAL
% SIGNAL
AVERAGE OF NOISE
100
90
80
70
60
50
45
30
15
0
80
VARIABLES
0
80
VARIABLES

Figure 6

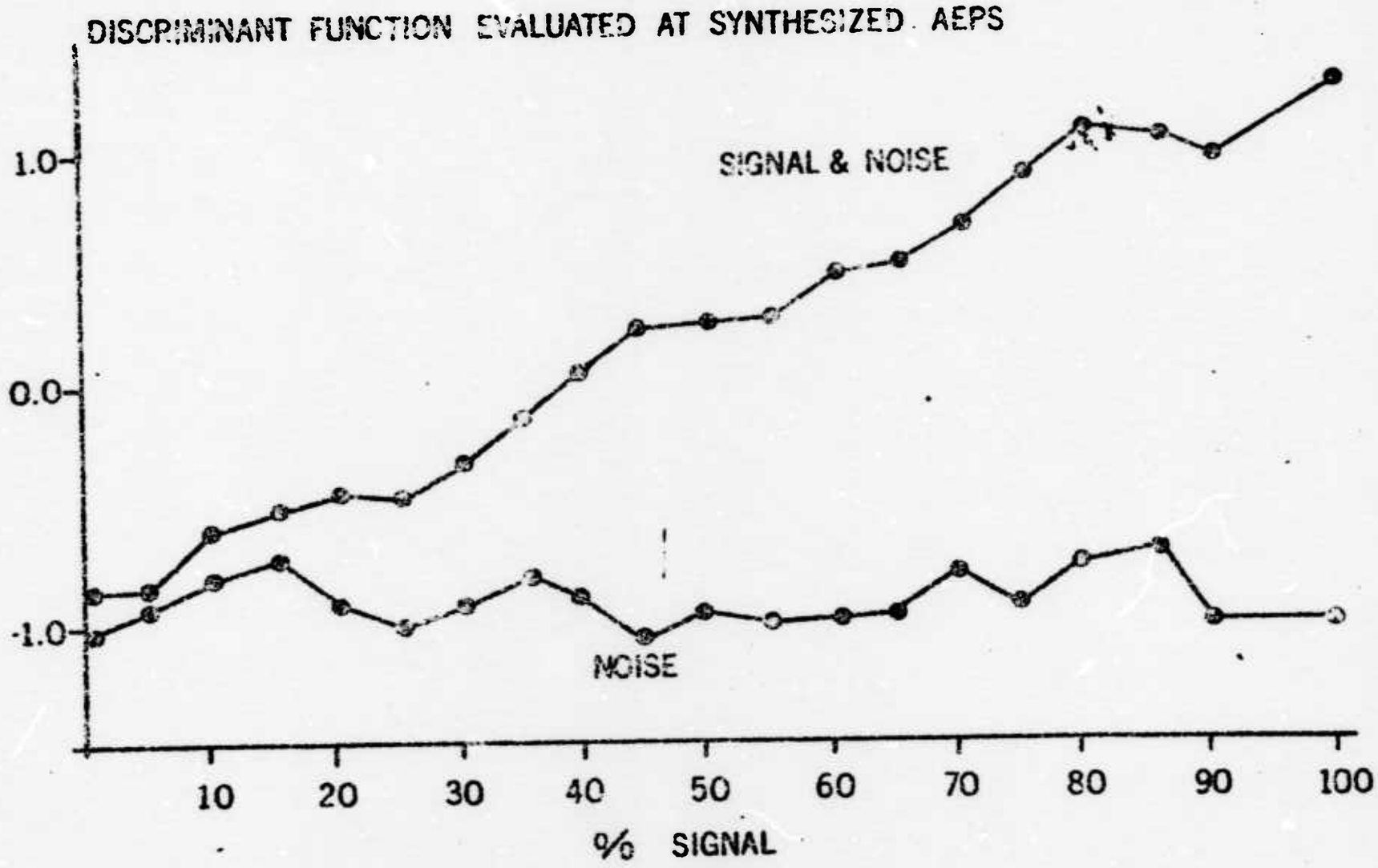

DISCRIMINANT FUNCTION EVALUATED AT SYNTHESIZED AEPS
1.0
0.0
-1.0
SIGNAL & NOISE
NOISE
10
20
30
40
50
60
70
80
90
100
% SIGNAL

Figure 7

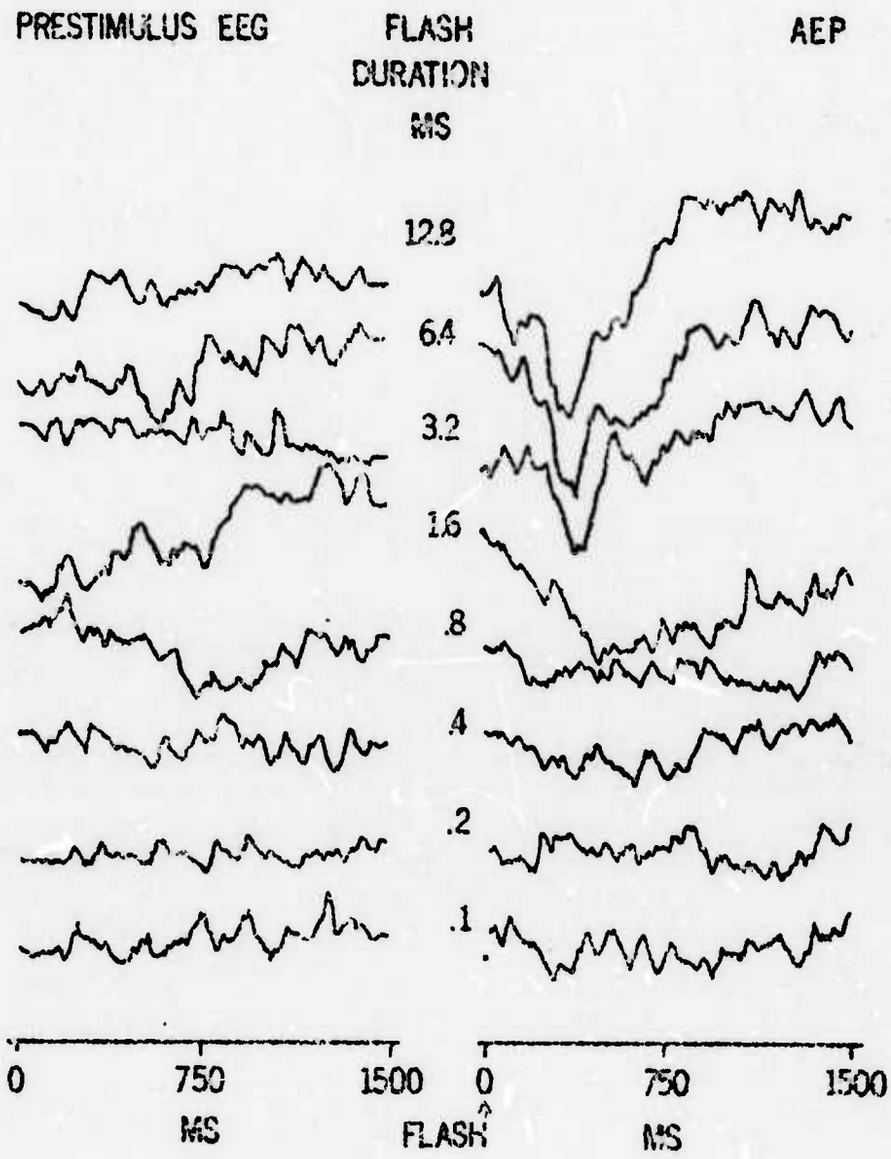

PRESTIMULUS EEG
FLASH DURATION MS
AEP
12.8
6.4
3.2
1.6
.8
.4
.2
.1
0
750
1500
MS
FLASH
0
750
1500
MS

Figure 6

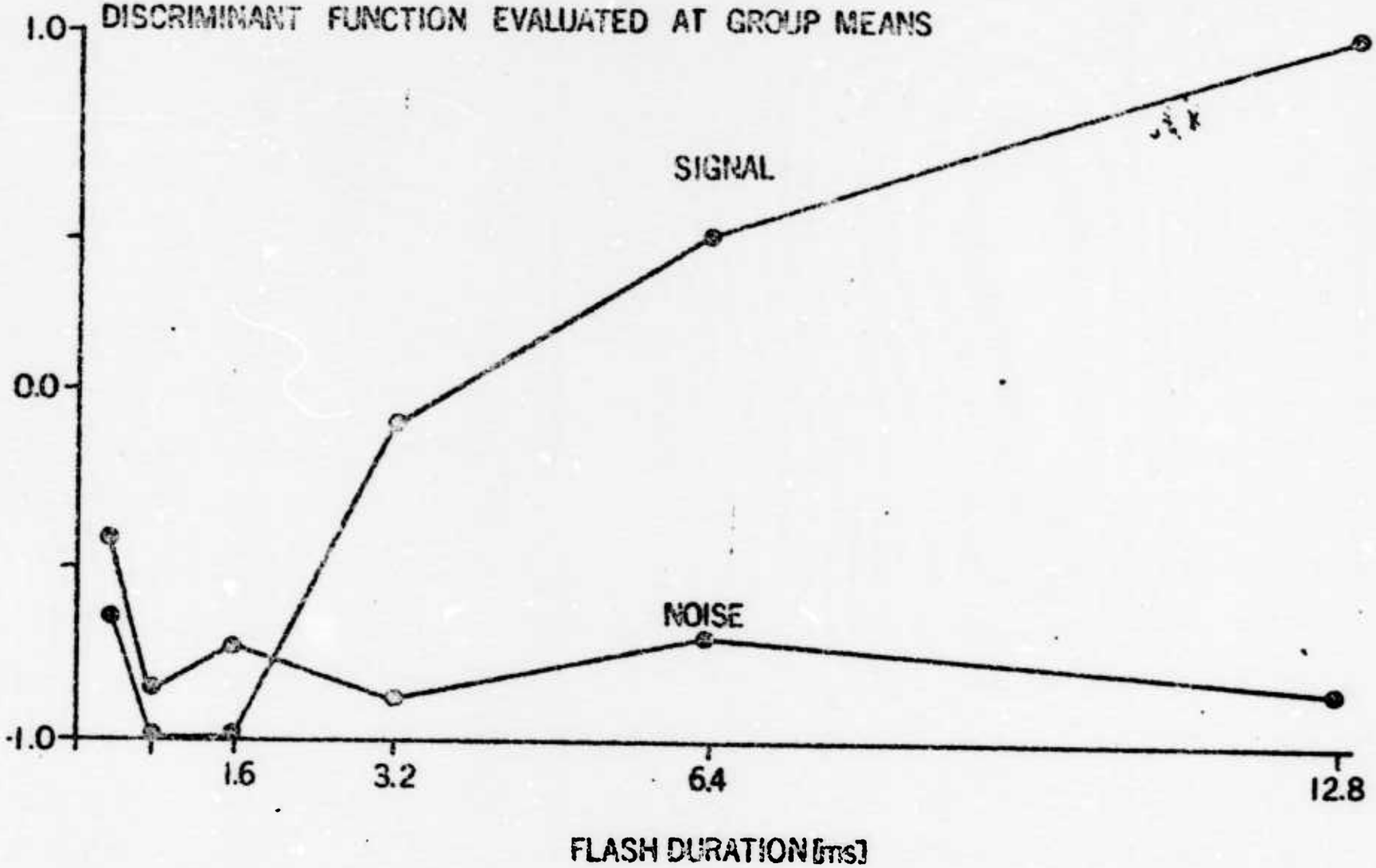
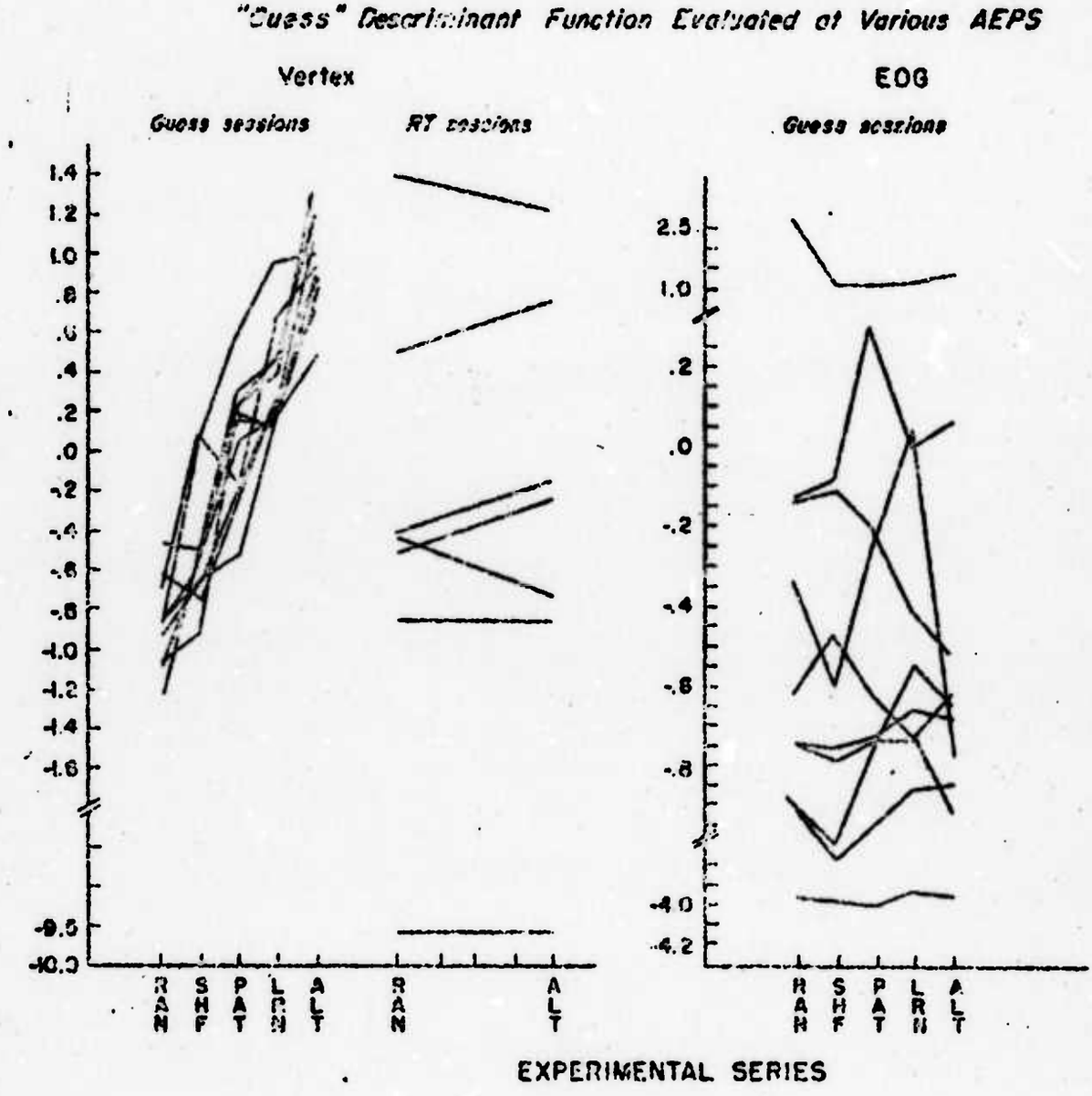

DISCRIMINANT FUNCTION EVALUATED AT GROUP MEANS
1.0
0.0
-1.0
SIGNAL
NOISE
1.6
3.2
6.4
12.8
FLASH DURATION [ms]

Figure 9


"Guess" Descriminant Function Evaluated at Various AEPS
Vertex
EOG
Guess sessions
RT sessions
Guess sessions
1.4
1.2
1.0
.8
.6
.4
.2
.0
-.2
-.4
-.6
-.8
-1.0
-1.2
-1.4
-1.6
-9.8
-10.0
2.5
1.0
.2
.0
-.2
-.4
-.6
-.8
-4.0
-4.2
RAN
SHF
PAT
LRN
ALT
RAN
ALT
RAN
SHF
PAT
LRN
ALT
EXPERIMENTAL SERIES

MEASUREMENT IN AEP STUDIES

by

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I shall take as my topic the problem created by the need to combine the data obtained from different subjects used in an experiment. The problem is two fold: how do you combine the data so that you know what the results were? A related, but quite distinct problem is that of communicating your conviction to others in a paper with a restricted number of pages and figures. I think you will all agree that this is a vexing problem in average evoked potential (AEP) research. Of course, it is the case that in all psychological experiments one records data from several subjects. Ordinarily, no special problems are created by this. Specific measures are obtained from each of the subjects. These measures will be somewhat variable but the investigator has at his service a large repertoire of statistical techniques devised to extract stable relationships from variable data. The crux of the matter however is that in most other areas the measures obtained from each subject are uniquely and clearly defined. The investigator uses a set of clearly specified operations from which his numbers are obtained. All the numbers obtained from all the different subjects can then be considered as being the results of an equivalent measurement operation. In AEP work we record from our subjects, using clearly defined procedures, a very complex waveform. While we can define the proper techniques for satisfactorily recording these waveforms, having recorded them we are faced with a new measurement problem. We must define some specific, consensual, methods for reducing these complex waveforms into uniquely defined values which can then be handled by the normal conclusion-deriving techniques available to most investigators.

Our problem is severe because any individual's AEP waveform tends to be quite distinct. The relationship between peaks and troughs, the relative amplitude of different evoked response components, the curvature of the

waves and the steepness of the slopes leading from one component to the other all vary from one subject to the other. There is, of course, considerable intersubject consistency. Complex and different as the waveforms may be, most AEPs elicited by visual stimuli, of moderate to bright intensities, will have a negative peak with a latency between 75 to 120 milliseconds, a positive peak between 90 and 120 milliseconds another negative peak often about 160 milliseconds after the stimulus and so on. The effect of the experimental variables is often quite consistent over the subjects. Similar components are affected in a similar way by the same experimental variables. Nevertheless, the subject's evoked responses will be sufficiently idiosyncratic to make it difficult to decide how to develop specific measures of the characteristics of the components. In other words it is difficult to decide how to combine these data.

Several approaches have been used to this problem. Often, one attempts an intuitive conclusion based on visual inspection of each subject's data. This is often very effective within the confines of one's laboratory. One can view at leisure all the data obtained from all the subjects, superimpose different AEPs in a variety of different combinations and quite often a clearly perceptible pattern appears in the data. However it is difficult to communicate such conclusions convincingly. Ordinarily only one subject's data are presented in public, often accompanied by a statement that "the rest of the data were quite similar." I suspect most of us remain unconvinced by such statements. Alternately, some unique characteristic of the AEP, such as a component's amplitude or latency are measured, these numbers are then used as measures of the data. We are then presented solely with an analysis of variance, with no real feel for what the data in fact looked like.

Another approach used by several investigators is to pool all the subjects data together. Thus one computes an AEP over all subjects for each of the experimental conditions and the number of complex waveforms one must then digest is drastically reduced. In these opening remarks I shall illustrate these procedures, and discuss an alternative approach we have found quite effective.

Consider Figure 1a., these data recorded from five subjects are discussed in detail in Donchin et al. (1973). We claim in that paper that the

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Insert Figure 1 here

AEPs represented by the dark and by the light lines are different in one experimental condition (Guess), and are the same in another condition (RT) and our thesis is that the difference lies primarily in the amplitude of the P300 component of these two AEPs. That this is the case for these five subjects is indeed quite obvious, but the subjects' data are variable. If we choose to measure, say, just the peak amplitude of P300 and study those measures it is quite obvious by inspection of the data that it would be difficult to devise a simple rule for selecting which peak should be measured in each subject. Do we measure amplitude exactly 300 milliseconds following the stimulus? Do we measure the peak amplitude wherever it is in time? Do we integrate the area under a base line? How would we define a base line if we wish to do so? Thus the complexity of the individual subjects' AEPs is a substantial hindrance to the development of simple measures of AEP characteristics.

