

*File: LTRs  
Kurland*

December 23, 1971

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RE: MH18490-03 - Kurland

Dear Jack:

It was kind of you to give me an opportunity to see the renewal request and thus get a follow-up of the progress on this study. There has been much effort expended in planning and equipping the EEG analytic portions of the study; the promised participation of an electroencephalographer. (Dr. Bohm) is no longer listed in the renewal or the progress report; and 16 patients have had records analyzed on 3 or 4 occasions during the ECT process. The principal focus seems to be on the EEG changes induced by ECT and this is reflected in the large supplementary request of \$36,000 for specialized EEG equipment.

A. Two observations have stimulated these workers. One is the increase in post-seizure alpha activity is related to a better clinical evaluation, and increased theta/delta activity with lesser degrees of improvement. This observation is reported as "contrary" to that of others. But, the study is not a replication, and many factors that influence the EEG expression are disregarded.

1. The EEG is recorded in the ten minutes before the ECT treatment is given, presumably in the treatment room. This is a time when anxiety may be intense - and which may alter the EEG. Prior workers did not use this method, probably because of its lack of stability. (Is it

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not also likely that many of the patients will have received some anxiolytic therapy before the treatment -- perhaps not for the psychotic depressives, but surely for those rated as depressive neurotics?)

2. In the EEG evaluation, a difference score is used, without any measure of variability. The sample size is 10 minutes, and considering the amount that must be artefact filled, it would be a surprise if the pre-sample, on which the results are highly dependent, is a reliable estimate. (One should expect an awareness of the variability of the EEG and estimates of variance to be included in the analyses).

3. The post-ECT assessment is at 48 hours. Realizing that there is a decay in the amount of slow waves, and that this is most rapid in the early treatments, the authors now suggest estimates continuously during and after the seizure for one hour; and again at 24 hours. Are they implying that this pilot work was wasted? Would it not have been useful to examine a few patients at various times and present this data for a new study?

4. The records were evaluated for normality. By whom? With what standards? One of the main reasons for the emphasis in the 1950's on quantitative measures for EEG evaluation was a difficulty to achieve agreement as to normality and abnormality among sophisticated EEG'ers. The authors have more than adequate equipment to provide pre-treatment estimates of the amount of various frequencies, the amount of variability in amplitudes, etc. - and to use statistical techniques to allow for the relationship between these measures and change with ECT or with clinical evaluations. But they eschew these techniques for an earlier technique, which I would consider less reliable.

B. The second focus is the isoelectric period after the seizure. This important observation follows the reports of Paul Blachly. The observation here is contrary to those reported by Blachly, and independently observed by my associates. Could it be that some of the isoelectricity that is measured is an artefact of the blockade of the amplifiers? This possibility is not mentioned. If the observation is valid, what possible explanations do they have, and how do they propose to examine it further?

C. Perhaps the biggest disappointment is in the behavioral observations. In any correlation, the highly sophisticated analyses of one variable (EEG) ought to be matched by equally detailed and time-related behavioral measures.

1. There is one clinical psychiatrist assigned to the project, so his observations must stand alone. He uses standard, gross measures of behavior and the results are couched in the grossest estimate of 'improvement'. Improvement in what? Mood? Affect? Sleep? What of the contamination of memory loss to the overall assessment?

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2, The authors use a fixed number of treatments. Under this condition, some patients may receive too many treatments and some too few. It is probable that psychotic depressives, the older patients, will respond to the extra treatments with memory loss. Hence, they may be evaluated poorly if memory is an integral part of the improvement evaluation. These patients may also show more slowing, for this correlation - increased EEG slowing and decay in memory function is well established.

3. Using their ratings for behavior, and assigning a value to the amount of 2-3 cps from the graphs, we find the following:

<u>Rating</u>	<u>(N)</u>	<u>Mean "delta"</u>
1	7	5.7
2=	1	2.0
3	2	3.0
4	2	7.5
5	3	7.4

I do not find their tentative conclusion, that delta increase is associated with lack of improvement to be well grounded.

Also, the behavioral measures are presented without making use of the statistical techniques available today.

This review is disappointing. Not because the authors have presented little data, for they have made a good effort to get started on an interesting problem. They have fulfilled some of the goals promised. But, the report shows a lack of involvement by clinical psychiatrists with sophistication and knowledge of the issue involved. The renewal focuses on gadgets - \$36,000 of equipment for engineers, and no funds for the researchers. Replacing some of the clinical funds and reducing the equipment would do little to improve the project, for it is clear that the important investigators are Seipel and Brown. If one had much money, perhaps the \$16,000 for each of 2 years may serve to keep the project going at its present level. The electroencephalographic portions are also weak, and this may reflect the lack of someone with personal experience or involvement with this instrument (the promised appointment of a neurologist-electroencephalographer did not materialize).

I regret that I am less than enthusiastic about this application. In my earlier review, I was optimistic that the problem may excite the staff to a productive involvement - but this has not occurred, and I am not sanguine that another two years would add much.

Sincerely yours,

Max Fink, M.D.  
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