[redacted] 25X1

March 12, 1969

TECHNICAL MEMORANDUM

TO: [redacted] 25X1

FROM:

SUBJECT: Comments on Project 6607 Report

REFERENCE: [redacted] 2300301-TM-69-2 25X1

1. I do not believe that the statement of objectives (Introduction) are sufficient. In particular, emphasis should be placed on the determination of photographically related error. I do not believe anyone would agree that the program was all-encompassing enough to answer question number 1. Question 3 has too many human factors overtones. It should be re-phrased. Question 4 was not an objective. The intent by changing resolution was to see if the basic conclusions were altered by changing the system performance in a known way. I am not sure question 5 was really an objective.

2. It would have helped if the pages had been numbered, even if by hand.

3. I still don't understand why they changed from 3 to 2 operators. I calculate that this should have changed the confidence interval by a factor of

$$\frac{\sqrt{3-1}}{\sqrt{2-1}}$$

or about 40%. I guess in most cases, this would not have overlapped the systematic error, so it really doesn't make much difference. I withdraw the comment. I would have liked to see a little more exploration of errors and confidence limits as a function of density with the sensitometric data superimposed.

4. Wording is not good in general. Page 2 of Section II is a good example. I found their definition of error a little confusing. After a while, I figured out that they generally referred to the random (precision-type) of error as "error". In this case, interaction of operator and photographic effects become lumped in the systematic error which is a dynamic type of error. The conclusion then is that a systematic error is introduced by the nature of the photography.

5. On page 2 of Section III, I do not feel that determining the existence of a "positive correlation" is a proper expression of objective. A much stronger relationship would be required to show that the operator was materially affected by the image structure and such a relationship was, in fact, shown.

6. It really wasn't necessary to make the $h(\mu) = h(-\mu)$ assumption in the microdensitometer section where the spread function correction was calculated. I think the recursive solution will still work.

7. I don't disagree with any of your comments.

8. As a general conclusion, I judge that this work is very significant. It shows that the situation-induced error is large compared to the precision involved in making repeated measurements by comparators in the presence of grain noise.

9. I would have liked to have seen an analysis for the three operators showing paired differences and an analysis of bias, if any, between operators, to support their arguments on confidence limits.

10. Figure 9, I think, shows a lower deviation for 8 than for 12 readings (I think, anyway; my copy is bad). What were the confidence limits on σ.

11. I wish they had used a more consistent verbal notation throughout. I got confused on some of the errors. Film random error was sometimes called deviation, for example.

12. The correction procedure is very clever and supports my earlier contention about the impact of image restoration on measurement.

13. I didn't care much for the format and content of the summary and conclusions section. It needs to be less wordy, more punchy and not based on the questions (which I think should be reworded anyway).

14. I do not think that dual gamma will affect these conclusions much for low contrast objects, and most small objects will fall into this category. If one can get some estimate of the spread function in the same general density and contrast range, correction could probably me made. For higher contrasts, problems will arise. I believe that a similar experiment should be conducted on dual gamma, using relatively low achieved ΔD's, say in the 0.5 range.

The basic phenomenon which causes the systematic error seems to involve the detailed image structure. Since we could not cover all cases of image structure anyway, the addition of dual gamma simply increases the size of an already very large matrix of possible responses. Consequently, I believe that dual gamma may change the conclusion about correction (at least in cases with larger ΔD or wherein the choice of spread function estimator is limited). The conclusion regarding an interaction between structure and measurement should not be affected. This is the fundamental result and is of the most importance to the mensuration field.