In Figure 1b we present the data from the same experiment. This time, the AEP was computed over 10 subjects. Of course the data are now far

less variable. The results stand out more clearly and we can summarize many different evoked responses in one figure. One might argue that as AEPs of the individual subjects show consistent differences it is permissible to average "over the subjects." The variability of the individual waveforms is "measurement error" of which we are best rid. This is a reasonable position. It does, however, present a problem. By pooling all the subjects' data into AEP we loose any indication of the variability in our data. Differences, however, must always be evaluated with respect to variability. A difference between means that is no larger than the variability inherent in the data is not really very interesting. Thus one difficulty with computing cross subjects' AEPs is that they lack an accompanying measure of variability.

Such a measure is inherent in discriminant analysis. It is essentially a pattern recognition approach, not different in its underlying logic from the Recognition Index-technique described by Weinberg et al. (this volume). All pattern recognition techniques involve the development of a decision rule. The decision rule is applied to the data and a score is computed. A score larger than a criterion value leads to one decision, a score below the criterion value leads to the alternate decision. Pattern recognition techniques differ in the way in which they compute these scores. They also differ in the logic underlying the selection of the criterion values. The advantage of discriminant analysis (DA) as a pattern recognition technique is that it has a fairly formal and well defined manner for defining the decision rules and the criterion values. For a detailed discussion of this technique see Donchin and Herning (in preparation).

In Figure 2 we present the results of applying DA to the data recorded in the Donchin et al. (1973) experiment. There are three panels each with

Insert Figure 2 here

ten lines. In the left panel the lines are parallel and grouped, in the other two panels the lines diverge. The figure summarizes the entire data recorded in the experiment. To explain the technique I shall describe a simulation study of evoked potential data which we have just completed, (it is described in detail in the paper by Donchin and Herning referred to above).

We generated synthetic AEP waveforms by summing five damped sinusoids, thus producing an AEP with five independent components, each of which could be independently manipulated. These evoked responses were embedded in "noise" generated by a Gaussian random process. We varied the noise characteristics: its frequency, its power, etc. We were thus generating different synthetic experiments in which specific evoked response differences embedded in specifically defined noise were generated. We then applied the DA technique to the data and determined if it correctly assayed the differences we have introduced into the data.

Consider for example the simulation of an "intensity" experiment. We generated the two waveforms shown in the top row of Figure 3a. On the left we see the result of averaging one hundred synthetic records each containing an "evoked response" mixed with noise. On the left we show the

Insert Figure 3 here

average of one hundred similar noise processes in which an evoked response was not embedded. We repeated this process several times each time reducing

the amplitude of the evoked response embedded in the noise. We obtained as expected a signal waveform which gradually disappears from the average evoked response.

How does one use the DA for analyzing this data? The approach starts with known differences. We assume that we have experimental conditions which on a-priori grounds should produce different evoked responses. These known-difference conditions are used as a base-line in terms of which the rest of the data are described. Thus we will not be measuring data simply in terms of specific physical units. Rather, each average evoked response is described in terms of two preselected AEPs representing conditions which in some sense define a dimension along which AEP data are supposed to vary. That is we say, all the differences we are interested in are either larger or smaller than the difference which on a-priori grounds we know to be as large as possible. Discriminant analysis is well suited to this approach. For example, in an intensity experiment we know that the stimuli of maximal intensity should yield the largest possible differences between the AEP and an average of ongoing EEG. Data obtained with other stimuli can be described in terms of a template which is derived from the two curves representing the maximal intensity AEP and the ongoing noise.

The program selects six of the 80 time points for use in developing the discriminant function. If we record the EEG at these six time points, apply six coefficients provided by the program, and then add the result of these six multiplications we obtain a score. This is our decision score. In Figure 3b we present the results of applying this discriminant function to the simulated data shown in Figure 3a. Each of the lines begins on the right with the score computed for the template condition, clearly the score for noise alone is quite different from the noise score for signal noise.

The rest of the scores were computed for data in which the signal was reduced by the percentages indicated on the abscissa. The score thus provides a good measure of the intensity of the signal. In other words, we have found a way of measuring the characteristics of an evoked response which immediately reflect the aspect of interest in the data. This technique provides a consideration of variability because the scores can be computed for each of the individual trails recorded in an experiment. The distribution of the scores provides all the information we need on the variability in the data.

That this technique works with simulated data is of course not very surprising. We need to show that it works as well with real, as it does with manufactured, data. We illustrate this by Figure 4. In Figure 4

Insert Figure 4 here

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we plot a series of evoked responses elicited by flashes with flash duration ranging from 12.8 to .100 milliseconds. As expected, as the brightness of the flashes is reduced, the AEP amplitude is diminished. We use the EEG recorded just prior to the presentation of the stimulus to represent "noise" records. We developed a discriminant function using the EP elicited by the 12.8 millisecond flash and the corresponding prestimulus EEG records. We applied the discriminant functions so obtained to the rest of the data. The results are shown in Figure 4b. Again the scores under the discriminant function act as good representatives of the data.

We can, now, return to Figure 2. The points plotted there are scores on a discriminant function developed using two AEPs, one obtained when the

subject was operating under conditions of uncertainty the other when the subject was operating under conditions of certainty. The scores of these two conditions, for the AEP recorded at a vertex electrode, are represented by the 2 points on the extremes of each of the lines on the left panel. These scores were computed on the basis of the averages we show in Figure 1. Our principal goal in this experiment was to determine if the conditions represented by the other three points on each line would be ordered in a reasonable fashion between the two extreme conditions. They obviously were, as discriminant scores indicated. In the other two panels we show how the introduction of a reaction time requirement into the same experiment eliminates the orderliness in the data and we also show that electrooculographic data obtained simultaneously with the vertex data in panel one fail to show the orderliness.

We thus see that in the stepwise discriminant analysis technique we have a very useful and powerful technique for measuring the characteristics of evoked responses, for summarizing the data obtained in an experiment in a meaningful way and for relating the between-subject and within-subject variability to our conclusions. The use of the technique is supported by a large body of statistical theory and the behavior of the statistics thus obtained under many different conditions can be derived.

I have been advocating for some time the use of multivariate statistical techniques in the analysis of evoked response data. To me it seems self-evident that as the AEP is a multivariate observation it is foolish not to take advantage of the developments of multivariate statistical analysis over the last few decades. Oddly, many evoked response investigators persist in handling the data as if they were recording an assortment of univariate observations. Various strategems are developed for combining

basic univariate statistical tests in ways which look intuitively satisfactory. However the combination of univariate tests into one multivariate test is essentially the task of multivariate analysis. There is a theory behind such combinations and proper usage of the results depends on clear and proper understanding of the theory. It seems to me that rather than develop a variety of palliative measures derived from the textbook statistics we were brought up on, it would behoove us to make use of the more advanced, more appropriate statistical techniques.

I realize that to some extent the reluctance to use multivariate techniques derives from the instrumentation requirements that may be imposed by such techniques. Clearly, it is a condition for such use that the investigator have all his data, trial by trial, in a computer compatible form so that he could use the data as input to statistical packages available at most University computer centers. Those of us who must make do with computers of average transients, or with small laboratory computers, with no access to tape facilities are forced to think about alternative procedures. This will probably be the case for some time to come. For those who can afford to obtain data in computer compatible format I should point out that discriminant analysis, and many other statistical techniques, are available in several easy to use statistical packages. I, for one, have used the BMD packaged developed at UCLA for the discriminant analysis described above. This package is readily available and other than some preformating of the data, and the preparation of a few control cards, it can be used with ease.

Reference

Donchin, E., Kubovy, M., Kutas, M., Johnson, R. Jr., and Herning, R. I. Graded Changes in Evoked Response (P300) Amplitude as a Function of Cognitive Activity. Perception and Psychophysics, 1973, 14, 319-324

FIGURE LEGENDS

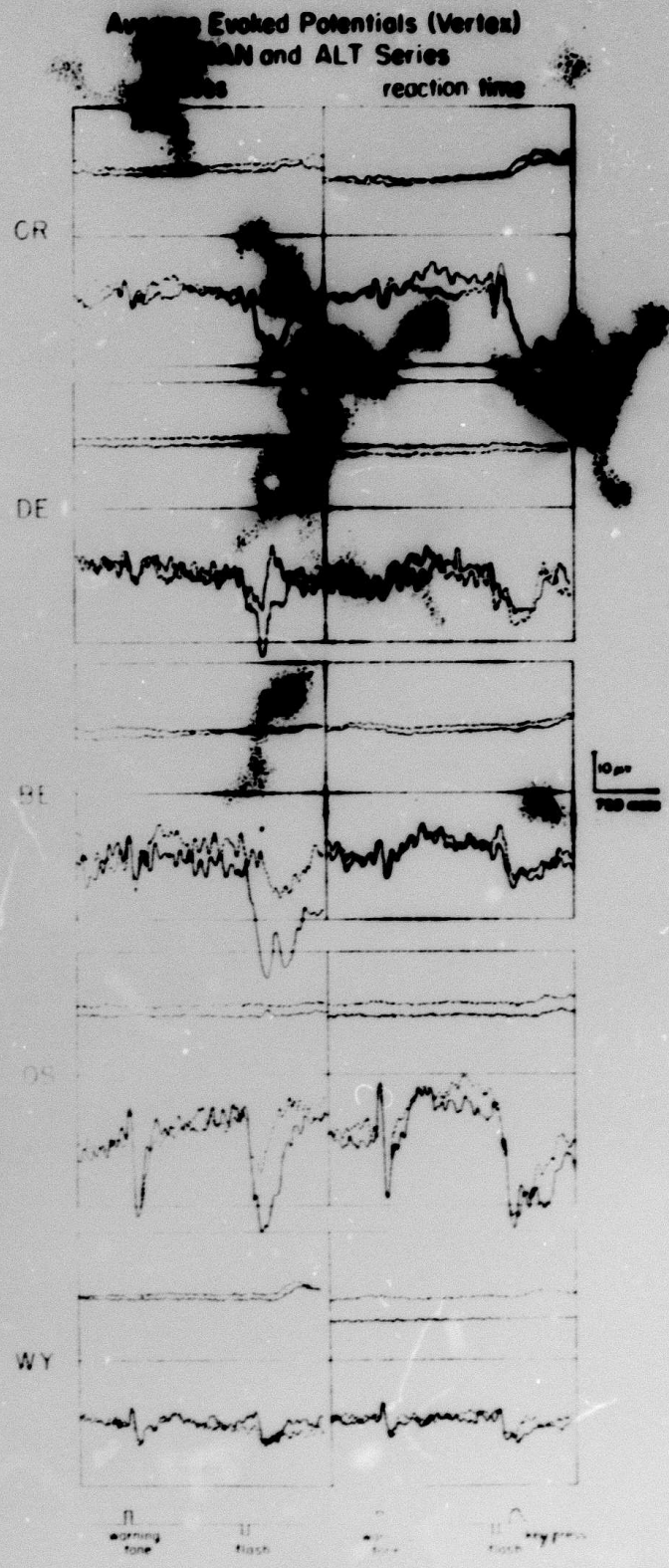
Figure 1. A) Average evoked potential recorded from five subjects in two different experimental conditions "guess" and "reaction time". The lower records in each panel were recorded at the vertex, referenced to a linked mastoid, the upper records are of EOG. The solid line was recorded in one experimental series, the dotted line recorded in different experimental series. The principal prediction of the experiment has been that a difference between the solid and the dotted line will be observed in the "guess" condition, and will be localised to the P300 component. B) Data from the experiment described for Figure 1A averaged over 10 subjects. Here the comparison between superimposed records is between the occipital and the vertex electrode. (From Donchin et al., ~~Perception and Psychophysics~~, 1973.)

Figure 2. Discriminant scores applied to the evoked responses shown in Figure 1B. (From Donchin et al., ~~Perception and Psychophysics~~, 1973.)

Figure 3. A) Simulated evoked potential "intensity" experiment. Each of the records in the left panel is a synthetic evoked response obtained by adding five damped sinusoids. The characteristics of the sinusoids are identical for all records in the left panel however the synthetic evoked response was successively multiplied by a fraction, to reduce its overall amplitude. On the right we show the average of 100 realizations of a noise process with which the synthetic evoked response mixed. A discriminant function was developed on the basis of the data

obtained for the full amplitude signal and the corresponding noise realizations. B) The discriminant scores for the synthetic AEP shown in the previous figure. These are plotted against the relative amplitude of the signal.

Figure 4. A) Visual evoked responses elicited by flashes with duration ranging between 12.8 and 0.100 milliseconds. In the left column we show ongoing EEG activity just preceding stimulus onset. A discriminant function was developed on the basis of the data elicited by the 12.8 milliseconds stimulus. B) Discriminant scores for the data shown in Figure 4A, plotted against flash duration.

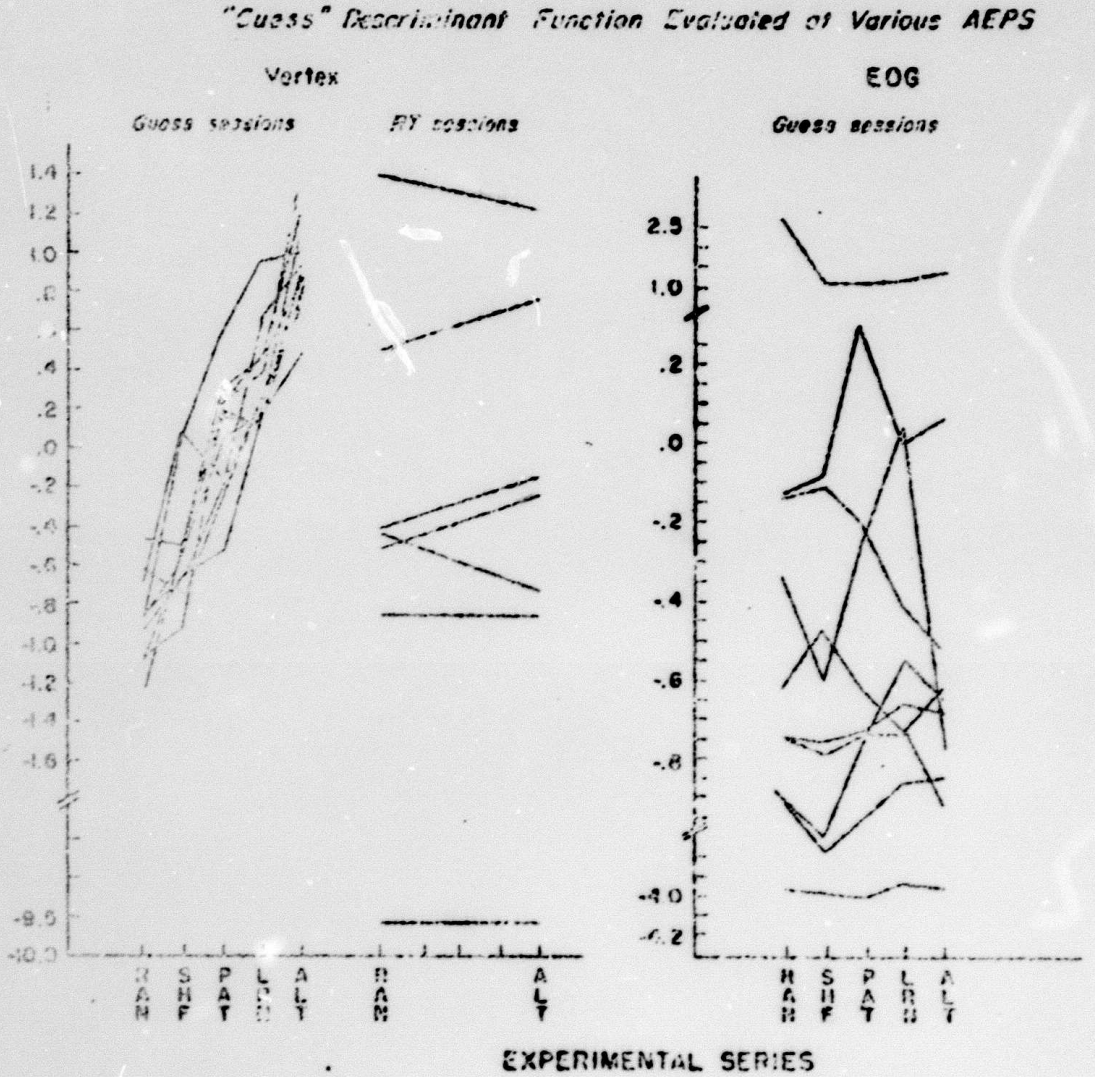


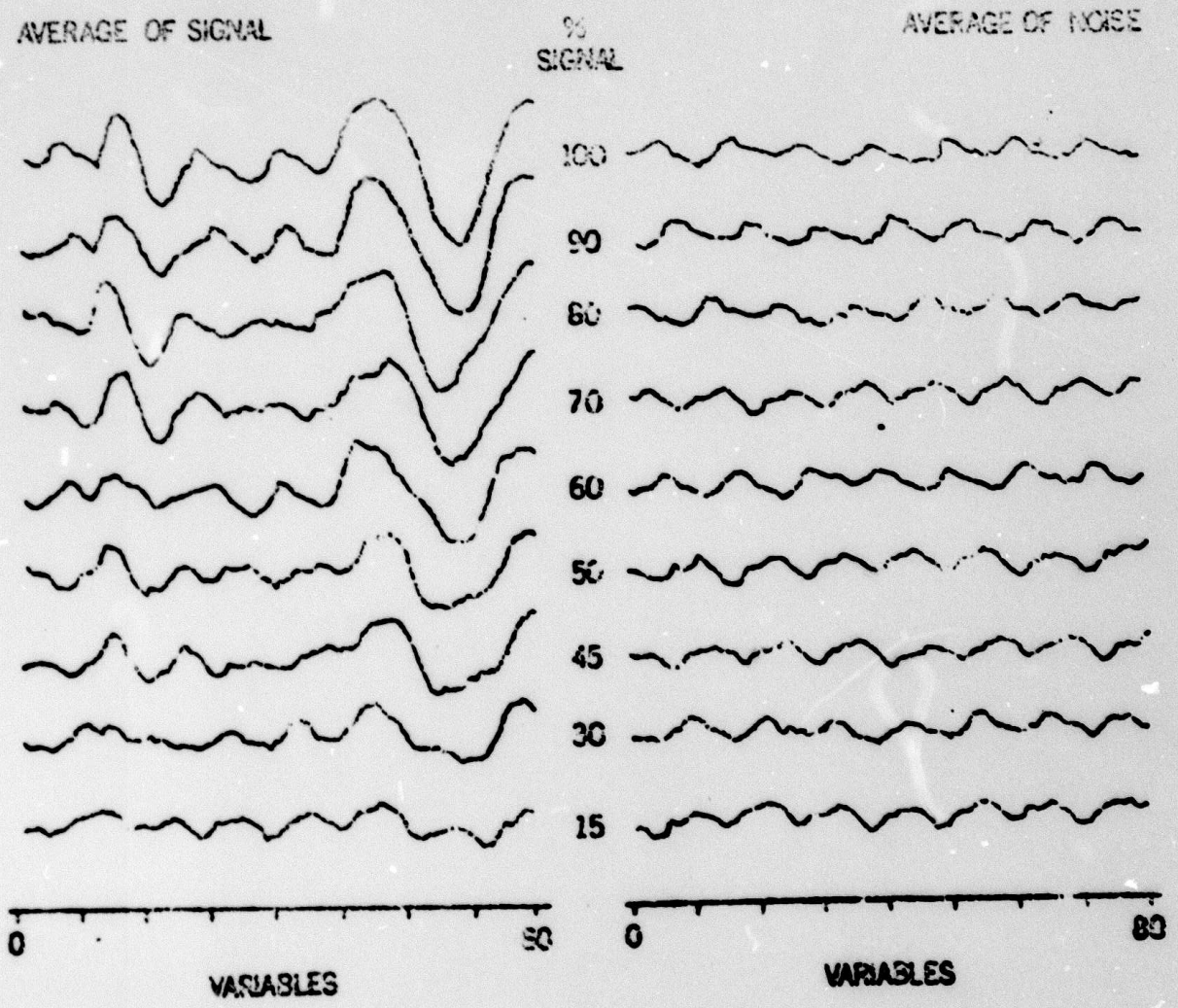
Reproduced from best available copy.

