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The enclosed manuscript* has been submitted for publication in Applied Optics. I would appreciate your reviewing it and advising me with respect to its originality and merit for publication in the journal.

The referee is asked in his critical evaluation of a manuscript to consider the following aspects:

A - Merit, significance, and originality.

B - Clarity and conciseness (suggestions for expansion or contraction are in order. Introductory, historical or other non-technical portions should be particularly scrutinized.)

C - Form of presentation. Could long tables be replaced by figures? Are the figures all necessary? Would the abstract be meaningful in Physics Abstracts?

If the article is considered worthy of publication but the subject matter is not appropriate for Applied Optics, suggestions as to more suitable journals should be made. Does it belong in J. Opt. Soc. Am.?

In cases where the referee feels that the manuscript should be rejected or returned to the author for revision, it would be appreciated if the referee would send along with his report to the Editor an anonymous copy of the report which could be sent to the author. Additional frank comments for the Editor's guidance would be appreciated if the referee feels such are in order.

John N Howard

JOHN N. HOWARD
Editor, Applied Optics
Chief Scientist
Air Force Cambridge Research Labs.
Bedford, Mass. 01730

Answer
* If you will not be able to review this manuscript in the next two weeks, please return it to the Editor. It would be helpful if you could suggest an alternate reader.

Please return the manuscript with your evaluation.

Does this relate to your paper?
John Howard



NATIONAL AERONAUTICS AND SPACE ADMINISTRATION
GODDARD SPACE FLIGHT CENTER
GREENBELT, MARYLAND 20771

February 20, 1969

Dr. John N. Howard
Editor, Applied Optics
Office of the Chief Scientist
A.F.C.R.L.
Bedford, Massachusetts 01730

Dear Dr. Howard:

I am enclosing herewith my review of "Determination of Extraterrestrial Solar Spectral Irradiance from a Research Aircraft" by J. C. Arvesen, R. N. Griffin, Jr., and B. D. Pearson, Jr.

Let me hope that these comments conform on the whole to what you expect from the reviewers and that where revisions are suggested they are sufficiently clear. If any point needs further clarification, please do not hesitate to write to me.

With best regards,

Sincerely yours,

M. P. Thekaekara

Enclosure

Comments by a Reviewer on "Determination of Extraterrestrial Solar Spectral Irradiance from a Research Aircraft" by J. C. Arvesen, R. N. Griffin, Jr., and B. D. Pearson, Jr.

The paper contains a significant contribution to the knowledge of the solar spectrum for zero air mass. There is considerable originality in the computer analysis of the data. The presentation is sufficiently clear and concise. The historical portion requires a minor revision. A few suggestions for modification, revision or omission are offered below. Except for two sources of error, air mass and transmittance of quartz, which can readily be corrected, these suggestions cover relatively minor points and do not detract from the merit of the paper. It was a highly complex instrument used with a great deal of experimental skill. Much painstaking labor went into the analysis of the data.

Page 5. line 3: needs to be modified. Moon's analysis of 1940 (Ref. 9) was based on a great deal more of data than the Smithsonian measurements of 1921-22 (Ref. 10).

P. 5 l. 15 sqq.: need to be modified. Nicolet's procedure was quite different from the one described here (See Ref. 7). It would have been quite erroneous to normalize the continuum level to the Smithsonian value. The continuum between Fraunhofer lines is measured at the center of the solar disc. Corrections are made for limb darkening and an average for the whole disc is obtained. Fraunhofer absorption is deducted. Area under the new curve is compared with the Smithsonian value. Considerable weight is given to more recent European measurements.

P. 6 Fig. 1: Is Fig. 1 necessary? The two curves are shown again in Figs. 10 and 12.

P. 9: Fig. 4 might well be omitted - a half-tone; adds little new information.

P. 14: Fig. 5 should be omitted. This lamp is sufficiently well known; and further the Cary 14 lamp within an enclosure has a different spectral irradiance curve.

P. 15 l. 11: Resolving power as understood by the author should be explained; obviously it is not $\lambda/\Delta\lambda$. (See Jenkins and White, Fundamentals of Optics, p. 276). Resolving power in units of distance is valid for telescopes and microscopes, but not, I think, for spectrographs. P. 15, l. 20: The divergent beam from the tungsten lamp illumines a larger area of probably different reflectance on the inner surface of the integrating sphere. Can

0.2 percent accuracy (p.17, 1.9) be claimed under this arrangement?