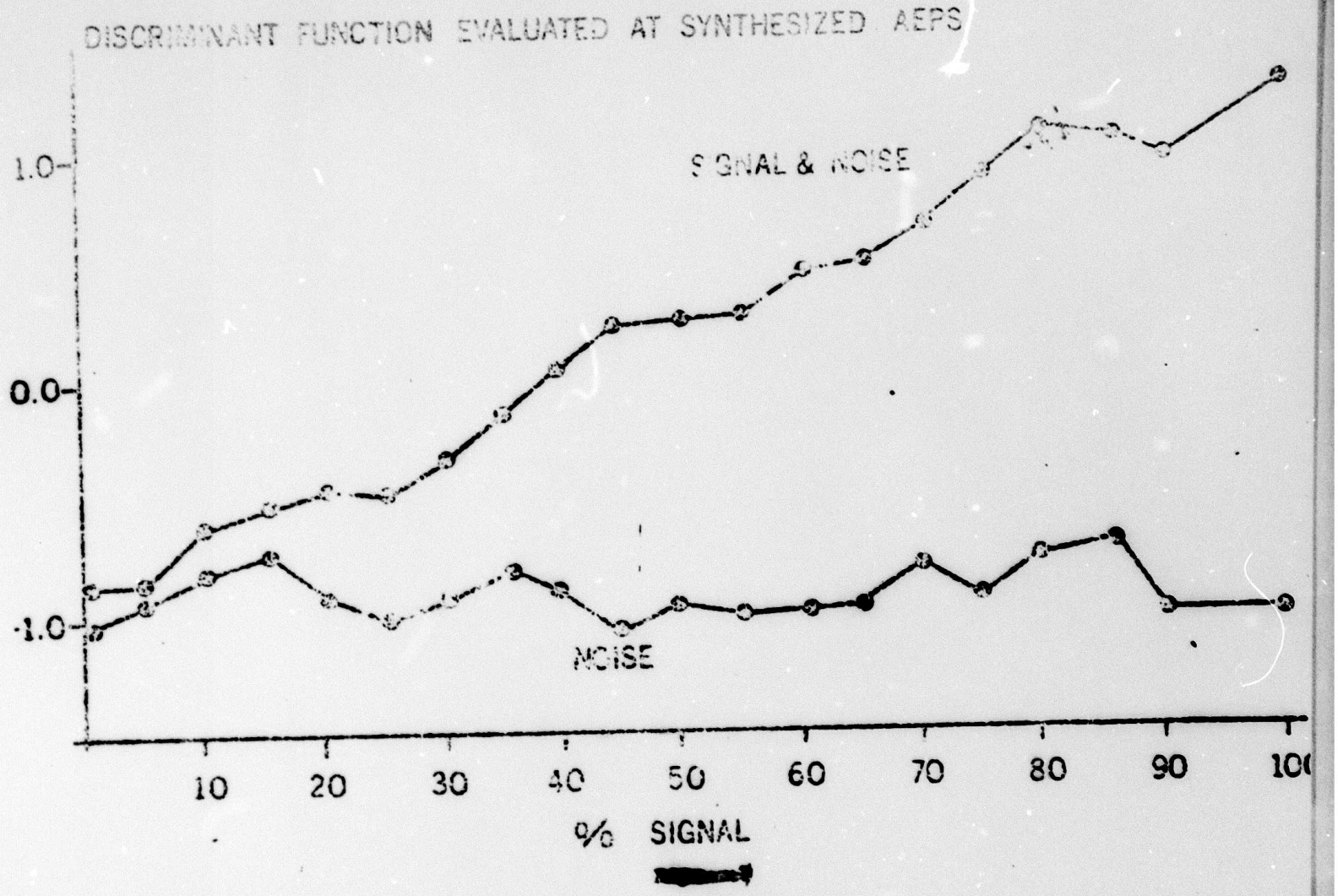
OVERALL AVERAGES (10 SUBJECTS)



Fig 2


"Guess" Discriminant Function Evaluated at Various AEPS
Vertex
EOG
Guess sessions
FT sessions
Guess sessions
1.4
1.2
1.0
.8
.6
.4
.2
.0
-.2
-.4
-.6
-.8
-1.0
-1.2
-1.4
-1.6
-9.5
-10.0
2.5
1.0
.2
.0
-.2
-.4
-.6
-.8
-1.0
-1.2
RAM
SHIFT
PAT
LRD
ALT
RAM
ALT
RAM
SHIFT
PAT
LRD
ALT
EXPERIMENTAL SERIES


AVERAGE OF SIGNAL
% SIGNAL
AVERAGE OF NOISE
100
90
80
70
60
50
45
30
15
0
80
VARIABLES
0
80
VARIABLES


DISCRIMINANT FUNCTION EVALUATED AT SYNTHESIZED AEPS
SIGNAL & NOISE
NOISE
1.0
0.0
-1.0
10
20
30
40
50
60
70
80
90
100
% SIGNAL

4a
Fig 4a

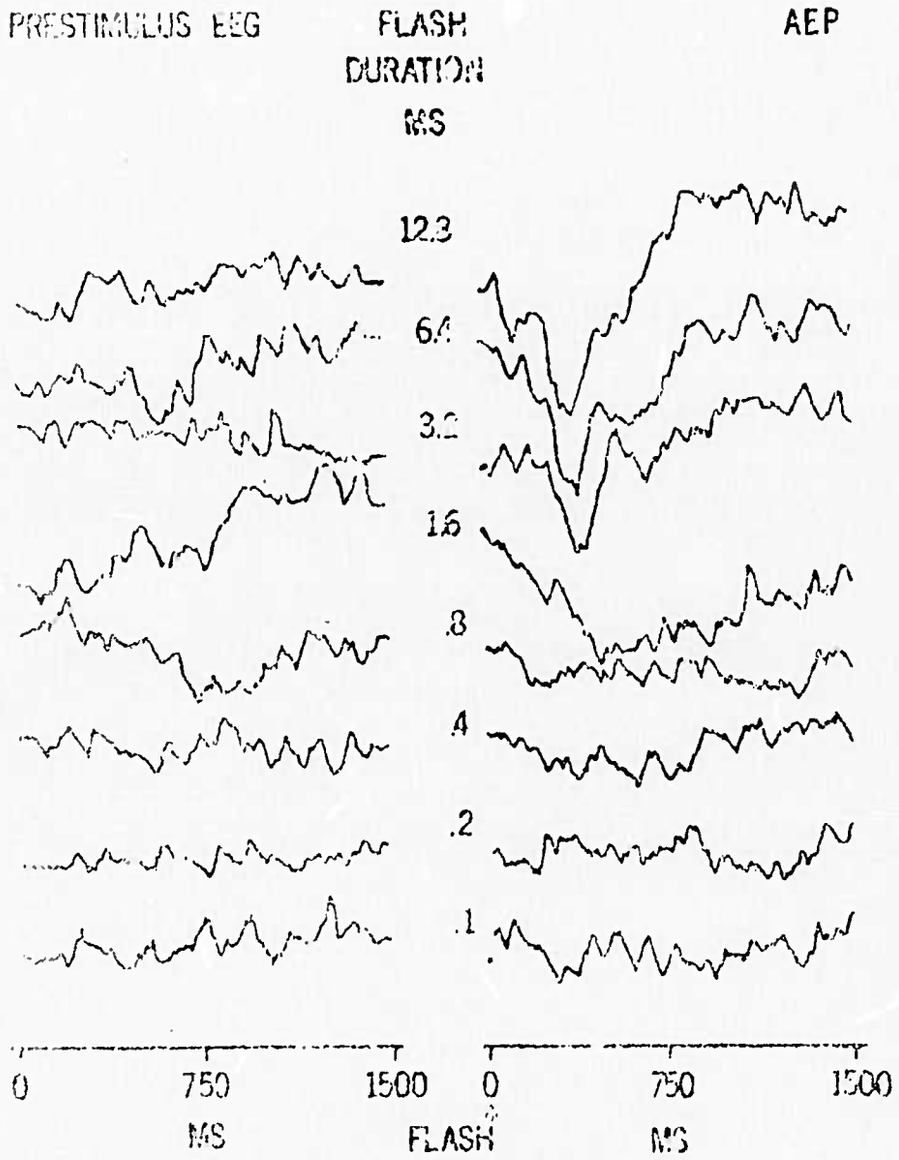
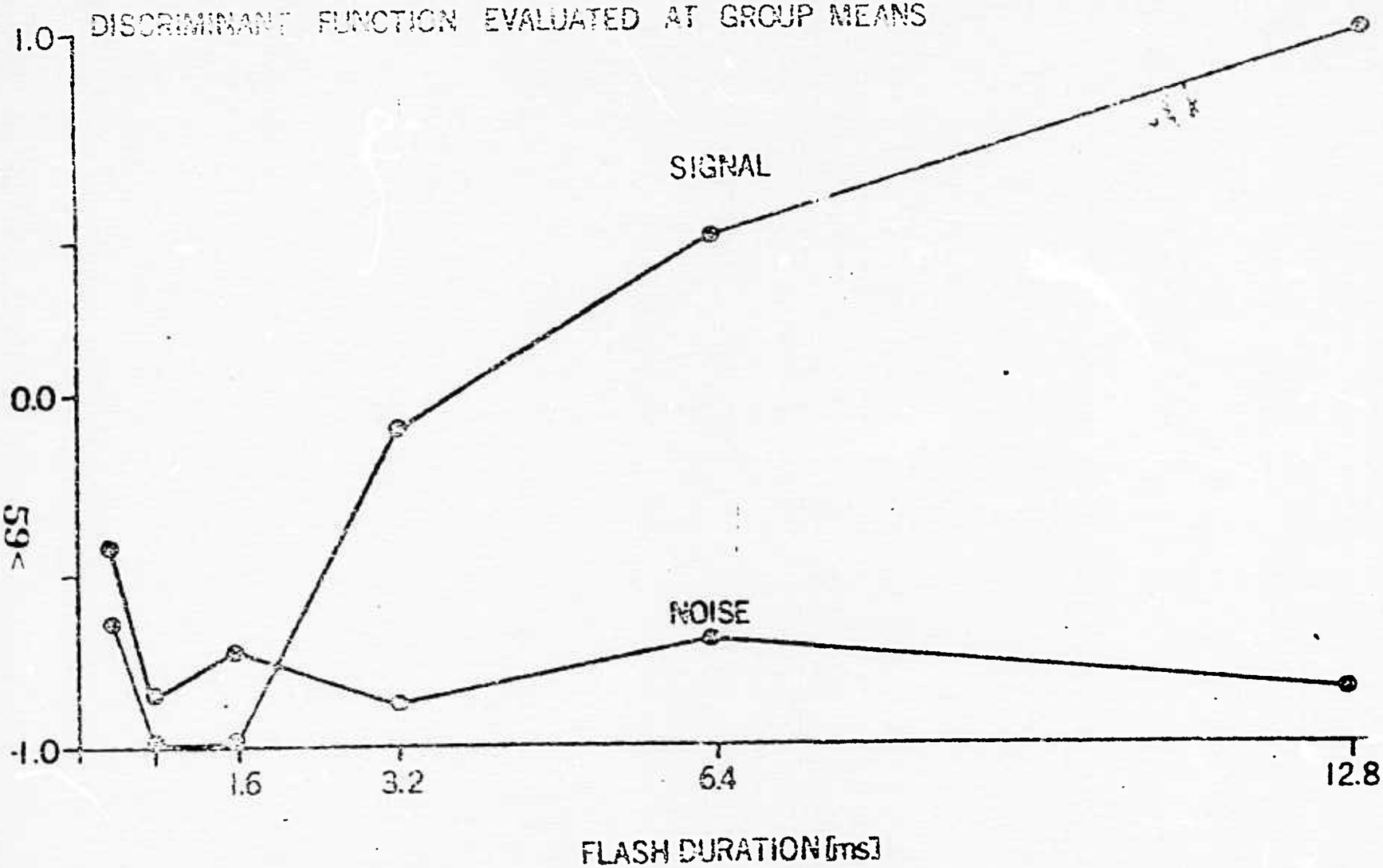


Figure 4


DISCRIMINANT FUNCTION EVALUATED AT GROUP MEANS
1.0
0.0
-1.0
SIGNAL
NOISE
1.6
3.2
6.4
12.8
FLASH DURATION [ms]

THE RELATIONSHIP BETWEEN P300 AND THE CNV: A Correspondence and an Experimental Demonstration

Edited by

Emanuel Donchin

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INTRODUCTION

In planning the Congress it was decided that accent should be placed on experimentation. A series of demonstrations were therefore scheduled, one of these being designed to cover the P300-CNV relationship, to be conducted by Dr. Emanuel Donchin. In the course of preliminary discussions between Drs. Donchin, Cooper and McCallum the view emerged that, given the circumstances, the facilities and the presence of an informed and critical audience, a simple demonstration would do less than justice to the occasion. It was therefore proposed that the opportunity be taken to try to resolve some of the differences of opinion and interpretation existing between those working in the P300-CNV field.

To achieve this it was agreed that Emanuel Donchin should convene a work-group whose aim would be:

1. To define the problems and identify differences of opinion.
2. To design an experiment leading to a resolution of these issues.
3. To demonstrate the experimental design to the Congress using the laboratory facilities of the Burden Neurological Institute.

For various reasons one or two of those originally approached were unable to participate fully, but the group listed above, representative of those working in this area, was formed and a correspondence conducted in an attempt to cover the requirements of Stages 1-2 and to carry out Stage 3.

For a period of 2-3 days immediately prior to the Congress those members of the group indicated by an asterisk met in Bristol for a Workshop to finalize the experimental design.

The account which follows is an edited version of the pre-workshop correspondence arranged by subject matter, with a narrative of the experimental presentation and a general discussion of the points raised during the conference. While this procedure may at times have done a little less than justice to some of the points made, we hope that it will have served to give an added degree of continuity and readability.

The Editors

A. A DISCUSSION OF THE SIGNIFICANCE OF P300

Donchin (4 April 73)[1]:

The basic assertion which guides and generates this discussion is that of the variety of claims made and studies reported in the last decade, concerning the relationship between evoked responses and complex psychological processes those which seem to be best sustained by a consistent, replicated body of data are concerned with P300 and the CNV. It has been established that under clearly defined experimental circumstances both of these potentials can be recorded with a high degree of reliability.

The conditions required for the elicitation of the CNV are simply stated. One must have an interval limited by at least two stimuli or events. One must have a task of some sort associated with the stimulus terminating the interval and the subject must be motivated to perform the task. The subject needs also to recognize the contingency between the interval and the task. Given these conditions a surface negative potential can be recorded, usually peaking just prior to the imperative stimulus. For the purposes of this discussion it is important to note that the negativity is ordinarily followed by a rather rapid positive going change in potential.

The P300 (or P3 as some of us call it)[2] component is a positive-going wave which characterizes the evoked potentials elicited by stimuli when these are presented under certain experimental conditions. Many manipulations have been shown to lead to the enhancement of P300 with considerable reliability. For example, the presentation of series in a random versus an alternate sequence, a signal detection paradigm, a habituation paradigm, a mixed tasks paradigm

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[1] The dates of the letters from which the following were extracted are given in parentheses.

[2] The terms of P300 and P3 will be used interchangeably in the following discussion.

and a missing signal paradigm have been used by various investigators to demonstrate P300's enhancement. Each of these paradigms has been replicated several times by the laboratory first reporting it as well as by other laboratories. Several variations have been played on each of these themes and the results are fairly consistent.

Given the above, there arise questions which relate to the basic physiology of both the CNV and P300, the identification of and specific locus of their generating mechanisms, the manner in which the waves are generated, etc. In this correspondence these issues will be discussed only to the extent that they can be studied with intact human subjects.

A second set of issues we should discuss involves the precise psychological circumstances under which P300 can be elicited. Several of us have expressed ourselves at one time or another with respect to this issue, proposing different interpretations of P300.

a) Sutton and his group have proposed that P300 amplitude is a function of the resolution of uncertainty by the stimulus, or alternatively the delivery of information by the stimulus.

b) Ritter, with Vaughan, initially tended to identify P300 with the orienting reflex, later modifying this view to incorporate cognitive processing in the functional significance of P300.

c) Various proposals have related P300 to the matching and mismatching of perceptual expectancies or templates; this has been particularly noticeable in the reports from Hillyard's laboratory.

d) Donchin has proposed that P300 is a reflection of a decision-making process by the subject, independent of the specific nature of the decision the subject takes.

e) A more recent statement has been made (not yet published) which has associated P300 with the generation of an affirmative decision by the subject.

It is necessary to distinguish between theories which specify experimental manipulations as a prescription for the enhancement of P300 and theories which postulate intracortical mechanisms descriptive of the generation of P300. The first type of theory has the merit of being operational; the second is more interesting but more difficult to test because it postulates unobservables. It is not clear, for example, how one can determine whether or not P300 is a mismatch-detector output without defining the properties of that mismatch detector and their effects on observables.

Thus the following questions need specific answers if P300 is to be understood:

1) What variables determine its amplitude?
2) Is there a theory about the functioning of the brain as an information processor which explains the known facts about P300?
3) Are the positions enumerated above mutually exclusive?
4) Is there any way in which we can combine or integrate the supposedly divergent views expressed above into one coherent integrated whole?
5) Given a theoretical position of one sort or another, what are the specific experimental operations that will allow a determination of the correctness or erroneousness of this theory?

My own personal prejudice tends to find the pursuit of physiological correlates of psychological processes as a less than interesting activity. I tend to be far more interested in knowing what mechanisms underlie observed behavior. Thus, if I am told that some physiological function is a "correlate of attention", or "reflects attention", that statement does not excite me.

It may be of some use in the development of monitoring instruments for a variety of purposes, and P300 indeed ought to be a useful monitoring tool. Yet I will not feel enlightened by this statement. What I would like to know is, how the brain controls its input ("attention") so that a P300 appears as a by-product?

Sutton and Tueting (27 April 73):

While we have used terms like "uncertainty" (Sutton et al., 1965), "information delivery" (Sutton et al., 1967) and, more recently, "salience" (Jenness, 1972) to characterize the situations in which P3 is obtained, the use of these terms is essentially heuristic. It is unclear whether all the situations which elicit P3 share some underlying and as yet undefined psychological variable or not. The only thing we are sure about such an underlying variable, if it exists, is that it must have some varying and quantitative properties (Friedman et al., 1973; Jenness, 1972; Paul and Sutton, 1972; Tueting et al., 1971).

Posner (8 June 73):

Unlike Donchin I feel that the question of the psychological significance of these brain events is interesting, but I agree that the method of going about this task has not been in the best direction. Most discussion has been of S-R relations. That is, the P3 is thought to be caused by certain stimulus and instructional conditions, the delivery of information, reduction of information, decision, etc. All of these statements are probably true to some degree, but it may be more useful to seek R-R statements. The behavioral data have shown that S can "pay attention" under nearly any set of stimulus and instructional conditions. He will be more inclined to do so when the signal is novel, has high uncertainty, or involves a decision, but he may,

if inclined, "pay attention" to a routine, predictable signal to which no decision is required. Once he "pays attention", a number of important consequences occur. For example, he will be less able to deal with new signals.

P3 seems to be closely related to the operations of a brain system which is serial, or close to serial, in its operations. That would be trivial if it were not possible to show that very complex psychological operations occur entirely outside this system, including operations as complex as obtaining the meaning of familiar words. Thus, this system is reserved for only a small subset of high priority operations. While we may never be able to describe the antecedent events which lead to the use of this mechanism, we should be able to describe the consequences of its use.

Hillyard (19 June 73):

Donchin has pointed out the difficulty in defining a hypothetical construct such as "mismatch detector" in terms of observables, a problem even more acute for a theoretical construct like "general purpose processor" proposed by Donchin (Donchin et al., 1973). The notion of "processing" as the reflector of P3 is so general and vague, like "reactive change of state", that it appears to remove us one step further from the problem.

Moreover, in order to make this a meaningful, testable proposition it is still necessary to specify: (1) what is "general purpose processing" and what isn't? (2) what are the conditions that elicit such processing and what are its consequences? and (3) how do you know how much of it has occurred? I don't pretend to have a theory that copes with such questions any better. However, concepts like "match" and "mismatch" detectors, "expectancy bias" on decision stages (Squires et al., 1973a), are sufficiently specific to be testable under some circumstances. The literature appears to show that P3

accompanies some kinds of processing and not others. If P3 reflects "general purpose processing" why should it be larger for (1) hits rather than correct rejections of equal confidence in the threshold task with cue lights? (Squires, 1973) (2) no-go than go responses in choice RT? (3) the more improbable signals in a host of detection and feedback paradigms? (4) signals of intermediate difficulty of detection rather than those of great difficulty of detection?

It is also clear that P3 does not reflect processing that precedes the identification or recognition of a stimulus, since P3 is small or absent for non-signals (Hillyard et al., 1971; Ritter and Vaughan, 1969), highly expected signals, (Tueting et al., 1971) etc. In other words, evocation of P3 is completely dependent upon the output of those processing and memory retrieval stages that result in the stimulus being identified as "meaningful", "relevant" or whatever.

Therefore, while any proposition that P3 reflects utilization of some processor rather than its output could be correct, it is unarguable that a necessary condition for evoking P3 is a particular kind of output from a particular kind of processor, having to do with perception and recognition.

The recent experiment of Donchin et al. (1973), showing late positivity to be independent of S_2 predictability in an RT task, is intriguing. It is of much theoretical importance to know if uncertainty is a necessary condition for enhancing the P3, and if increasing uncertainty always increases P3. To my knowledge, the only other study which showed no effect of an uncertainty manipulation on P3 was also in the RT situation; Karlin et al. (1970) found that P3 was the same for simple RT as for choice RT (on the "go" trials). In the Karlin study, however, there was always temporal uncertainty, despite stimulus certainty, as in prior experiments by Wilkinson & Morlock (1967) and Vaughan & Ritter (1970).

Accordingly, before the Donchin et al. proposal that uncertainty is not a necessary condition for P3 can be accepted we need to know whether the equality of "P3s" would hold at shorter warn-S_2 intervals, where temporal uncertainty would be less than with the 1.4 sec ISI. Related to this it is not clear whether an order effect might be present. If the alternate-RT condition was given to all Ss as the first condition of Session 2 (the first EP recording session) they would tend to have greater temporal uncertainty regarding the ISI in this initial run than in later runs when the ISI had been better learned. Increased temporal uncertainty then might inflate the P3 on the alternate condition so that it matched the P3 on the random condition which has less temporal and greater stimulus uncertainty. Secondly, it is necessary to know whether the "P3" in the warn-S_2-RT task is the same P3 as in the guess-feedback condition, or whether the post-S_2-RT positivity is composed mainly of the CNV crashing down to base plus a P_2 of the motor potential. The third question is how does RT change with S_2 uncertainty? Karlin et al. (1971) found that an auditory evoked "P3" was larger for faster RTs. Possibly then the faster RTs in the alternate condition would in themselves, for whatever reason, enhance the P3 that would otherwise be smaller because of the certainty of S_2's identity. These complexities make it difficult to arrive at an unequivocal conclusion that P3 can occur in the absence of uncertainty.

Various experiments do demonstrate that P3 occurs differentially after a relevant "signal" that is to be counted, etc., when signal and non-signal are equally probable, Donchin & Cohen (1967); Harter & Salmon (1972); Ford et al. (1973); Wilkinson & Lee (1972). These results, plus our findings that P3 on Hits is much larger than on equally confident correct rejections, makes me tend towards the view that P3 is elicited when a stimulus matches a memorized template of a signal that is currently relevant (in that a

differential cognitive or motor response must be made to it). This view is substantiated by our recent finding (Squires et al., 1973b; Squires, 1973) that P3 is also enlarged on the "false alarm" trials in which an absent signal elicits an affirmative detection response. The signal can, of course, be anything - a threshold pip, omitted click, a click among letters, one of two visual patterns, etc. This variety of selective attention, where stimuli are selected on the basis of comparison against memory is called "response set" by some psychologists (e.g. Broadbent, 1971), as opposed to a "stimulus set" when stimuli are "filtered" at a processing stage prior to comparison against memory. Associating P3 with "response set" selective attention does not take one very far, but it does make explicit the point that P3 is elicited only after a comparison of sense data with internal memorized stimulus representations. In the past I have speculated that the P3 reflected an integral part of this template matching, recognition decision process, but it is entirely possible that P3 is a sign of a subsequent state, cognitive or non-cognitive.

I concur with Sutton and Tueting that in most discrimination-decision type tasks which do not involve a motor response the P3 component is much more reliably obtained and is usually larger than the CNV. Our earlier report that larger CNVs accompany correct detections in the threshold task was a very small effect indeed (-4 vs -2 uV), $p < .05$, see Hillyard et al. (1971). The P3 effect, however, (hits vs misses, etc.) was indubitable. Considering all the unpublished things I have done since, it seems that the correlation between CNV and perceptual accuracy is at best small and inconsistent. It is my current unsubstantiated belief that a subject who concentrates intensely will not show a CNV-perception correlation while one who is drowsy or inattentive enough to produce considerable trial to trial CNV variability will show covarying changes in perceptual accuracy.

Sutton and Tueting (25 June 73):

We have just completed some experiments which show that Donchin's conclusion that in a reaction time task there is little or no increase in P3 as a function of uncertainty is a premature conclusion. Initially, we were tempted to agree, since we found that in both simple RT (press on all stimuli) and in choice RT (press left for one stimulus, press right for the other) there was little if any difference in "P3" across the certain and uncertain conditions. We put "P3" in quotes because the waveform during reaction time trials was so unfamiliar we could not be sure which component was which.

We then changed the task from "press left - press right" to a "lift-no lift" reaction time task. The "no lift" trials could be examined separately and would contain little if any of various forms of "motor" artifact. Further, it is known in reaction time work that the lift response is simpler than the press response. The no lift trials yielded P3s which were very clearly larger in the "uncertain" condition than in the "certain" condition. Second, even the lift trials gave a clearly larger P3 (with smaller P3s on the no lift trials) in the uncertain than in the certain condition (Tueting and Sutton, this volume).

Donchin (29 June 73):

Tueting and Sutton's new data on reaction-time are most interesting but I do not believe they will lead me to change any of the statements made in Donchin et al. (1973). We never claimed that in all conceivable reaction-time tasks one would obtain a large P300 in a "certain" condition. We claimed that uncertainty is not a necessary condition for the elicitation of P300. To show that something is not a necessary condition requires only one negative instance. Given our specific experimental instructions, we find we

can get a P300 in a "certain" condition. So, I take it, did Tueting and Sutton in their left/right respond condition. That P300 in the lift/no lift condition is reduced during a "certain" condition is not surprising. In formulating future accounts of P300 there may be several situations in which there is no uncertainty and yet P300 is large. As we stated in our paper, it may very well be that uncertainty is a sufficient condition for the elicitation of P300 but it is by no means a necessary condition.

I do not see why Posner suggests I feel the question of the psychological significance of "these" brain events is not interesting. On the contrary, I think it is fascinating. However, looking for "correlates" does not get at the "psychological significance" of these brain events. To say that a brain event is a "correlate" of a psychological process is to say only that both are observed at the same time and that they vary concurrently. It says nothing about the causal relationship between them. The situation becomes more confusing when either "correlate" is diffusely defined with multiple meanings, even though most of us may have an intuitive understanding of what is meant.

I have never understood what one gains by saying that the CNV is a correlate of "attention". If we can show how processes that lead to the development of this particular potential are involved in the selection of input by the nervous system then the CNV can contribute to understanding attention. A symmetrical question involves the degree to which we can translate hypotheses about the mechanisms of input-selection into biophysical terms leading to predictions that a CNV-like wave would develop over the cortex.

While we are nowhere near achieving this goal, the strategy for achieving it is not to keep searching for what might very well be spurious correlations. We need very precise operational definitions of the concepts

we use, and we need to know exactly what are the situations influencing the CNV and P300 and what is uniquely common to all such situations (Donchin and Cohen, 1967; Smith et al., 1971; Donchin et al., 1972). In this I tend to agree with Posner's point of view, if I understand his usage of "R-R statements" correctly (see also Donchin, 1973).

I am glad to see Hillyard now concurs with my rejection of the interpretation of P300 as the output of a mismatch detector. The idea that P300 represents the activation of a response-set is a perfect example of our notion that the component is an indicator of the activity of a processor with wide, general, utility, invoked by many, different, tasks.

I stand corrected by Hillyard's comment about our use of the term "general purpose processor". What we meant to say is that P3 reflects a rather specific processor which performs specific functions but which can be called, or invoked, by programs performing a variety of tasks. An appropriate metaphor would be that of a "subroutine". In this sense the subroutine is a "general purpose" subroutine needed by many different programs. Its usage in a given program is determined not so much by the ultimate task of the program, but by the algorithm which the programmer has selected for achieving his ultimate goal. This seems similar to Posner's points about high priority operations and one could consider interrupt-service routines as providing a good model. Hillyard's response-set suggestion simply describes a candidate subroutine that can now be evaluated. Of course, the fact that False Alarms, where presumably a positive match occurred, and correct rejections in which an affirmative decision have been taken, fail to elicit a P300, poses serious difficulties for Hillyard's interpretation of his data.

I do not think that the subroutine metaphor is just another vague theory of P300. Rather we point out a possible research strategy and a conceptual framework within which P300 can be considered. In most of the literature

concerning these brain events we have been assigning tasks to the subjects and then trying to correlate the appearance of P300, or its amplitude, with some features which are defined by the ultimate goal of the assigned task. What I am trying to propose is that there may be some feature of these tasks, some aspect of the algorithms used by the system to handle the tasks, which leads to the invocation of the P300 "interrupt routine". (After all, it is possible that the nervous system handles input-selection and priority assignments using an interrupt system rather than the multiplexed, time-shared, system which the previous generation of advanced computer hardware and software has led psychologists to propose. Within a multi-program, multi-task environment the ability of different tasks, different signals, to interrupt ongoing activities and force themselves into the "center of attention" would be very useful.) Speculatively, P300 may be a sign of the occurrence of an interrupt-request, while its amplitude is a measure of the complexity of the interrupt servicing that was required on any given occasion.

I do agree that P300 accompanies some kinds of processing, and not others. What I am asking is, what processing is accompanied by P300? The answer, to my mind, lies not in the ultimate goal of the subject's task but rather in the processing employed on the way to the task.

Ritter (20 June 73):

There is a negative potential which precedes P3 which is "endogenous" as is P3, and probably is intimately associated with P3. This negative potential is clearest when P3 is elicited by a missing stimulus, but it may occur for stimuli that are there. Picton, and also Vaughan and I, have found that the negative potential has a different distribution across the scalp than P3. This potential is something new on the scene and therefore deserves our attention. What is the relationship between these two waves?

Do they always occur together? Do they have similar functions? Since P3 mainly occurs after RT, but the negative potential occurs before RT, this suggests the possibility of different functions for the two components.

The most difficult problem to face is the absence of P3 to correct rejections, false alarms and misses in signal detection experiments. Hillyard's latest position that P3 is associated with an affirmative decision is probably addressed to this issue. But if P3 reflects an affirmative decision, then in Sutton's guessing experiments, when a subject is wrong and gets a P3, P3 must reflect something like: "Oh yes, that is not the stimulus predicted." We have turned our vigilance task around and instructed subjects to respond to each of the non-signals but to withhold response to the signals. The signals still elicit P3, and this result also seems to strain a notion that P3 reflects an affirmative decision.

B. THE CNV AND P300

Donchin (7 April 73):

Several years ago, Smith and I attempted to make the point that many of the experiments in which the CNV was recorded had much in common with the experimental situations in which P300 is recorded (Donchin and Smith, 1967, 1970). The CNV is usually followed by a substantial positive-going limb, which, in many cases, has a peak latency of some 300 msec. The question we raised at the time concerned the degree to which the positive-going limb of the CNV is closely related to P300.

Several studies have led to a general consensus that the CNV and P300 may be distinct. First it appears possible to record different amplitudes of P300 following identical CNVs (i.e. Donchin et al., 1972). Donald and Goff (1970) have shown that the same P300 could be associated with different amplitude CNVs. In both cases, it is deduced that variables

which determine the amplitude of one component do not affect the other component and that the two waveforms are thus distinct.

From the fact that the amplitude of P300 seems to be determined by the characteristics of the imperative stimulus rather than by those of the preceding CNV, it follows that P300 is affected by variables which do not affect the amplitude of the CNV. Does it follow from this that P300 is not identical with the positive going limb of the CNV? I think not. There is nothing in the CNV literature whish suggests that there is a positive correlation between its positive and negative limbs. It is perfectly conceivable that the resolution of an anticipatory and preparatory process will be determined by the specific nature of that which has been anticipated, so that both the positive limb of the CNV and the P300 will be affected differentially by the properties of the imperative stimulus in the same way. It would appear that the relationship between the negative and the positive going limbs of the CNV is unclear and the issue still open.

If we consider two cortical waves "identical" we assume that we are observing the activity of the same physiological mechanism generating the same voltages for essentially the same purposes. Thus the implications of labelling P300 as related or unrelated to the CNV are important. If we imply that P300 is generated in the same locus as the CNV's return to base line, then the CNV is not an index of generalized cortical arousal or of the priming of the motor cortex, but is related to more specific data processing mechanisms. Alternately, if one is convinced that the CNV is a diffuse process, and one is also convinced the P300 and the CNV are related, then one must assume CNV to be more diffuse and generalized than many of our discussions of it imply.

A somewhat different set of questions is raised by the Donald and Goff study. I believe that study not to be directly relevant to the issue raised

by Donchin and Smith. The discussion in the preceding paragraphs focussed on the relationship between A CNV and the positive going limb of that CNV. Donald and Goff, on the other hand, investigated the relationship between a CNV and P300 elicited by a stimulus not related to that CNV. This probing technique is indeed useful and elegant. However, it is quite conceivable that a CNV will have little effect on the cortical response to stimuli which are not part of the warning-stimulus/imperative stimulus cycle. Several issues must explicitly be dealt with in this context. Are we assuming that only one CNV can be elicited in the human cortex at any time? Is it possible, alternately, that several ongoing processes would take place concurrently, possibly without interfacing, or perhaps displaying a variety of complex, interacting modes?

It is also useful to determine whether or not we believe the CNV processes can occur which do not manifest themselves in the classical form by which we have come to know the CNV. Is it possible that the processes reflected by the CNV precede numerous stimulus triggered events and that we cannot observe these events because we can observe the CNV only when it is time-locked and synchronised with our averaging computers. Possibly, the CNVs we do observe are but a small class of CNV-like process. The class we observe contains those records which are associated over a short time interval with an imperative stimulus. The broader class includes all preparatory and anticipatory processes which precede action. Thus it is not unreasonable to assume that Donald's probe stimuli were preceded by their own CNV, and that the CNV was not "visible" to averaging computers. Is this an empirical question? Can we ever determine if there is CNV activity which always precedes action or is it a moot point which cannot be resolved by experiment?

Sutton and Tueting (27 April 73):

We were intrigued by Donchin's thought that P3 might be the descending limb of the CNV and nevertheless be, to varying degrees, independent of the amplitude of the CNV. Of course, what this resolves into is the question of the brain generator of the CNV and of P3. We would hope that one of the people who has been looking at the distribution of P3, might have some data relative to this issue before the conference.

Our views on the relation of the CNV to P3 are that this is a question that may not have a yes or no answer. Essentially we feel that there are some experimental situations in which the CNV strongly influences P3 amplitude, other situations in which the P3 amplitude is independent of the level of the CNV and, finally, perhaps even situations in which P3 is the joint resultant of two processes, one of these arising from the CNV (Tueting and Sutton, 1973).

In our hands the CNV has generally been a low amplitude, experimentally somewhat unpredictable, process. Partly this was due to the fact that we were concentrating on guessing designs in which contamination of P3, and even the CNV, by the subject's motor involvement would be avoided. However, even when we used more classical CNV paradigms we had difficulty in obtaining reliable CNVs within and across subjects. Our experimental difficulties with the CNV have been one of the main reasons why we have never been impressed by the hypothesis that P3 is the descending limb of the CNV. We find that most of our P3 experiments are highly reliable across subjects and repeatedly within the subject. It is possible that the CNV may be more difficult to time-lock and therefore in our averages (in which we use relatively large numbers of trials) the CNV could "smear" in the average, while P3 does not or does so much less. But whatever the source of the variability of the CNV, it certainly does constitute a problem. Hillyard et al. (1971) found larger CNVs associated with accurate than with inaccurate

discriminations. We (Paul and Sutton, 1972) found no consistent relationship between accuracy of discrimination and amplitude of the CNV, and Delse et al. (1972) reported larger CNVs associated with inaccurate discriminations (in females). These kinds of problems make me think that the systematic CNV versus P3 relations are going to have to be worked out in one laboratory rather than inferred across a series of studies from different laboratories.

Posner (8 June 73):

It seems to me that the CNV-P3 relationship is not a very direct one. I understand Donchin's objection to the Donald-Goff study with probe stimuli and also the view the P3 might depend on the existence of CNV but not vary with its amplitude. However, we are able to obtain P3 where there is no CNV, and also to manipulate P3 amplitude where CNV is held constant. The former finding arose when we found that a warning signal presented on only a portion of the trials produced a clear P3 wave. Usually, we have not gotten P3 to a warning signal which was given on each trial. There was no evidence of CNV preceding the warning and very clear evidence of it following the warning. Thus, we knew that the recording technique was adequate to record CNV. Moreover, there was no overt response to the warning and thus, no CNV of the type which sometimes occurs between a signal and its overt response.

Second, we find in comparing warning with no-warning trials that CNV varies greatly but the EPs to the imperative signal do not differ in shape, although the non-warning EP begins at a point far more positive than the warned-trials EP. Thus, the existence of a large CNV seemed to have no effect upon EP shape. Unfortunately, the P3 component in this study was not too clear in either condition, but I think this tends to confirm the Donald-Goff result in a situation in which the EP is to a relevant signal (i.e., one he is preparing for).

Third, our instructions to "attend" by counting the stimuli did serve to increase the size of the P3 in a situation where the CNV was kept constant by randomization. Unfortunately, this kind of evidence is somewhat flawed by the possibility that the instruction affects the CNV release even though it cannot affect the CNV amplitude prior to the signal.

Fourth, we often find that when we delay the overt response to a discriminative signal by not allowing S to respond until a later event, the CNV does not appear to release after the discriminative signal. Yet, in many cases, we get P3 to this signal. Again, this technique is somewhat flawed by our failure to compare EPs in the delayed and immediate response trials directly. This is hard to do because of motor and blink involvement near the P3 when an immediate response is allowed.

In summary, I think, the evidence is fairly convincing that CNV and EP are separate, though sometimes superimposed. It would seem to me that use of warning signals which occur only on a portion of the trials would be a good way to investigate P3 to signals not preceded by a CNV and to compare, at the same time, P3 with and without CNV to a subsequent imperative stimuli. I believe our data on this are encouraging, but I would like to see someone better equipped to do this study.

Ritter (6 June 73):

The relationship between CNV and P3 are, in Posner's words, very indirect. He reports obtaining P3 without CNV. We have similar data, as apparently do Tueting and Sutton.