P. 19, 1. 18: The author states on page 19 "As shown by Kasten, the greatest contribution to these corrections results from the lower atmosphere. At altitudes above 11.6 km, the total correction is smaller than 0.5% for $0^\circ < Z < 80^\circ$." But eq. (3) of p. 20 yields a correction about six times greater. To take a numerical example, let $\frac{P}{P_0} = 1/5$; $Z = 79.72^\circ$. $\frac{P}{P_0} \sec Z = 1.12064$. According to the

formula used by the author, $M = \frac{P}{P_0} M_k = 1.08790$, which introduces a

correction of 3% since $\frac{1.12064}{1.08790} = 1.03$. A correct formula can be

obtained by applying Kasten's method of integration to the atmosphere above 11.6 km. A high order approximation to a correct formula is given by the equation $M = \frac{P}{P_0} (0.8 \sec Z + 0.2 M_k)$, which gives for this

case $M = 1.11410$. The ratio $\frac{1.12064}{1.1141} = 1.006$, so that the correction

is 0.6%. The author apparently assumed that the over-correction of Kasten's formula can be diminished by taking the product $\frac{P}{P_0} M_k$, and

this is not the case. This introduces an error in all the Langley plots by shifting the points for large air mass significantly toward the Y-axis. Thus, the author gets for the slope of the lines and zero air mass irradiance values higher than what they should be. Since the points for low air mass all lie very close together and the least squares method gives greater weight to large values of air mass, the error may well be quite large. This is a systematic error and is biased in the same direction for all wavelengths. Apart from this effect on the final result, objection can be raised that eqn (3) of p. 20 and last line of p. 19 are in mutual disagreement.

P. 20. 1. 20: Ref. 15 is Vigroux; change 15 to 14. P. 21, 1. 20: It would seem that the evidence for transmittance being 0.99 is insufficient. Since each scan takes about two hours, there is one data point per flight for the Langley plots. Effects of the variation of water vapor above the aircraft with latitude would not be observed under this method.

P. 25, 1. 7: Water vapor bands have been ignored or not observed.

P. 25, 1. 17: "Extrapolation 200% from sea level" - change preferably to: "For example the atmospheric transmittance from space down to 11.6 km is 82% as compared to 33% down to sea level."

P. 26, 1. 5: Change B1 to BI. This reviewer doubts whether Table BI, five pages long, 3500 three column entries, should be reproduced in the Journal. Comparison of Table BI with the Utrecht Atlas shows

that the wavelengths of several of the major absorption minima are displaced by more than $\pm 0.1 \text{ nm}$. "Spectral resolution" attainable by the instrument may well be 0.1 nm ; the dips as shown by Table BI show that the resolution actually achieved was 0.3 or 0.4 nm . The table apparently carried with it the random errors due to computer generated Langley plots.

If the data were sufficiently accurate the excursions of the curve from a smooth blackbody continuum should be due to merging of the Fraunhofer lines, and comparison with the Utrecht Atlas shows that this is not the case. Since over 95% of the Fraunhofer lines have half-widths less than 0.01 nm , many as low as 0.001 nm , a listing at 0.1 nm is not representative of the Fraunhofer structure. Perhaps Table BI should appear in a Research Report and not in a journal article.

P. 30, l. 8 and 9: Is not the primary uncertainty rather in having only about 2 data points for zenith angle above 60° , air mass above 0.4, to permit reliable Langley plots?

P. 31 The Y-values of Figs. 10, 12 and 14 are about 20 percent less than in Fig. 13 and Tables III and BI; shouldn't the y-scale be changed?

P. 38, l. 11: change to "joint total radiometer experiment by Jet Propulsion Laboratory and Eppley Laboratory aboard ..." Note: The article discusses the results not only of X-15, but also of B-57B and CV 990.