Nevertheless, when a CNV terminates, is that termination necessarily associated with a P3? Most of us have apparently been able to obtain P3 without any CNV. But is the reverse true: are there CNV terminations which do not include P3? And if so, how would we recognize them? Since

P3 varies considerably in latency, latency by itself is not a sufficient condition to establish the presence of P3. It seems to me that examining the relative distributions of CNV and P3 across the scalp could indicate whether the two events come from the same generator. Accordingly, I would suggest that in any experiment we conduct at Bristol we place several electrodes (for example, Fz, Cz and Oz as a minimum) in order to ascertain whether the simultaneously obtained CNVs and P3 have similar topography. Donchin quite rightly says with respect to CNV termination and P3 that "when I consider two cortical waves 'identical' I am assuming that I am observing the activity of the same electrocortical generator," it follows that the only currently available method of resolving whether two events have the same cortical generator is via their scalp distribution.

It could, of course, be the case that the generation of CNV has locus A and the termination of CNV has locus B, and that locus B is also where P3 is generated. That seems to be an empirical question, and if we could obtain data which speaks one way or another to that question at Bristol, that would be quite important.

Sutton and Tueting (21 April 73):

We should not loose sight of the fact that the relations between the CNV and P3 are only one dimension of the problem. The other dimension of the problem is whether the P3 findings can be accounted for by a pre-stimulus process other than the CNV. The pre-stimulus process here is on the experimental side and is usually assumed to be some differential degree of preparation, or some excitability cycle, or arousal cycle, which the subject sets up in relation to that stimuli. This aspect of the problem has come up most often in connection with the selective attention issue and its effects on evoked potential components, P3 among others. We are fairly well convinced

that in our own work in which the subject does not know in advance which of two stimuli will be presented, that P3 is larger for the rarer event. Our work of course has been done at a constant level of attention.

One formulation that we would like to solicit comments on from the rest of you is Karlin's (1970) formulation that the stimulus "only" acts to release the prior state of readiness -- and in the selective attention situation, releases it differentially. It is hard for us to make very much sense of such a formulation since it concedes the battle while at the same time appearing to win it. If the stimulus differentially releases the prior state as a function of its identity (which by instructions has been associated with one or another meaning), then necessarily the identity of the stimulus plays a role in determining the characteristics of the evoked potential and of course the amplitude of P3. That the prior state also has an influence on the meaning of stimuli is self evident and surely not the issue.

One critical experiment is whether, as Ritter has suggested, in an S_1-S_2 design it is ever possible to get a CNV without a P3. Here only the negative answer -- that you can have a large CNV and no P3 would tell us something important about the necessity of association between the CNV and P3. Such a finding would imply that P3 might be only one of several ways in which the CNV descends to baseline. We certainly have seen situations where there is no S_2 and the CNV slowly goes positive without anything resembling a P3. We also have seen a slow negative shift that builds up during the first three stimuli of a discrimination trial and that "terminates" in a sustained positive return of long duration after the subject's response. In this case the very large P3 in response to a feedback click at the end of the train seems to be independent of the slow negative and positive shifts present in the trial (Jenness, 1972).

Hillyard (19 June 73):

I have noticed a general consensus that Karlin's diffuse arousal type of hypothesis regarding the P3 are moribund. I am of the opinion that the "reactive change of state" hypothesis of Karlin, while unsatisfactorily vague in many ways (since any brain event can be included under the term "change of state") still has some life in it. First, Karlin has to admit (and I think he does) that evocation of a P3 is dependent upon a selective stimulus discrimination and recognition process, since P3s are differentially larger after some types of stimuli than others. Thus the P3 is dependent upon a type of selective attention, a prior "set" to detect or react differentially to some stimuli as opposed to others. This does not necessarily mean, however, that the P3 is a sign or code of the actual signal recognition process or any subsequent specific cognitive act, although I have argued for a specific processing type of P3 myself. It could be, as Karlin has suggested, that the P3 reflects non-specific cortical events which follow the signal recognition/processing, such as a relaxation or readiness, activation of the reticular system, activation of a response program, etc. As Karlin (1970) puts it, "some form of cognitive processing is a prior condition of this (non-selective) change of state, but the change does not embody the processing."

So, as we know nothing of the brain events underlying the P3, I don't see how Karlin's particular hypothesis can be rejected out of hand. On the other hand, while it could be true, the nonspecific P3 hypothesis does not get us very far and is in a sense superfluous, since we still have to define those specific processing events which do control the P3.

Donchin (29 June 73):

A few words on Karlin. It seems to me that Karlin tried to say two

different things simultaneously. His main theme was similar to Naatanen's and assumed P300 to be reflecting primarily the effects of diffuse undifferentiated arousal which may, artifactually, develop prior to the critical relevant stimuli. The other point he made has been formulated as the "reactive change" hypothesis but, in fact, as long as the stress is on non-cognitive, pre-stimulus processes, this formulation is not strongly differentiated from the first one. Once it is admitted that the resolution of these pre-stimulus processes depends on post-stimulus processing, Karlin's position is indistinguishable from any other position and "reactive change" becomes just a euphemism for cortical potential. Here I fully agree with Sutton and Tueting's statement that "if the stimulus differentially releases the prior state as a function of its identity which by instructions has been associated with one or another meaning, then necessarily the identity of the stimulus plays a role in determining the characteristics of the evoked potential and of course the amplitude of P300. That the prior state also has an influence on the meaning of stimuli is self-evident and surely not the issue".

To summarize where we now stand: On the theoretical question of the nature of P300, we have no consensus though we all seem to agree that P300 is not determined by the pre-stimulus negativity. We all seem to agree that it is affected by a multiplicity of different conditions though we are not agreed on the construct which best explains the commonality of this multiplicity of P300 eliciting conditions.

It seems however unarguable that P300 related effects tend to be temporally coincident with the descending limb of the CNV. What the implications of this temporal coincidence are is yet to be elucidated and apparently we do agree that we need to find methods for teasing out the two, using either spectral analysis, spatial distribution, experimental manipulation, or some other method.

Donald (24 July 73):

I do not think there is much point in re-emphasizing the search for the ideal psychological determinant of event-related EPs. Each laboratory has evolved its own procedures and theories, and one theory seems about as useful as the next. I think we know pretty well what laboratory conditions produce event-related potentials, or ERPs, and we must attend to the more basic task of classification.

Classification means attaching labels in some systematic and comprehensive way to all of the numerous phenomena we call ERPs. We have a lesson to learn here from taxonomists: avoid using the same term at various levels of abstraction, and construct a system which accounts for all differences between phenomena, from the most mundane to the most fundamental. Thus, we should stop using P3, P300, CNV, EMR, etc. on so many different levels of abstraction. The worst offender is P3 or P300. It gives the appearance (as was originally intended) of concrete precision - a positive EP at about 300 msec latency. But in the many pages of correspondence in this exchange it has often been used as if (1) it was the only ERP in the sensory evoked response and (2) as if it had been proved that late waves recorded under many experimental conditions of varying latency ranges, were identical with P300. We then argue about the P300 distribution and the CNV-P300 relationship, for example. We do not even know yet whether P300 is distributed the same way in all stimulus modalities, let alone whether highly variable late potentials (LP) of over 400 msec latency, LPs to missing stimuli, post-motor positivities or the original 200 msec Vertex wave, can all be classed alongside it. I continue to use P300 in a more restricted way: to me it means a sensory evoked potential of about 300 msec latency and very reliable intra-individual waveform. Until we have a system of classification by function we should use terms only on this restricted, concrete level of description.

There is much evidence for the independence of P300, defined in this restricted sense from CNV: That the two components occur independently has been noted above. It should be noted that Ritter and Vaughan (1968) reported that a large P300 was elicited by the initial stimulus in a repetitive series: certainly no CNV could have preceded these stimuli. It is important to determine whether the CNV and P300 interact when they occur simultaneously in the brain (as they are in most of our studies). I believe the answer is unequivocal - they do not. Donchin seemed to place considerable emphasis on the thesis that our study (Donald and Goff, 1971) did not bear upon the issues he raised because the P300 wave was elicited by a probe stimulus which was peripheral to the CNV task. However, as I pointed out in our Vancouver meeting (McCallum and Knott, 1973) we analysed in the same way the P300 elicited by the CNV task stimulus and found the same lack of correlation. Furthermore, I think it was an important aspect of our study that it demonstrated that the amplitude of the CNV could be manipulated (by instructions) independently of the amplitude of the CNV, when both were simultaneously present in the brain.

The weight of evidence to date, in fact, suggests that virtually everything can be dissociated from everything else: the early from the late evoked potentials, all evoked potential components from CNV; prefrontal from parietal CNV; etc., and the dissociation can be demonstrated in different ways: by comparing distributions, interactions, behavioural control conditions, and so on.

What this dissociability of components implies to me is that we can not unify either the P300 literature or the CNV literature under the aegis of one monolithic 'nonspecific' system. If we want to establish general laws it will be by demonstrating analogous functional relationships in a variety of paradigms, not by showing that the same neurons are doing the same thing in

a variety of paradigms. Maybe Posner's 'serial processor' lies in a principle of neural organization rather than in a specific locus in the CNS. We cannot assume that activity related to the processor should always look the same, only that it should serve the same function for whatever neural networks are on demand in a given experiment.

Papakostopoulos (July 73):

There is at least one point on which the correspondents seem to agree; namely that the P300 is a brain event. This article of faith is strengthened by recordings of the P300 directly from cortical electrodes in man (Papakostopoulos & Crow, this volume).

Donchin (4 April 73) distinguishes several lines of investigation of the P300 which may, for convenience, be paraphrased as follows.

a) 'The pragmatic approach' which concerns itself with the sorts of circumstances which lead to the appearance of the P300.

b) 'The brain model approach' which attempts to consider which brain processes could employ such a phenomenon, and

c) 'The physiological approach' which seeks to identify the physiological mechanisms giving rise to the phenomenon.

However, this categorization raises further questions. First, can any one of these lines be pursued independently? Second, are we really employing different experimental paradigms if they include a common important element. Third, how much is the brain event called P300 subject-dependent, situation-dependent, instrument-dependent, experimental-theoretical-attitude dependent? For example could such factors as differential control of eye blink artefact, number of trials, length of experiment, time constants used, placement of electrodes and training subjects be crucial? Practices differ from laboratory to laboratory in these respects and it is perhaps surprising that in spite of

them such consistent findings are encountered in P300 research.

The common element in all P300 experiments is that a YES or NO action is retained on every trial. The subject presses (Donchin) or does not press a button (Karlin, Papakostopoulos) or switches from one response to another when a different kind of signal is presented (Donald), or detected (Hillyard), or he gets or does not get what he expects (Weinberg) with various degrees of certainty (Sutton, Tueting). Clearly then what we are energising through our experimental paradigms is a basic mechanism of the nervous system related with the after effects of binary choice of a go-no-go type. The suggestion is that this mechanism is inhibitory in nature. Two questions then arise: First, what is the relationship of this basic mechanism with the prestimulus electrical state of the brain and second, what is the common element in go-no-go actions which is mediated by this mechanism.

The first question includes among others the particular problem of CNV-P300 relationships.

Different spatial distribution for the pre-action negativity and the following P300 has, I think, been well established from multi-channel cortical recordings. I agree with other members of the Workshop on the point that the P300 does not necessarily depend on pre-stimulus negativity but this is not to say it does not depend on the pre-stimulus state of the subject because the pre-stimulus negativity, a) does not spread over all of the cortex, b) is not the same event morphologically for locations where it is present (Weinberg & Papakostopoulos), c) is not the only sign of the pre-stimulus state of the subject. The autonomic system (Lacey & Lacey 1970; Papakostopoulos 1973) and even the monosynaptic spinal reflexes (Papakostopoulos and Cooper 1973 and this volume) show signs of involvement. It has been argued that various brain systems mediate various changes, the integration of which may be the function of still another system.

The answer to the second question would seem to be that in both conditions inhibition occurs. The difference between go and no-go situations could be that in the former, local (parietal lobe) inhibitory mechanisms are involved, whereas in the latter more general and frontally dominant inhibitory systems operate.

The spread of positive, post-action potentials (P300) over the prefrontal cortex in cases of inhibition of ongoing or planned action fits in with physiological, clinical and behavioural evidence gained from experiments concerned with the functions of the frontal lobes (Teuber 1966, Pribram 1961, Luria 1973).

Additionally, during conditioning, negative potentials strongly resembling the CNV are followed after delivery of reinforcement by electrocortical synchronization and positive potentials. Both the post-reinforcement synchronization (Clemente et al. 1964) and the contingent positive variation are considered as signs of brain inhibitory mechanisms (Marcznski 1972). There is some evidence that stimulation of "reward" areas which lead to approach behavior, is followed by negative cortical shifts and hippocampal synchronization, while stimulation leading to avoidance behavior is followed by different EEG manifestation (Wurtz 1966).

Positive shifts of the kind described above seem to be cholinergic in nature and are abolished by atropine and increased by chlorpromazine (Marczynski 1972), a question which is open for test in humans. Again lesions of the pyramidal system, the cerebellum, the basal ganglia and the prefrontal or parietal cortex lead to difficulties or disabilities in alternative motor behavior. Patients exhibiting these conditions could readily be employed to help define the systems which are involved in generating P300, an undertaking with obvious clinical value too.

Humans and animals at various ages have a different capacity for inhibiting

planned action or alternating from one set of actions to another. This differential capacity could be utilized in studying the behavior of the P300 under appropriate experimental paradigms.

There is experimental evidence from animals and our data in man (McCallum et al., this volume) that the caudate nucleus is activated during the cortical negativity of the CNV or the Bereitschaftspotential. Other evidence suggests that the function of the striatum subserves behavioral inhibition (Hull et al., 1967). It is possible that during preparation for action, striatum activity is inhibited which could explain why positive shifts are recorded from these nuclei during periods of cortical negativity. Already evidence exists showing longlasting cortical slow positive waves after caudate nucleus stimulation (Buchwald and Hull 1967).

In conclusion: It seems that the controversy over the nature of the P300 wave is a result of diversity in techniques rather than of differences in the experimental paradigms used. In all paradigms a basic mechanism of electrophysiological and behavioral inhibition is activated. This mechanism seems necessary to arrest planned behavior or to generate the bases for alternation from one behavioral set to another. This proposition can explain the existing data from various laboratories collected with experiments in man and animal. It allows predictions, and suggests experiments to test those predictions in the fields of behavior, normal or abnormal, neurophysiology, neurochemistry or developmental neurobiology. However, parameters of equipment are critical.

C. NOTES FOR AN EXPERIMENT

Meeting (21 April 73):

It is obvious that it would be wise to consider the application of every technical method we have to the CNV-P3 problem, in addition to relying on ingenious and simple design. For example, both CNV and P3 could be

compared for different experimental conditions on height from baseline, area slope, trial by trial variance, between-subject variance, habituation rate, recovery, on differences in averaging from the stimulus versus averaging on the motor response, and on topography using at least six electrode locations. It might even be possible to utilize frequency analysis in order to separate hypothesized overlapped components, e.g. a relatively slow positive return to baseline overlapped by a faster P3. It might also be a good idea to consider using a relatively short S_1-S_2 interval (possibly 800 msec or less) in order to insure time-locking of the CNV.

Sutton and Tueting (29 June 73):

We continue to be troubled by the assertion by Vaughan and Ritter (1970) (and also Hillyard) that P3 is larger at Pz than at Cz. In our work using high- vs. low-pitched clicks, P3 is always clearly larger at Cz. Vaughan (personal communication) suggests that when P3 is obtained when a stimulus is present, the overlap of components -- perhaps P3 with P2 -- may create this impression. It is true that when P3 is obtained to a missing stimulus, P3 is larger or equally large at Pz (Ruchkin's recent unpublished data). While this provides some support for Vaughan's suggestion, inspection of our waveforms provides no support for the overlap of components explanation when a stimulus is present. We would be delighted if any further light could be shed on this question.

Posner (8 June 73):

I cannot add very much to Sutton and Tueting's comments on an appropriate experiment. A short S_1-S_2 interval would be very desirable, such as 0.5 sec which is, by the way, nearly optimal for a large class of behavioral

tasks.* The use of S_1 on only a proportion of the trials, and the use of an S_2 task which may be varied in difficulty, would help to check on some of the preliminary results cited by Tueting and Sutton (this volume). Return to baseline of the CNV can be manipulated very well by increasing the difficulty of S_2. We used a physical versus a vowel-consonant matching task, which has the advantage of same stimuli, but very different RTs and manipulated return to baseline over a range of several hundred msec. Also, the use of the C reaction method (e.g., respond to matching pairs, but not to mismatches) allows direct comparison of overt and non-overt responding without increasing the complexity of the design.

Hillyard (19 June 73):

Given the multi-channel, long time-constant facilities at Burden, I would suggest that a useful type of experiment might be to attempt to partition the late P3-type positivity into separate components on the basis of their differential scalp topography. We all agree that stimuli which elicit P3s tend to be preceded by CNVs, and that the CNV can come crashing down to baseline during the general latency range of "the P3". Thus there is a confounding of these two possible sources of positivity (assuming that there are indeed at least two sources), which has yet to be satisfactorily clarified. Donchin made the key point that CNV fall-off may be governed by different rules than prior CNV level, so that lack of correlation between P3

*Drs. Knott and McCallum feel that this ISI is too short for CNV work. However, Dr. Posner remains convinced that this interval is to be used. Data he has obtained with Wilkinson suggest that CNVs are largest with this interval, a paper by Gaillard and Naatanen (1973) also supports this view, according to Posner.

and prior CNV does not necessarily imply any dissociation, and topographical studies may be the only way to separate the two.

In some published reports the purported "P3" looks to me like "nothing but" a CNV returning to baseline, while in other situations a large, pointed, "P3" can be seen in the absence of any appreciable phasic D.C. shift, before or after (e.g. Hillyard et al., 1971). Looking over some of my old data recorded with D.C. amplifiers it appears that the P3 which follows an auditory or visual discriminandum (as S_2) has a clearly more posterior scalp distribution than the preceding CNV and its offset. Perhaps at Bristol we could set up several classic P3 situations while recording complete D.C. scalp distributions of P3 and CNV and observe the degree of spatial dissociability of the two phenomena.

Donald (24 July 73):

The following is a research strategy which may yield the kind of data we need. The most obvious requirement is that one laboratory, with a single pool of subjects, should try to elicit all of the phenomena (controlling stimulus probability, response set, task difficulty, instructions to the subjects, etc.) recorded in the literature in an unified context, to allow direct comparisons of topography and amplitude between all the different conditions. There should be no phenomenon left in the 1500 or so papers in this area which cannot be accounted for in the context of such a study. We are on our way towards some synthesis already, but to my knowledge no single laboratory has tried to account for or even elicit every major phenomenon in an unified context. Although classification schemes can be dreamed up by anyone just reading and speculating, we will not really know whether a scheme works until these data are available.

Sutton and Tueting and also Hillyard seem to agree that P300 is more reliable than CNV. My experience is the opposite. Provided you give the

subject a task on the fringes of his capacity I have yet to see a subject who will not develop a CNV. On the other hand, I have seen dozens of sessions where a subject was performing a complex vigilance or discrimination task without producing a P300 wave. I suspect our different experiences simply reflect the particular bias built into our experimental procedures.

Donchin (29 June 73):

I propose that we use a paradigm two of my students and I have been using to resolve the CNV/P300 issue. We can use the same structure in Bristol and have a basic paradigm for investigating several of the questions raised by this group. Essentially, the experimental idea was expressed by Posner when he said that we have to compare the evoked responses to warned and unwarned stimuli. Presumably unwarned stimuli are not preceded by a CNV and their evoked response is therefore not affected by such processes. On the other hand, the warned stimuli would be preceded by a CNV and would then provide us with a comparison of the evoked response to the stimulus when it is or is not preceded by a CNV.