P. 40, l. 15: Change 94 to 95. Table III shows that the energy in the range 300 to 2500 nm is 95.51% of the total.

P. 44; P. 46: Are the half tone figures necessary? Can they be replaced by line drawings?

P. 47, l. 18: Change "et al." to "et al."

P. 49, l. 5: Change "is" to "are"

P. 50, l. 10: "A view of the Fraunhofer absorption structure resulting from ionized atoms in the solar atmosphere is shown in Fig. 18." Figure 18 cannot truly be called a "view of the Fraunhofer structure". There are some 22,000 listed lines; the "faint lines" are probably five times more. Fraunhofer lines clearly identifiable in Figure 20 are perhaps less than 20.

Nor is it true that Fraunhofer structure results from ionized atoms. The vast majority of the lines are due to neutral atoms; the first spectra of Fe, Ti, Zr, etc. are the most abundant.

P. 50, l. 20: The numerical examples might well be omitted. The application of the method is rather obvious.

P. 51, last line: Change B1 to BI.

Pp. 53-57: The references need some minor revisions to supply missing information and to conform to A.I.P. Style Manual. Omit titles of journal articles, unless Editor decides otherwise - Refs. 2,6,7,8,9,10,11,12,15,16,17,21,22,23,24,26.

If titles are kept, change Probleme to Problème in Ref. 7; change Extraterestie to Extraterrestre in Ref. 8.

For books and "unpublished" reports, add name of publisher and city; give page references where needed.

1. Add NASA Ames Research Center, Moffett Field, Cal.
3. Add NASA, Washington, D. C.
4. Add Institute of Environmental Sciences, Mt. Prospect., Ill.
5. Add NASA, Washington, D. C. Either add or give instead the more readily available reference, from the same author, Solar Energy 9, 7 (1965).
7. Change 14 (1951) to 14, 249 (1951)
8. Change III to A III
11. Delete No. 4
12. Place p. 279 last.
14. Add pp. 27-29.
15. Change 8 (1953) to 8, 709 (1953)
18. Add Hanover, N. H.
19. Add Bedford, Mass. after Res. Lab.
20. Add Washington, D. C. before No. 396
25. p. 36 should be placed last.
27. Add Pasadena, Cal. after 32-1263
28. Add p. 376
29. Delete No. 1
30. Add Book Company, Inc. New York before 1963; place p 6-103 last
31. Delete No. 11

P. 62: Transmittance of 2.5 cm fused silica - Table A1.

The values given by the author are higher than what is theoretically possible for quartz and is experimentally observed for thin plates of the same material. In the wavelength ranges where quartz is fully transparent and hence there is no decrease in transmittance due to increased thickness of the plate, thin and thick plates should have the same transmittance for a collimated beam of light. Losses are due to reflection at the two surfaces and hence transmittance is given by $\tau = \frac{2n}{n^2 + 1}$, where n is the refractive index. The values given

in Table A1 are higher than those given by the above equation by percentages varying from 0.9% at 450 nm to 0.7% at 1500 nm. The two sets of values have been compared only in the wavelength ranges outside the absorption bands of dynasil quartz. This discrepancy has

been studied rather carefully. Measurements made on plates of thickness about 2 mm yield values in close agreement ($\pm 0.5\%$) with the theoretical value. The same holds true for measurements made with relatively thicker plates with sunlight as the source. But laboratory measurements on thick plates yield highly divergent values, depending on the method of measurement. This has been the experience also of other experimenters whom we have consulted.

One possible explanation for the high values in Table AI is that the beam of light (see p. 42) was not sufficiently collimated. If τ and τ' are the transmittance values for a plate of thickness t as measured with a strictly collimated beam of light and with a point source of light at distance D , respectively, and n is the refractive index of light, it can be shown that

$$\frac{\tau'}{\tau} = 1 + \frac{2t}{Dn} (n - 1) \quad (1)$$

For $\lambda = 500 \text{ nm}$, $n = 1.46$; given $t = 2.5 \text{ cm}$, $D = 200 \text{ cm}$,

$$\frac{\tau'}{\tau} = 1.008$$

Thus a slight degree of decollimation as is due to a point source at 200 cm can cause an error of 0.8% in τ' . Further, a comparison of the observed and computed values at 500 nm and 1500 nm shows that the error is nearly proportional to $\frac{n-1}{n}$, as given by eqn (1).

Whether the above explanation is valid or not, it is certain that transmittance of a thick plate of quartz cannot be higher than that of a thin plate of the same material. There is a systematic error which can be detected readily in the wavelength ranges outside the absorption bands and must be assumed to extend to the whole range of measurement.

Suggestions: 1) Correct Table AI with the aid of values derived from the expression $\frac{2n}{n^2+1}$ 2) Make measurements under sun-

light with correction for circumsolar radiation 3) Make measurements with thin plates and extrapolate (rather difficult process!)

It would seem that all values of total and spectral irradiance must be increased by about 0.85%.

Conclusion: The above comments notwithstanding, the main conclusion of this reviewer is: an ambitious project, excellent experimental techniques, masterly use of high powered computers, clear presentation of material. Congratulations to the authors.

Comments for the Editor's Guidance

Mr. John Arvesen sent copies of his paper for comments to R. Kruger, C. H. Duncan and myself, and independent views from several members of our team have reached me.

"J. A. quotes results from different authors; all of them are in much closer agreement with each other than with J. A." said one.

Another remarked that the Cary 14 calibration is not based directly on the N.B.S. standard, contrary to what is stated on p. 15, l. 5. The Cary 14 lamp is in an enclosure and has its calibration from Cary with reference to an Eppley lamp. The Eppley lamp is referenced to the N.B.S. lamp. Thus it is a standard twice-removed. J. A. had planned to calibrate the Cary 14 lamp at N.B.S. after the flights, but the lamp burnt out during the flight period.

Statement on p. 13, l. 1, that the amount of precipitable water vapor above the aircraft ranged from 8×10^{-4} to 14×10^{-4} cm puzzles me. It is presumably based on a flight made by ESSA in September 1967 in the vicinity of Moffett Field. J. A. had called me about it and told me that from the depth of the H_2O band at 6μ ESSA had computed a value between 12×10^{-4} and 18×10^{-4} cm. Did they revise the figure later? At any rate, our data show that this figure is not valid given the wide range of latitudes covered by the NASA 711 Galileo flights.

One of our group remarked that the estimates given for accuracy and precision, for wavelength resolution and amount of scattered light in Cary 14 are highly optimistic. But J. A. believes these figures are OK and he has a right to publish them. The sensitivity of S. 5 surface of 1P28 tube which Arvesen used up to 700 nm falls rapidly at this extreme range, the values are (A per W) 0.03 at 500 nm, .005 at 600 nm, 0.0007 at 650 nm; at 700 nm the value is hardly ever listed. As for J.A.'s comments on the large disagreement between his results and ours in this range (p. 38, ll. 2 to 7): (1) Our four curves cross each other in this range; J.A.'s curve lies above all these; the differences between these are less than the difference between our weighted average and J. A.'s curve. (2) Could the relative insensitivity of the 1P28 and the uncertainty in the standard lamp be possible sources of error?

Please note my comments on References, pp. 53-57; your decision is requested as to whether titles of journal articles should be printed.