The experimental arrangements we used were as follows. The subject sat in a darkened, electrically shielded room, facing a 3-field tachistoscope. Two letters, A and B, alternated at randomly controlled intervals, the mean interval being about 3 minutes, the inter trial intervals ranging from 2-6 minutes. The subject had one major task: at randomly controlled intervals of around 10 seconds, a transilluminated circle was projected, superimposed on the displayed letter, and the subject was to press a switch as rapidly as he could. If he failed to press within a brief interval, usually about 250 msec, the circle was reflashed and the subject was financially penalized. The displayed letter indicated to the subject in which type of reaction time regime he was operating. Letter A indicated that the circle will be

always preceded by a tone. The interval between the tone and the flash was 1500 msec. The letter B indicated to the subject that the tone will be omitted and the imperative stimuli will be presented without forewarning.

The general idea was to try directly to compare the response to warned and to unwarned stimuli, while insuring that the subject's overall state remained relatively the same in both conditions. By intercalating the A and the B regimes we tried to insure that we eliminated major changes in arousal, excitability, motivation, or what have you that can be attributed to session to session variability. This arrangement has an additional advantage, it allows an analysis of the ongoing EEG activity in the A and the B regime. In particular we can study the background activity during the transition from the A to the B state. To repeat this with DC recording would of course be invaluable.

The results were interesting, but puzzling. For some time I have been trying to decide the best way to summarize the data and to present them in a comprehensive manner. The greatest lack that I see in our previous data collection is that we have failed to anticipate two of the major suggestions made by this group. We did not use any manipulation that assured a reliable elicitation of P300, we did not conduct extensive spatial mapping over t' skull, and we did not use DC recording. What I am proposing is that we try to replicate this experiment using the above and any other improvements. The advantage is that the experiment is quite simple to run, the data are straightforward and do not require complex data analysis. At the same time, this paradigm provides sufficient meat for discussion of basic experimental procedures, methodological considerations and interpretation problems. I am assuming that if we agree to the major paradigm, you would all prefer to introduce numerous changes to the arrangements.

Ritter (17 July 73):

The experiment Donchin suggests is indeed interesting and could help to get at some of the issues we have discussed. These suggestions come to mind:

1. We should get the EPs to the changes in the A and B letters. The ISI is very long here, the changes deliver information, we should get P300s, quite large P300s, so large that a small N would suffice, and we may even be able to look at single trials. These EPs are likely to have the $N_?$ potential. There should be no CNV which precede these stimuli. (I wonder whether a change from A to B would elicit the same EP as a change from B to A).
2. Perhaps we could not present S_2 in a small percentage (say 10%) of the trials with a warning stimulus. We could do this both for the RT condition and the no-response condition where a warning stimulus is delivered. (Ss could be asked to count the circles, or their absence, or both, in the no-response condition). By not giving S_2 we are likely to see $N_?$ as there would be no sensory EP. We then could compare the EP to a missing S_2 for the conditions which generate CNVs and those which do not.

Hillyard (19 July 73):

I like the basic design of Donchin's proposed experiment. The alternation between "A" (warning cue) and "B" (no warning cue) conditions seems an excellent way of getting the CNV in and out of the picture. However, I would urge that we also manufacture P3's by one or more procedures in addition to that of making prompt motor reactions to the light flash.

The problems with the motor response type of P3 are: First, the prior CNV is much larger before a motor act than before any type of perceptual act, per se, and this "motor CNV" may be a "different kind of CNV" than that which precedes a perceptual discrimination or decision (following Jarvilehto and Fruhstorfer, 1970). This large CNV comes crashing down after the flash and

confounds itself with the P3 as we have been discussing. What I am afraid of is that a 20 uV CNV, returning to baseline very sharply (as motor-RT CNVs tend to), will obscure the P3 to the point that it cannot be measured and its independent topography cannot be determined. Second, P3's in the simple RT situation are not usually as large as those generated in other paradigms, such as signal detections (of various sorts), wrong feedbacks, and the no-go of discriminative RT. Third, and most important, following a motor response is the "P2" of the motor potential, which Gerbrandt et al. (1973) think is a vertex potential evoked by the somatic stimulus entailed in the motor response. This positivity also comes at around 300 msec after the stimulus, so that McAdam and Rubin (1971) have termed the total positive wave accompanying a motor reaction to a stimulus a "P302" composed of a "P300" and a "P2". For these reasons I think it would be more straightforward to study the P3 in another situation, perhaps in comparison with the motor-RT task.

If we do use the motor-RT paradigm I think it would be better to use a discriminative-RT (go, no-go) task, in which one type of light flash means "respond" and a second type means "don't respond" (in randomized order). In this way the contamination by motor potentials is removed on the no-go trials and a very large P3 is also generated on those trials. In this and other paradigms discussed below, the basic "A" (warning) vs. "B" (no warning) feature would be retained.

One paradigm that I think would work nicely would be to reduce the intensity of the circle-light flash down to threshold levels and simply have the subject detect and count them (or make a delayed "Yes" or "No" response). In the "A" condition, the flash (just suprathreshold) would follow the tone on 50% of the trials at random. In the "B" condition this faint flash would be presented irregularly without warning. A "hit", which

would occur on most signal present trials, (easy detection), should have a large P3, if the visual threshold task is anything like the auditory task. Perhaps a better design for this experiment would be to keep the flash bright and let it serve as a "marker" for when a threshold level tone pip may or may not be presented. This sort of paradigm works well.

A final suggestion for a signal detection paradigm would have the light flash be a bright circle about 90% of the time and a square (or something else) on the rest of the trials, at random. The detection of the "target stimulus" which the subject had to count gives a big P3, preceded by a small CNV.

As an adjunct to the motor-RT experiment it would be advantageous to give a feedback cue for correct (fast) versus incorrect (slow) performance. Such feedback cues elicit large P3s, as do the feedbacks in the guessing paradigm of Tueting-Sutton and Donchin et al.

So I am suggesting that we could make our major point about the interaction of CNV and P3 and their respective topographies in a more clear cut fashion by using one of these variants on the basic "A" vs. "B" setup, in addition to (or instead of) the motor RT task. By generating two or more "types" of CNVs and P3s we would also contribute to the interesting problem of whether "different CNVs" and "different P3s" have the same topography, a question I discuss elsewhere (Hillyard et al. this volume).

D. THE EXPERIMENTAL DEMONSTRATION

Introduction:

A variant of the experiment discussed above has been conducted just prior, and during the conference in the laboratories of the Burden Neurological Institute (BNI). Dr. Ray Cooper kindly modified the standard programs available at the BNI computer to meet our requirements. To assure a smooth

demonstration we chose to use only one subject. The methods described below were used in the demonstration, reflecting a consensus which emerged among members of the working group from the evaluation of data collected during pilot runs. While designing the experiment and conducting pilot runs, the working group derived several methodological conclusions. To place these conclusions in perspective we are presenting the experimental methodology in detail. Some of the preliminary data will be used as illustrations.

Method:

Subject: One female nurse from Frenchay Hospital, (age: XX) participated in all the experimental sessions. The subject has had previous experience as an experimental subject in the BNI Laboratory, and was very cooperative throughout the experiment. On July 26, 1973, we conducted a pilot recording session. In the following weeks the subject participated in two sessions in which a mechanical eye monitor (described elsewhere in this volume by Papakostopoulos) has been used to train her to minimize eye movements in the interstimulus interval. Two recording sessions were conducted on August 6, 1973. The experimental procedure were then modified by the working group and two additional sessions were run, one just prior to and one during the conference.

Recording Apparatus: The electroencephalographic data were acquired and processed using the BNI Laboratory equipment, described in detail elsewhere in this volume.

Experimental Arrangements: The subject was supine on a hospital bed with her head resting on a pillow. A Grass photic stimulator lamp was mounted 50 centimeters over the subject's head while a television monitor was hanging on a wall 6 feet away from, and 3 feet above, the subject's head. Two patterns could be displayed on the TV monitor, in each case

the monitor was divided by a horizontal line into a black and a white half. The experimenter could reverse the vertical order of the two halves. We shall cal. a display in which the upper part of the screen is white the "A" display and the inverted display the "B" display. The subject could listen to tones through earphones. She held a microswitch in each hand. The microswitch was so mounted that a light depression with the thumb produced a contact closure.

Electrodes and Electrode Placement: We used BNI silver-silver chloride electrodes. The structure and characteristics of the electrodes, as well as the placement technique are described elsewhere in this volume. Electrodes were placed in the following locations, F_{P2}, P_3, C_3, C_Z, C_4, P_4 and O_2. Recording was referential to a linked mastoid. In some of the earlier experimental sessions Temporal rather than Parietal electrodes were used. Electroculographic activity was monitored using one electrode next to the right canthus and one electrode just under the orbit. In some of the earlier sessions the infra-orbital electrode was replaced by a supra-orbital electrode.

Experimental Design and Experimental Procedures: The experiment consisted of a series of trials, presented in runs. On each trial the subject was presented with one of two tones. During Reaction Time (RT) runs the subject was to press the button in her right hand if the tone were high, and the button in her left hand following a low tone. During the Predict (P) runs the subject was to guess which of the two tones will be presented on the following trial. During RT trials, the tone was repeated if the subject failed to respond within a specified interval. During P trials the tones were always repeated. Within any run some of the tones were preceded, at an interval of 1500 milliseconds, by a warning flash. On other trials the tones were presented without warning. The subject

was always informed if she will be forewarned of the arrival of the tone. This information was conveyed by the television monitor screen described above. Pattern A informed the subject that the arrival of the tone will be preceded by a warning flash. Pattern B indicated the absence of the warning flash. The status of the television monitor was determined by the experimenter who reversed patterns whenever he either deleted or added the warning stimulus. Note that one of the two patterns was always on.

Runs could also differ in the sequence with which the tones were presented. In some runs the tones were presented in a regular (alternating) pattern. That is, a high tone always followed the low tone, a low tone always followed the high tone. On other runs the tones were presented in an irregular (random) order where the probability that a high, or a low tone, will be presented on any trial was equal to .5 and was independent of the outcome of the previous trial.

The entire experimental structure can be seen in Table I. There are two main experimental conditions, the RT and the P condition, which differed in the task the subject had to perform following the stimulus. In each condition the imperative stimuli could have been presented in a Random (RAN) or an Alternating (ALT) series and in each of these cases a warning tone may (W) or may not (NW) have been used. As indicated in the Table, the experimental design is predicated on the assumption that the presence or absence of the warning stimulus determines the presence or absence of a CNV. Furthermore, we assume that the predictability of the stimuli will affect the amplitude of the P300 component they elicit. We included the Predict condition because data collected during the pilot runs, and recent reports (Donchin et al., 1973; Tueting and Sutton, this volume) indicated that, at least under some circumstances, the effect of stimulus predictability on the amplitude of P300 is diminished in a reaction time experiment. We felt that it is

important to have an experimental condition in which large P300 differences could be indubitably recorded.

In all of the experimental sessions covered in this report RT runs always preceded P runs. RAN series always preceded ALT series. Within each series the alternation between the warned and the unwarned conditions took place on a random basis every one to three minutes.

There is a clear need to counterbalance the order with which different combination of values of the independent variables are used on successive series. This was however clearly impossible using one subject during one session.

Each experimental trial was initiated by the experimenter. Trial initiation caused the PDP12 to select the appropriate sequence for the trial events to generate the stimuli and record the subject's responses. The EEG data were digitized on-line and stored in the computer's memory. A display of two channels, commonly the EOG and the vertex channel, were presented to the experimenter on a television monitor. The experimenter could then judge whether to retain this trial's data on tape or to "cancel" the trial. Trials were cancelled whenever the data were contaminated by large eye movements or blinks. These could be detected readily as large deviations of the waveform of the ECG record. Data from noncancelled trials were stored on digital tape for subsequent off-line processing. The EEG data were stored in conjunction with information detailing the stimuli and responses generated during the trial.

Data Analysis: As can be seen from Table I for any one electrode location we could record eight different evoked responses, for each of the eight channels.

All of these averages were obtained off-line by averaging the records stored on the tape. Of course, the subjects received high and low tones and responded with their right or left hand thus providing additional criteria

for a classification of trials and the generation of additional evoked responses. We did indeed average all reaction time trials separately for reactions produced with the right hand and reactions of the left hand thus yielding two averages for each of those indicated by Table I. Our examination of the data has shown but little difference between the right hand and the left hand response and we have therefore concentrated on the analysis of right hand response data. The subject's reaction time was recorded on all trials and stored on the tape with the EEG data. An analysis of these reaction times will be presented below.

The entire set of averages has been plotted to allow visual inspection. No other analysis of the data was attempted during the conference thus all statements in the succeeding paragraphs are derived solely from a visual inspection of the records.

RESULTS AND DISCUSSION

One methodological problem which has been highlighted by the data collected from this subject is that of defining P300 and the CNV. The problem is best illustrated with reference to Figure 1, in which we present an evoked response

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Insert Figure 1 here

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obtained during a Warned Irregular series. A CNV clearly develops between the warning and the imperative stimulus. It is followed by a positive going wave. Is this post-CNV wave a P300? The answer depends on the definition of "positivity". There were those who felt that components are positive if, and only if, a positive polarity is observed relative to a base-line. Others felt that only local direction of the polarity is important without considering absolute base-lines. Often a base-line is defined in terms of the activity prior to the warning stimulus. In the data of Figure 1, and in all

other data recorded from this subject - the post CNV component never "recrossed the base-line". In these terms, there is no P300 in Figure 1. The members of the working group rejected this point of view. To our mind, the pre-S1 level does not, necessarily, define a base-line with respect to which the polarity of the component should be evaluated. It seems equally reasonable to assume that the large negative shift, which is the CNV, cancels the base-line processes represented by the pre-warning stimulus activity and establishes new conditions. Components should then be classified as positive or negative depending on their local direction. Thus a positive going component, even though it never recrosses the "old" base-line, is a serious candidate for being considered a P300 wave.

There is a more appropriate way to our mind to evaluate whether or not an evoked response component, is or is not P300. It seems to us that the crucial consideration is the degree to which the amplitude of a component is manipulated by variables which are known to manipulate the amplitude of P300. Thus it seems that a comparison such as is presented in Figure 1B provides a better definition of P300. In that figure we superimpose the data obtained during the Predict series, on Unwarned trials, for regular (ALT) and irregular (RAN) sequence of stimuli. Clearly, there is a major difference between the two curves. The curve obtained during the RAN series is characterized by large positive going waves with a latency 300 milliseconds following the stimulus. This component is missing in the ALT series. These data, of course, replicate previously reported data. What we propose is that it is legitimate to consider these two records in Figure 1B as defining a sort of P300 template, a "pure" P300 which can now be used to determine the presence or absence of P300 in other evoked response curves. In Figure 1D we superimpose the difference between the two PR curves of Figure 1B, on the data obtained during the RT. We find that a component much like the one

recorded during the PR conditions appears during the RT condition. It would seem unreasonable not to consider it a P300 even though this component never crosses the base line. It seems then that dependence upon pre-S1 base-lines in defining polarity may be misleading. It seems more reasonable to interpret the direction of polarity reversal rather than its absolute level, especially when dealing with potentials following a CNV.

An interesting methodological issue well illustrated by the data obtained from the present subject has to do with interpretation of EOG records. In Figure 2A we show potentials recorded from several locations on the subject's

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Insert Figure 2 here

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head during one of our earlier sessions. The EOG was recorded between a supra-orbital and canthal electrode. A large potential can be observed at that electrode and a, disturbingly, corresponding potential clearly appears in most other cortical locations. We can now adopt two different strategies. A very cautious attitude will suggest that any potentials which appear in the EOG location, and correspond to potentials recorded at scalp locations are suspect. The fact that we can record a sharp negativity at the EOG channel should then be interpreted as indicating a need to disregard all the concurrently recorded potentials. It does not immediately follow however that all potentials recorded at the EOG electrodes are necessarily Electroculographic potentials. It is quite possible that the electrode placed supra-orbitally reflects cortical activity recordable at that location. What criteria can one use to determine if the recording in question is electroculographic or electroencephalographic?

By way of an answer we note two reasons which led us to conclude that the potentials recorded at the eye electrodes are representations of cortical potentials. First, we note that the latency of the peak negativity at the

various scalp locations varies. In fact, it is clear that the negative peak does not occur simultaneously at all scalp locations. Its appearance at the vertex procedes its appearance in the EOG electrode. It would seem to us that all potentials that are recordable at scalp electrodes as a result of electroculographic activity ought to be simultaneous or at least ought to show a spread from the front backwards.

Equally convincing is the fact that the amplitude of the potential does not show a Fronto-posterior distribution. In fact, the amplitude tends to be larger at the more posterior electrodes, a distribution unlikely in an electroculographic potential. To check on the validity of these considerations we have placed electrodes in a subsequent session in a somewhat different manner. We retained a canthal electrode but instead of a supra-orbital electrode we placed an infra-orbital electrode. The results can be seen in Figure 2B. This data obtained in a similar condition to that in Figure 2A, show that the appearance of the scalp electrodes is hardly changed, yet no corresponding potentials appear at the oculographic channel. The main thesis we are presenting, therefore, is that electroculographic records must be interpreted with care. It would be misleading to assume that all potentials which are apparently synchronous at the scalp electrode and at the eye electrode are necessarily reflections of the EOG.

In evaluating the results the working group discussed in detail the optimal electrode montage during CNV, and P300, experiments. A description of desired electrode locations must take into account the fact that the number of recording channels available to different investigators is different. We have, therefore, ranked the desired electrode locations, indicating which electrodes must be used in a minimum configuration and which should be added as the number of channels is increased.

We are assuming that no laboratory has less than 2 recording channels.

We feel that in a two channel system the investigator must use one channel for recording ECG. The other channel should be used for the vertex (Cz). The vertex electrode is, in fact, a less informative recording location than lateral placements. However, interlaboratory comparisons require a vertex placement. Lateral locations provide useful information about the distribution of the potentials and about hemispheric asymmetries. It becomes increasingly obvious that in the interpretation of electrocortical potentials the distribution over the head is of crucial importance (Donchin, 1973). Thus, any placement which maximizes distributional information should be preferred over placement at the mid-line. However, if only one Central electrode must be used we would recommend a vertex placement if only to assure comparability of recordings between laboratories.

The working group feels that Parietal placements (P3, P4) are preferable to Central placements, (C3, C4) at least in the study of P300. The component does seem to have a larger Parietal than Central focus (Hillyard et al., this volume). Again we recommend that if two channels can be devoted to Parietal recording the electrodes should be placed at P3 or P4. If only one Parietal channel can be recorded Pz should be preferred. Following are Frontal electrodes, with F3 and F4 to be preferred if two channels are available for frontal derivations, Fz if only one channel is available. Occipital electrodes are next in order with O_1 or O_2 to be preferred. If only one occipital electrode can be afforded O_2 or O_1 are each preferable to Oz. The primary function of occipital electrodes in these experiments is to provide a check on the quality of the experimental data collection procedures. If no prominent visual stimuli are elicitable at the occipital electrode the investigator can safely infer that something is wrong with his recording procedures, either his electrodes, his stimulation equipment, his subject or with his experimental design are faulty. The occipital electrode at the O_1 or O_2 location

will maximize the amplitude of the potentials recorded and is thus preferable. In summary then, the electrode ranking we suggest is that EOG and Cz be always recorded following which P3 and P4 (or Pz), C3 and C4, then F3 and F4 (or Fz) and O_1 and O_2, or O_1 by itself. If the investigator has channels to spare placements at T5 or T6 can be made though we seriously doubt the usefulness of these placements. A related issue is, of course, that of the reference electrode. A strong sentiment was voiced against linked mastoids or the mastoid references in general. Members of the working group seem to feel that such placements are more likely to include contamination from skin and muscle potential sources, at least to a larger degree than reference electrodes placed on the ears. The suggestion was made that the upper pinnae of the ear should be preferred to the ear lobe. The pinnae, according to Dr. Donald, are far less subject to skin potential variability and produce a more stable reference. The use of linked references one on each of the ears is considered to be standard operating procedure. The use of noncephalic references is recommended by some workers, especially focussing on a placement on the collar bone and the neck bone.