Museum's data

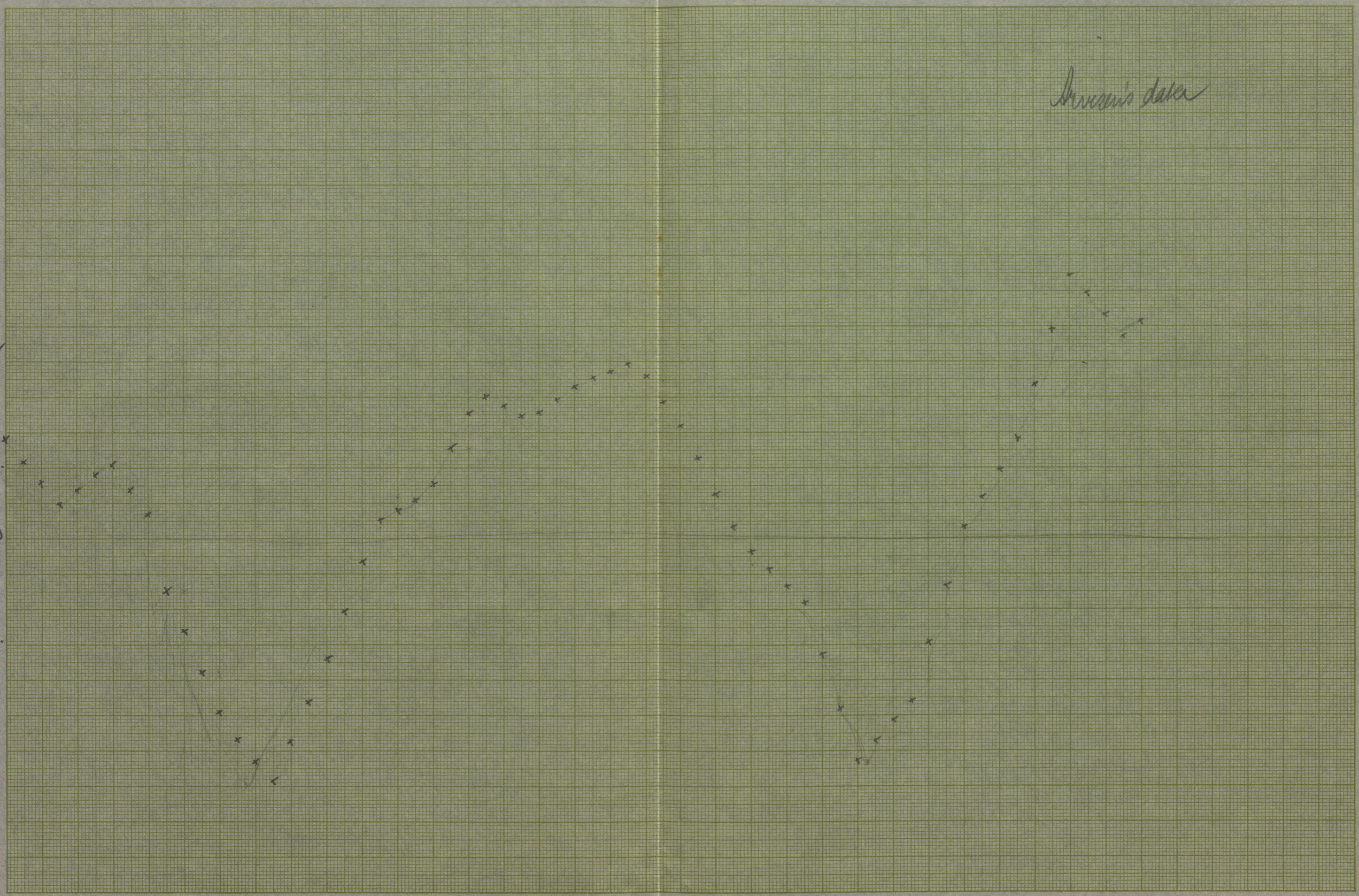
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1.0

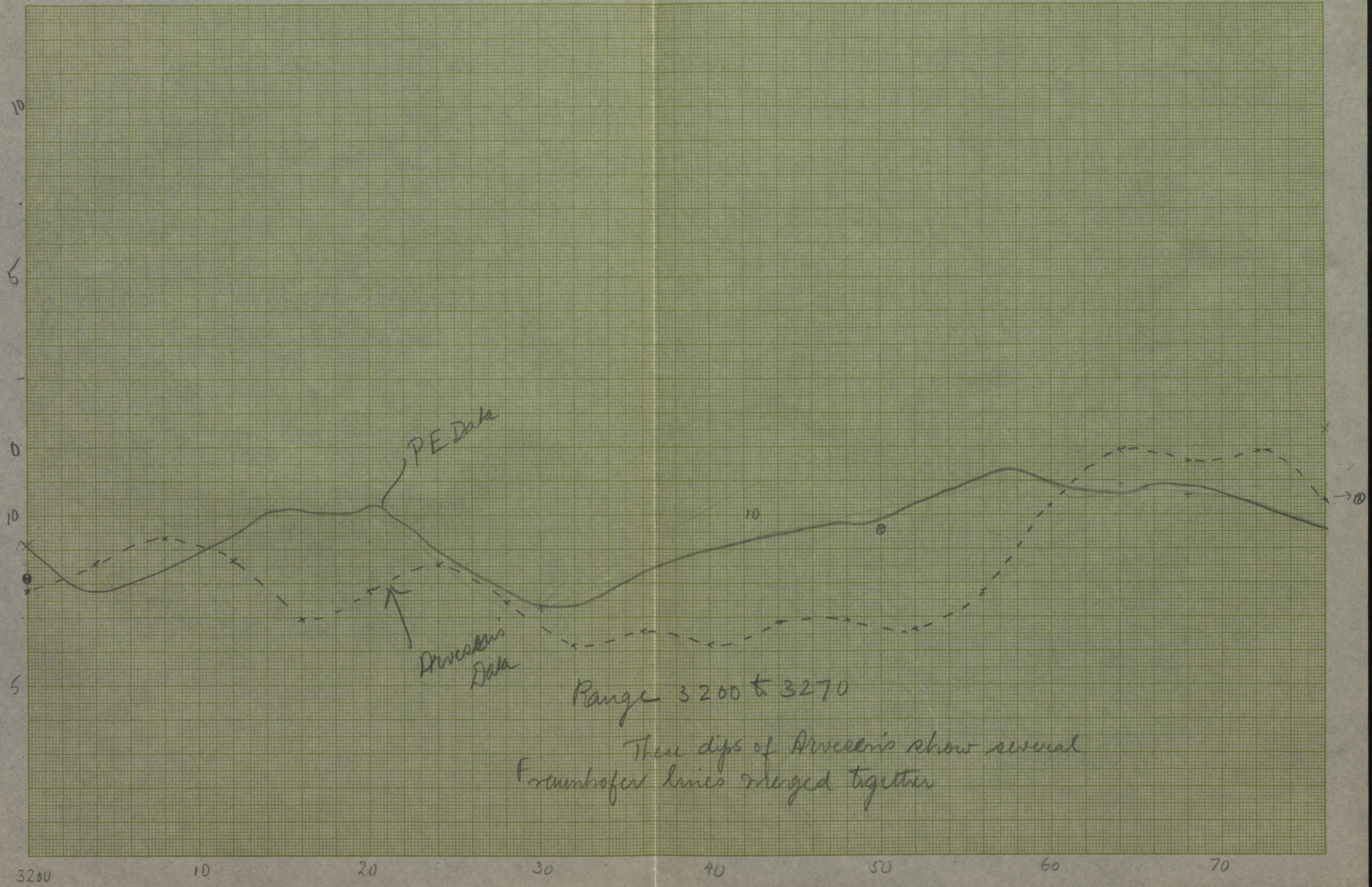
.5

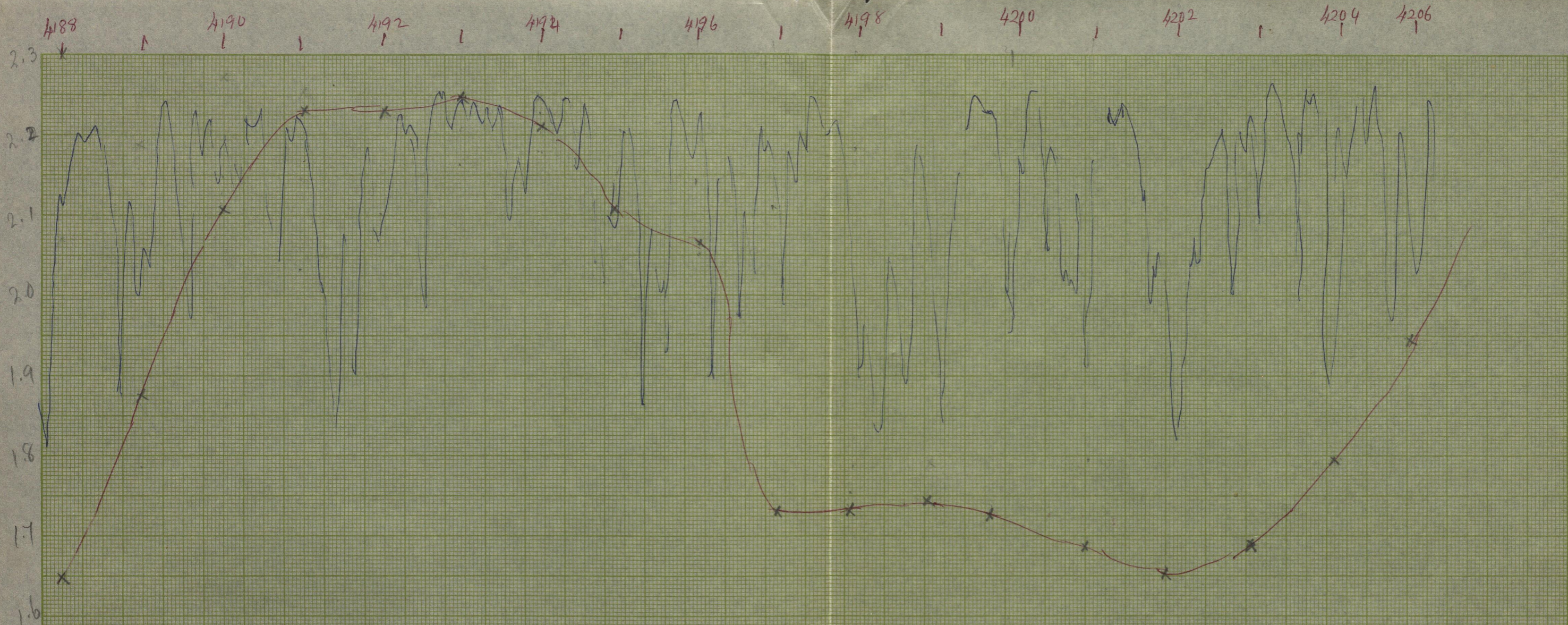
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KE 10 X 10 TO THE CENTIMETER 47 1516
29 X 30 CM. ALBANY, N.Y.
KEUFFEL & ESSER CO.