We discussed in some detail the implications of the experimental design used in the Demonstration for the substantive issues discussed in the correspondence. Of course, members of the working group had various opinions concerning the interpretation of the data. We are, however, on purpose avoiding the presentation of these conclusions, for the obvious reason that no conclusions should be based on the data of one subject. There is, however, one major question which has been repeatedly discussed during the conference and which can be illustrated with reference to the data. It is primarily a design issue. This experiment was designed in order to determine, at least in part, whether the presence, or absence, of a CNV prior to a stimulus affects the degree to which that stimulus elicits a P300. We have therefore tried to vary

orthogonally variables controlling the presence or absence of P300 and variables controlling the presence and absence of the CNV. Presumably, in the absence of a warning stimulus no CNV will be recorded. This should permit us to observe changes in the potentials elicited by the imperative stimulus when it is supposed to elicit P300 when such an imperative stimulus is or is not preceded by a CNV. The nature of the comparisons we intended to make can be seen in Figure 3. We compare there the potentials recorded during an irregular

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Insert Figure 3 here

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series over all electrode locations, in the presence and absence of warning stimulus. That a CNV is lacking in the absence of a warning stimulus is clear. Equally clear is that a P300 is elicited in both experimental conditions. There are some differences in amplitude and waveform, however the basic similarity of the imperative stimulus evoked response in the two conditions is evident. Whether or not these data are of general validity remains to be seen. There is however an important logical point that needs to be underlined and which has been brought out by several participants in our discussions. At issue is the degree to which it is indeed reasonable to assume that the condition in which no imperative stimulus has been presented is indeed a "no CNV" condition.

It is of course unarguable that no "traditional" CNV is apparent in the records, in the sense that no negativity gradually develops preceding the onset of the imperative stimulus. There is, however, the possibility that in the absence of a warning stimulus the subject performs the task in question while generating, to use Dr. Wilkinson's apt phrase, a "resident CNV". In the correspondence above the possibility is discussed that CNVs of the rapidly rising wave shape are observed because of the nature of the

averaging process we use to elicit CNVs. They are characteristic of relatively short-interval processes in which the very development of the preparatory processes does not begin until the presence of a warning stimulus. When the subject knows he will be forewarned he can hold his preparation until the arrival of the warning stimulus. It is well known that the slope of the CNV is considerably reduced as the interstimulus interval is increased. What if the interval is very long, on the order of many seconds or perhaps minutes? Is the CNV-generating process of such a nature that it might be continuously activated at a very low slope, so to speak, so that the imperative stimuli are always presented during a CNV process though no CNV is recordable. It is clear that this possibility must be tested for. It is equally clear that no obvious methods for measuring the "resident" CNV have been proposed. The experiment we ran at Bristol could have provided the answer had we analyzed DC recordings of the potentials. However, this assumes that a resident CNV will reflect itself in a changing, shifting level of the DC potential over periods without warning-imperative stimulus episodes. We find it tantalizing that one could shift the records obtained with, and without, a warning stimulus in the manner shown in Figure 4. What we have

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Insert Figure 4 here

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done is superimposed the warned and un-warned records so as to maximize their overlap at the imperative stimulus evoked potentials (rather than at the pre-S1 base-line as we have done in Figure 3). What we find, in all the data obtained from the subject used in Bristol, is that the pre-imperative stimulus level in the un-warned case is considerably more "negative" than the pre-S1 base-line in the warned case. It is as if during the unwarned condition the EEG base-line has shifted in the negative direction. We

emphasize that we do not report this as a finding we merely wish to point out the logical difficulty with the implication that in the absence of a warning stimulus there is no CNV.

In summary, the working group felt that the experiment that has emerged from our efforts over the last several months is indeed relevant to a resolution of the basic issue of the relationship between the pre- and post-stimulus electrocortical activity. The experimental design is elaborate and many precautions must be kept in mind. We wish however to stress and underline two major aspects of this effort. There is a need to carefully design experiments so that variables affecting cortical potentials are carefully and systematically manipulated. This may require experimental designs of a complexity not previously used in evoked response research however the subtlety of the questions we must now cope with clearly call for such sophistication. Equally clear is the grave importance of the use of distributional data in analyzing cortical potentials.*

*A full report of a replication of this experiment, using 12 subjects, conducted during the Fall of 1973 is presented in Donchin, E., Tueting, P., Ritter, W., Kutas, M., and Heffley, E. On the Independence of the CNV and the P300 Components of the Human AEP, In preparation.

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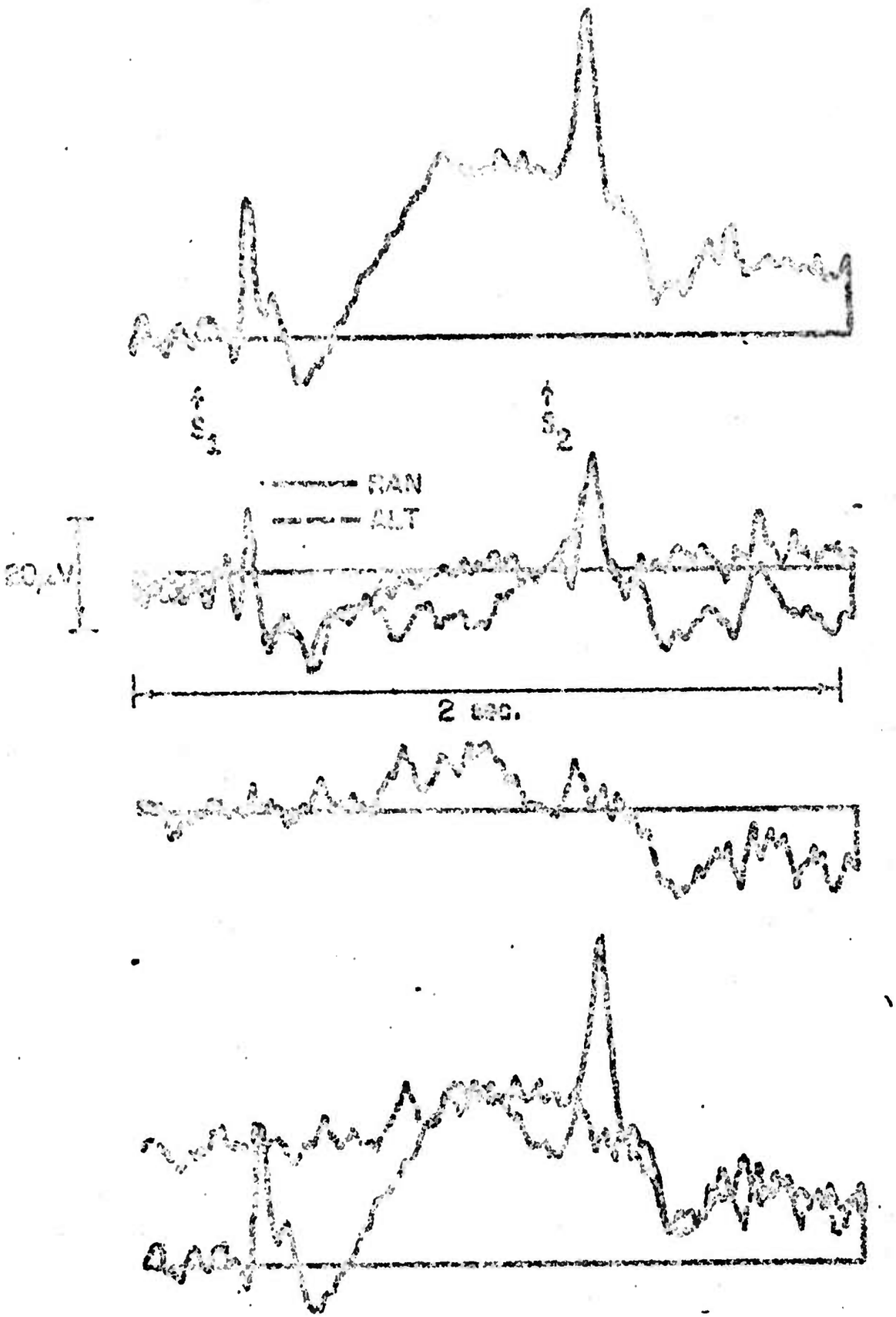
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TABLE I

Experimental Design

Condition	Stimulus Sequence	Pre-warning	Electrodes
Reaction Time	Random	Warn	Frontal Occipital Vertex Right Central Left Central Right Parietal Left Parietal EOG
		No Warn	
	Alternate	Warn	
		No Warn	
Predict	Random	Warn	(Repeated for all conditions)
		No Warn	
	Alternate	Warn	
		No Warn	



CORTICAL POTENTIALS (VERTEX)

Figure 1

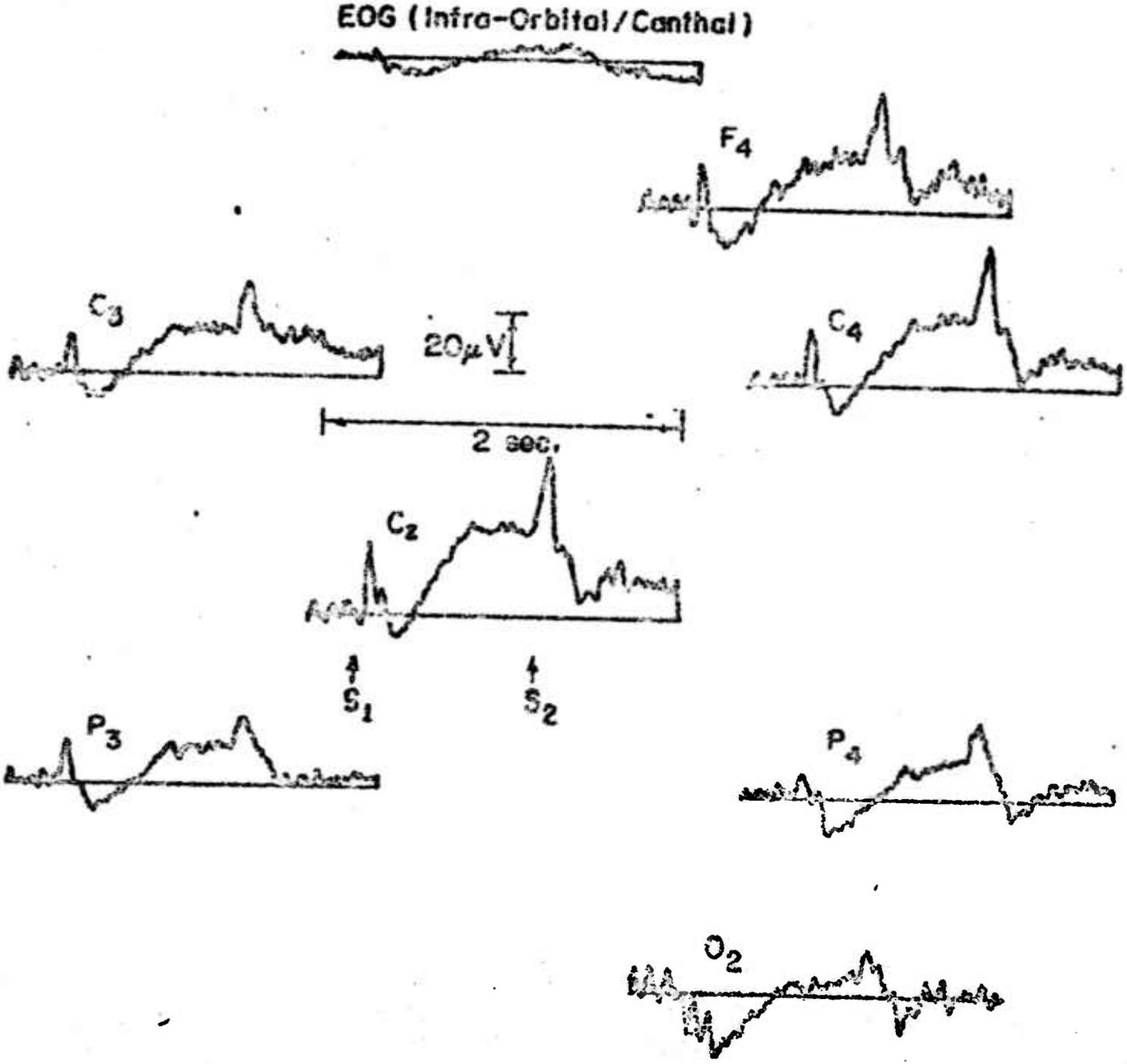

EOG (Infra-Orbital/Canthal)
F4
C3
20μV
C4
2 sec.
C2
S1
S2
P3
P4
O2
RT. RAN. WORKED. SESSION II

Figure 2a

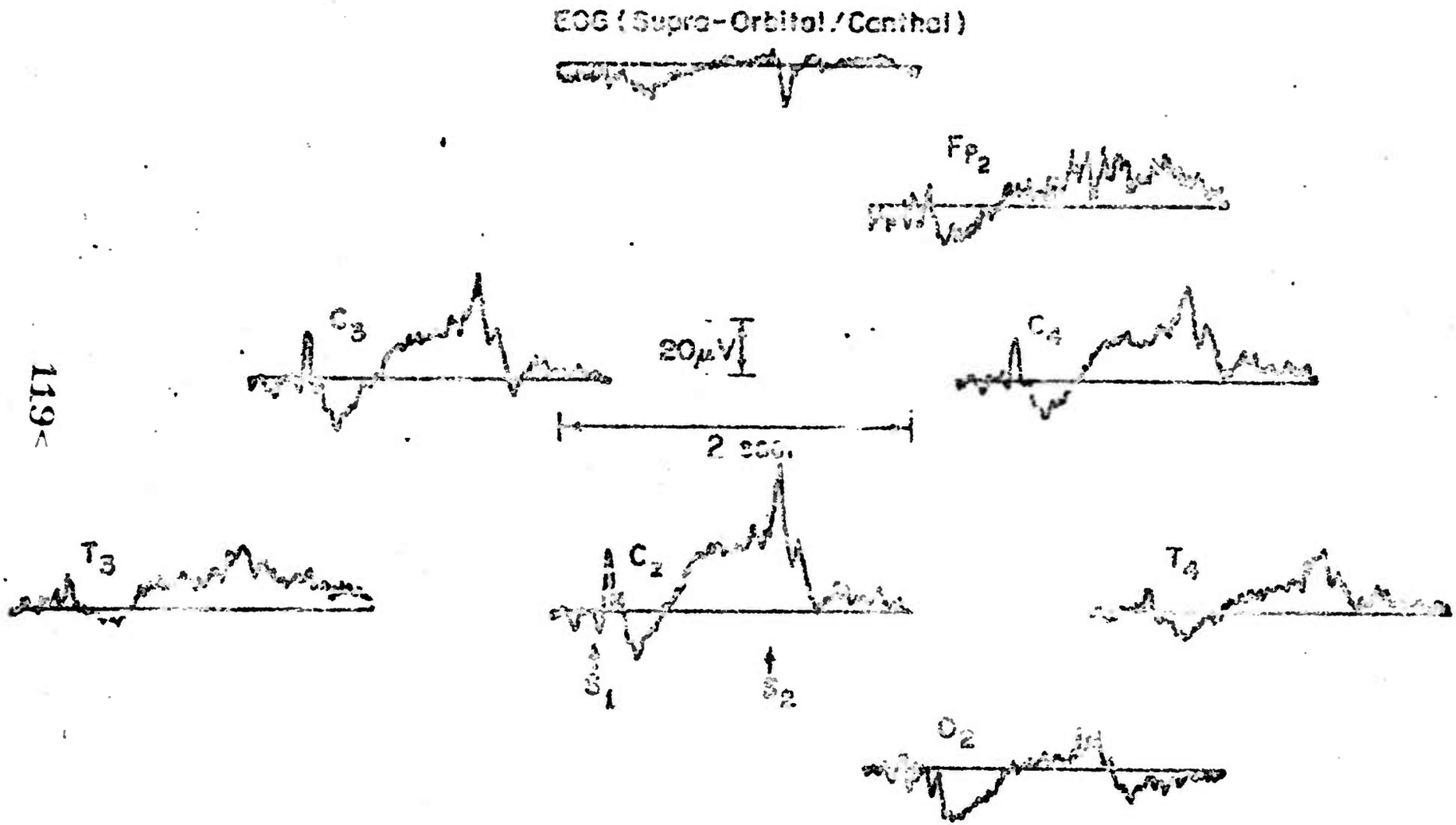
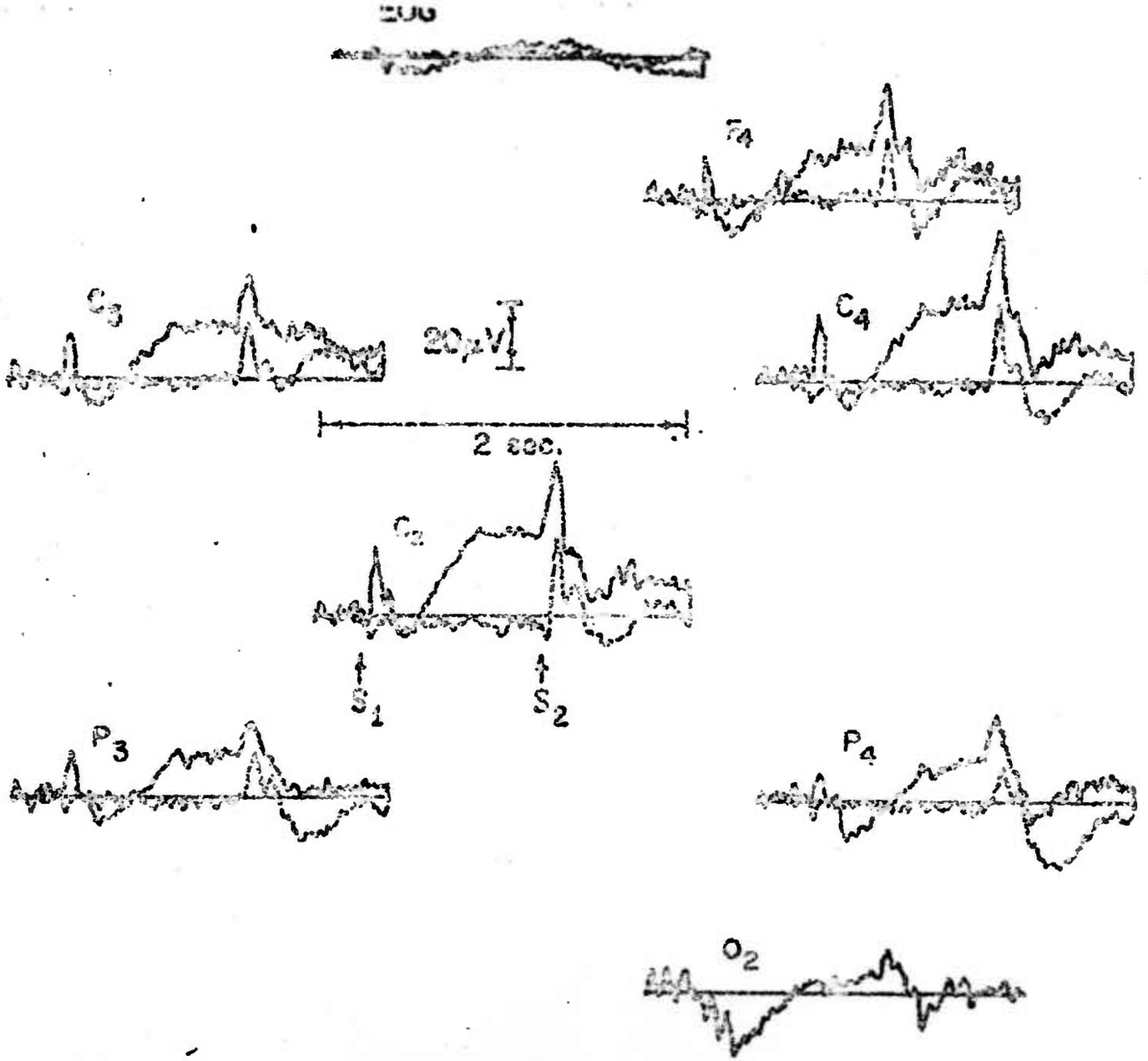