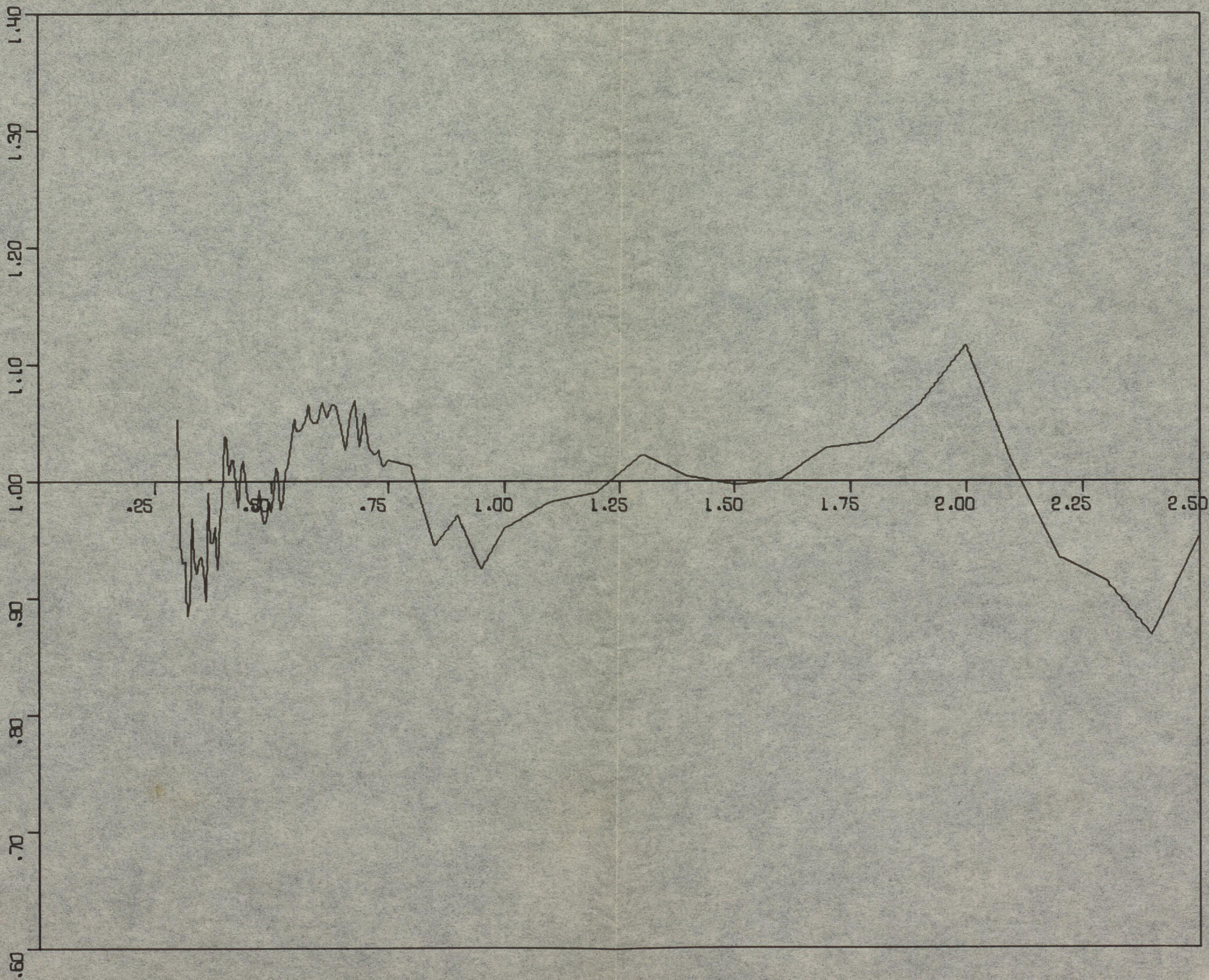
K&E 10 X 10 TO THE CENTIMETER 47 1516 MADE IN U.S.A. KEUFFEL & ESSER CO.