Figure 25

RT. RAN. WARNED, SESSION I



EFFECTS OF WARNING STIMULUS

Figure 3

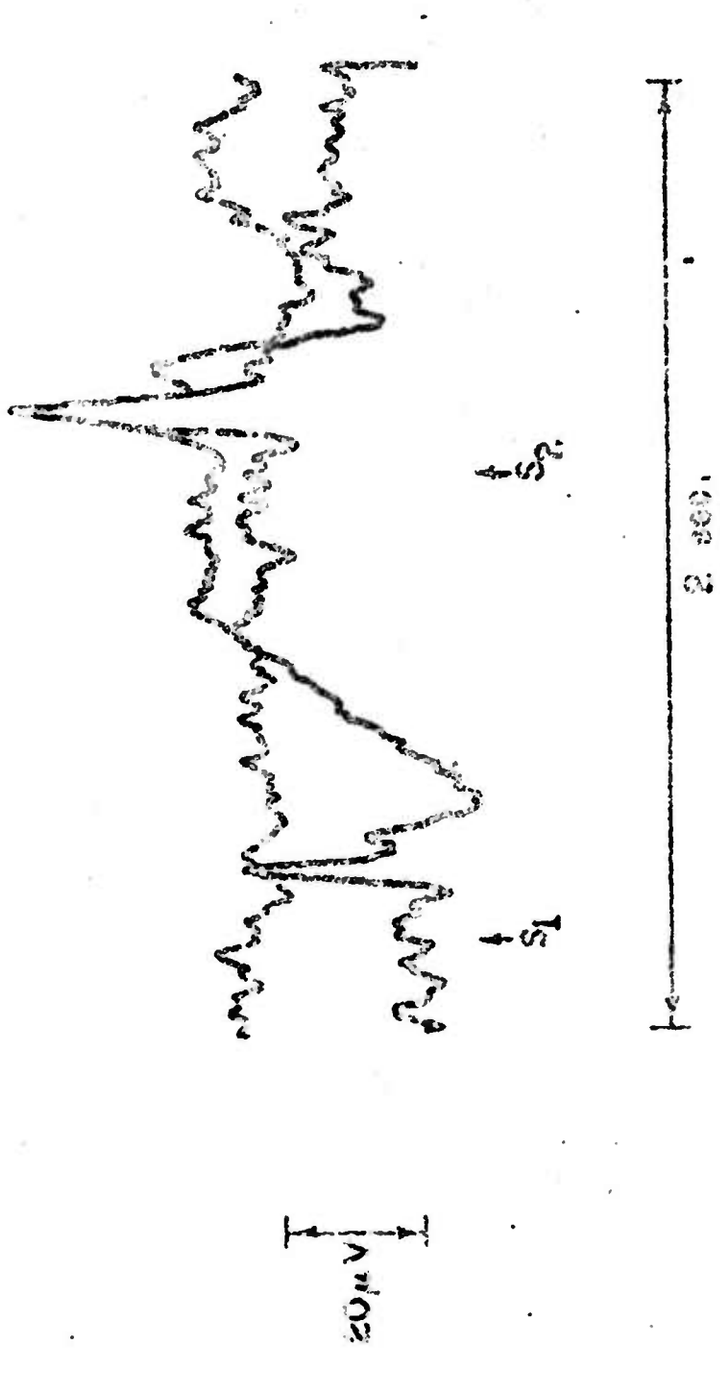


Figure 4

FIGURE LEGENDS

Figure 1: A. Average Evoked Potential (N = 16) recorded at the vertex from subject R, recorded during the Reaction Time (RT) condition under the "A" paradigm (warning stimulus used) using a random sequence of the two imperative stimuli. The baseline drawn through the data is the mean of the 500 milliseconds just prior to the warning stimulus.

B. Data recorded from the same subject from the same electrodes during a Predict (PR) condition with both an RAN and an ALT sequence of choice stimuli. Note the general reduction of the CNV in the PR condition and the enhanced P300 during the RAN condition.

C. A curve obtained by point by point subtraction of the random and alternating curves in Figure 1B.

D. The difference curve of Figure 1C is superimposed on the RT data of Figure 1A.

Figure 2: A. Data recorded during the RT condition in paradigm A, using random presentation of the stimuli, from eight different electrode locations. All cortical electrodes referred to a linked mastoid. The EOG is recorded in a bipolar mode. Note that with a supraorbital placement of one of the EOG electrodes a sharp deflection appears in the EOG record.

B. Data as in Figure 2A except that temporal placements have been replaced by parietal placement and an infraorbital rather than a supraorbital EOG electrode is used. Note the disappearance of the sharp component of the EOG electrode. The N1 component is still the same.

Figure 3: Cortical potentials recorded during the RT paradigm with random presentation of the choice stimuli with (solid line), and without (dashed line), a warning stimulus. Note the general similarity of the waveform elicited by the imperative stimulus in the two conditions.

Figure 4: The data shown in Figure 3 for the Cz electrodes are replotted superimposing the two curves not at the pre-S1 baseline but rather at the peak of N1 of the imperative stimulus evoked response.

COVARIATION OF THE MAGNITUDE OF THE CNV AND P300 AS A FUNCTION OF THE SUBJECT'S TASK

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This paper is concerned with the relationship between the CNV and P300 An opportunity to analyse jointly these two components of the human cortical evoked response was provided by an experiment, reported elsewhere (Donchin et al. in press), designed to assess the effects of cognitive complexity on the amplitude of P300. The subjects in that experiment were instructed to predict the outcomes of series of Bornoulli trials (trials which can have one of two possible outcomes). In each experimental trial a warning click was followed after 1400 msec by a flash of light illuminating either the letter A or the letter B, the letter defining the outcome of the trial. The subject had to predict the trial's outcome prior to the presentation of the warning click. From one experimental series to another the rule which determined the sequence of outcomes was varied, thereby varying the degree to which the subjects could utilize information from preceding trials in developing their predictions.

In the paper cited above we focused on P300 elicited by the flash and were able to demonstrate that its amplitude varies systematically with the sequence-generating rule. The experimental arrangement clearly allows a similar study of the effect of the same experimental variables on the CNV. Moreover, we can, by using these data, study conjoint changes in the CNV and in P300. Equally interesting is an analysis of the distribution of both components over the scalp, an analysis made possible by simultaneous recording from 8 electrode locations.

The interrelation between the CNV and P300 has been subject to considerable debate (Donchin and Smith 1970; Naatanen 1969; Donchin and Cohen 1969; Donald and Goff 1971). At issue is the degree to which the amplitude of the P300 component reflects differential pre-stimulus anticipatory processes as has been suggested by Naatanen (1969) or by Karlin (1970) rather than post-stimulus processing as proposed by others (Sutton et al. 1967; Donchin and Cohen 1967; Hillyard et al. 1971). A different, though related, issue concerns the degree to which the P300 component interacts with the positive going limb of the CNV. A detailed discussion of these issues appears elsewhere in this volume.

METHOD

The experimental procedures have been described in detail in our previous report. Briefly, 10 subjects were each presented with five series of trials. 64 trials per series. Each trial consisted of a warning

click followed after 1400 msec by a flash illuminating the letter A, or the letter B, in an Iconix Tacistoscope. For stimulus parameters and instrumentation details see Donchin et al. (in press).

The experimental series differed in the rule used to determine the sequence of outcomes. We used an Alternating series (ALT) in which the As and Bs alternated on successive trials. In the Learn series (LRN) the subject was handed prior to the series a sheet on which appeared a 9-element sequence of As and Bs. The subject was told that this sequence will be repeated in the coming series and that he is to memorise the sequence. In the Pattern (PAT) series a similar 9-element sequence was used, and the subject was so informed, except that he had to determine the elementary sequence from the presentations. In a series we called Shafty (SHF) the subject was again told that the series are generated by repeating an elementary sequence except that no such sequence was used, the series in fact being random. In the Random (RAN) series a random sequence of As and Bs was used, and the subject was so informed.

These five series were repeated in a counterbalanced order, under three different experimental regimes, each regime run on a different day. The following regimes were used:

1. Guess (GS). The subject was instructed to predict prior to the warning tone what the outcome of each trial would be. Proper care was taken to avoid contaminating the data by movement generated noise in the EEG. The subject received a financial bonus for guessing correctly on at least 60% of the trials.

2. Reaction time (RT). In this set of sessions the subjects were instructed to press one of two microswitches following the illumination of the letter. A right hand response followed the presentation of an A, a left hand response followed the presentation of a B. Both speed and accuracy were stressed, and slow responses were indicated by repeating the flash, incorrect responses were indicated by a loud buzzer.

3. Reaction time with Catch (RTC). These sessions were identical in all respects to the Reaction time sessions except that catch trials were embedded in the series. On 10% of the trials the warning click was not followed by a flash. The subject was abjured from responding in the absence of a stimulus. Catch trials did not disrupt the sequence generated in a specific series. The stimulus scheduled for the Catch trial was present on the following trial.

Eight Beckman Ag-AgCl electrodes were referred to linked ear electrodes. Electrodes were placed at Fp_z, F_z, T_3, C_3, C_z, C_4, T_4, O_2. EOGs were recorded from a pair of electrodes placed above, and to the right of, the subject's left eye. The EEG was amplified using Brush amplifiers (13-4218-00) and recorded on FM magnetic tape (Hewlett-Packard 3955). The system's bandwidth was .01-30 Hz.

All processing was performed off-line using the IBM 1800 computer. All averages considered here were obtained by averaging together the data from all subjects. A detailed analysis of the data obtained from the individual subjects is reported by Donchin et al. for the GS and the RTC condition. This analysis revealed a large measure of intersubject homogeneity which suggests that the overall averages used in this report are valid. We computed for each trace the area under the CNV and the P300 portions of the record. Integration was performed with respect to a base line defined as the Midmean of the 500 msec epoch just preceding the warning stimulus. (The Midmean, a statistic recently proposed by Tukey, is the arithmetic mean of the data falling into the interquartile range).

RESULTS

In Figure 1 we plot the area of the CNV and P300. Panels A and B present the areas for the Vertex electrode plotted as a function of the sequence generating rule (SGR) for each of the three experimental regimes. The other six panels plot the areas for all SGRs and experimental regimes (tasks) as a function of the location of the electrodes.

The data presented in Panel B, for the GS and the RTC task repeat in a condensed form the data presented in great detail by Donchin et al. (in press). The area of P300 is clearly a monotonic function of the SGR, from a minimum at ALT to a maximum at RAN. At the same time, when a motor response is required of the subject, with catch trials included in the series, P300 area does not vary with the SGR. The values for the RAN and ALT series are virtually identical. When catch trials are eliminated from the series the relation between P300 and the SGR is different than it is with the other two tasks. During RT the area of P300 during an ALT series is larger than it is during the RAN series. In fact, P300 area for ALT during an RT condition is quite similar to the area of P300 for the RAN series during the GS regime. (A full report of these data, including the

levels of statistical significance,are to be published elsewhere).

Panel A presents the corresponding areas of the CNV. Interestingly the CNV areas vary as a function of the various experimental variables in much the same fashion as do the areas of P300. CNV area increases with the complexity of the SGR in the GS condition. It shows an inverse relationship in the RT condition and seems to fluctuate in the RTC condition. In general CNV areas obtained during the RT series are larger than CNV areas obtained in the other two regimes except for the RAN series, where the GS and RT areas converge.

The correlation between the CNV and the P300 areas is indicated by the behaviour of these two measures along the interaural line as shown by the remaining six panels of this figure. Two aspects of these plots are worthy of note. The distribution of the areas within any of these panels seems to be quite homogeneous. That is, while there are clear differences between the distributions in the different panels, within each panel the relative magnitude of the CNV at each of the five electrode locations is similar. In other words, the tasks (GS-RT-RTC) affect the distribution of the CNV and P300 along the interaural line. The SGRs appear to modulate the amplitude of this distribution without affecting its shape.

Of particular interest is the fact that, while in most cases the area recorded at the Vertex electrode is larger than that recorded at the more lateral positions, there is a substantial difference in the amplitude at the lateral electrodes between the data recorded at the GS and the two RT conditions. In the GS condition the left electrodes yield a larger area than the right electrodes. This relationship is reversed during the two RT conditions. It is important to note that the above statement is equally correct for the P300 areas. The distribution of P300 and the CNV, at least along the interaural line, seems positively correlated.

The effect of the SGR on the overall amplitude of both the CNV and P300 varies among the six panels. There is very little difference between the P300 areas recorded during the RTC condition. On the other hand for the CNV the areas are least variable during the RT condition. The areas of the CNV recorded during the RT condition are consistently higher than those obtained during the RTC and GS conditions.

DISCUSSION

The above results indicate that by varying the subject's task it is possible to manipulate the distribution over the scalp of both the CNV and the P300 as well as the overall amplitude of these two components. Within each task, the magnitude of the components, though not their scalp distribution, is modulated by conditions under which the subject is performing the task. In two tasks requiring a motor response we find right-hemisphere potentials exceeding the left-hemisphere potentials. In one task, not requiring a motor response, this relationship is reversed. However, in all tasks we find that the amplitude of the distribution can be shifted up or down as a function of the complexity of the sequence generating rule used to determine the succession of trial outcomes. In any event, P300 and CNV areas appear to be affected in a similar manner by all these manipulations.

We speculated about the implications of the effects of the sequence on P300 amplitude in our previous report (Donchin et al. in press). For the present it is noteworthy that similar considerations might apply to the CNV. This of course revives the possibility that there is a strong relationship between these two components of the human evoked response. Needless to say, the demonstration of such a correlation does not imply a common causal source.

It is difficult to see why we should find the different tasks affecting the laterality of the CNV and P300. We are quite confident that this result is not due to any imbalances in our electrodes, amplifiers or recording system. That this is the case is attested by the fact that the evoked responses elicited by the warning click are virtually identical on the left and right hemispheres in all conditions. Had we changed in any way the parameters of our recording system between tasks, we should have seen a change in the relative amplitude of these evoked responses.

Why are the potentials recorded during the RT and RTC condition so different? The overall reduction in amplitudes during RTC is of course consistent with previous reports that diluting a series of RT trials leads to a reduction in CNV amplitude. Yet the dilution was only of the order of 10%, far less than is required to obtain "dilution" effects. It seems to us more likely that the differences in CNV amplitude are due to the relative useability of temporal information by the subject in performing his task.

Consider for example the ALT condition during the RT task. In this case the subject can successfully perform the task by estimating the interval between the warning and the imperative stimuli. As he can well predict which hand will respond, the stimulus in itself is less critical. In the RTC series such temporal information is less relevant in that the subject can not be sure that he will indeed have to respond. Similar considerations can be applied to the other combinations of RT by series used. What we are proposing is that CNV amplitude is strongly affected by the degree to which the information about the interval between the warning and the imperative stimulus can be utilised by the subject in optimising his task performance.

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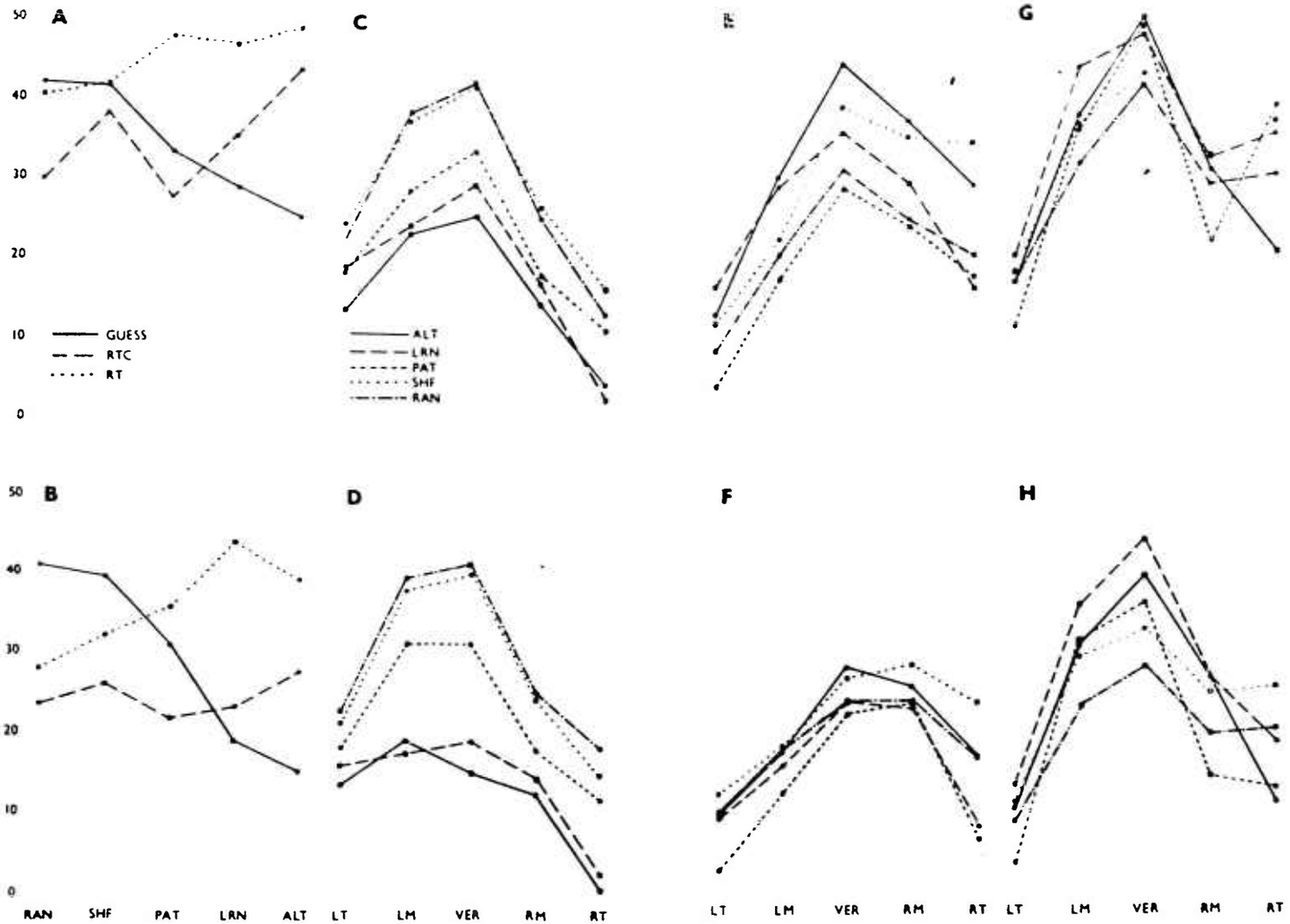
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Legend.

Figure 1. Each dot represents the area of either the CNV or the P300 integrated over a base line defined as the midmean of the 500 msec preceding the warning stimulus.

Panels A and B plot the areas of the CNV and P300 respectively as a function of the sequence generating rules, for each of the three tasks (GS, RT and RTC) for the Vertex electrode.

The remaining panels present the distribution of CNV (C,E,G) and P300 (D,F,H) areas along the interaural lines. The five lines in each panel represent the five sequence generating rules. Panels C,D represent the GS condition, E,F the RTC and G,H the RT conditions.


A
B
C
D
E
F
G
H
50
40
30
20
10
0
GUESS
RTC
RT
ALT
LRN
PAT
SHF
RAN
RAN SHF PAT LRN ALT
LT LM VER RM RT