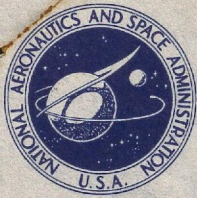




Comparison of Utrecht Atlas with Aderson's curve.

ARVESEN'S CURVE





NATIONAL AERONAUTICS AND SPACE ADMINISTRATION
AMES RESEARCH CENTER
MOFFETT FIELD, CALIFORNIA 94035

REPLY TO
ATTN OF: STP:234-1

August 19, 1970

Dr. Matthew P. Thekaekara
Goddard Space Flight Center
Code 322
Greenbelt, Maryland 20771

Dear Dr. Thekaekara:

As members of the IES Solar Radiation Committee we are presently charged with the responsibility of considering the establishment of a Standard of the Solar Constant and Solar Spectrum. I am herein forwarding to you for your consideration a copy of a paper delineating the results of our independent investigations.

Of considerable importance to committee action regarding adoption of a Standard for the Solar Spectrum is the fact that the National Bureau of Standards is presently working on redefining the primary Standard of Spectral Irradiance. This redefinition would affect all prior spectral irradiance values based upon the Standard. The proposed Standard for the Solar Constant would not be affected because its value is based upon independent total radiometric measurements.

I recommend that committee action on establishment of a Standard for the Solar Spectrum be postponed until the National Bureau of Standards has issued a corrected scale for the Standard of Spectral Irradiance and that consideration be given at that time to all available data.

John C. Arvesen
Member
IES Solar Radiation Committee

Memo

To Mr. C. Duncan

From M. P. Thekkarak

Subj: Solar Constant & Solar Spectrum

Dear Mr. Duncan:

This brief memo is with reference to the letter which Mr. Arvesen sent to you as also to several others including myself about the proposed standard values of the solar constant and zero air mass solar spectral irradiance. You were one of those mainly responsible for setting up this task group. We had a rather well-defined though difficult problem: to propose these standards. We had also a ~~xx~~ dead line to meet: the IES meeting in Boston in April 1970.

Two points were raised in Mr. Arvesen's letter, that his data from the Cary 14 should be considered and that in view of the possible revision of spectral irradiance standards of the NBS the proposed ~~the~~ solar curve should ~~be~~ await further corrections.

As for the first point, the Cary 14 ~~xx~~ results were carefully considered. In fact until the filter data of Eppley-JPL became available in February,

The GSFC curves from the four instruments and the Cary 14 curve were the only ones available from high altitude measurements. The differences were in the range 0.525 to ~~0.675~~ μ . ~~A weighted average of the set~~ Elsewhere the agreement was well within the range of error of the different instruments. In the range around 0.6 μ where ~~the~~ the four GSFC curves ~~the~~ crossed each other ~~and~~ and the Cary 14 curve was distinctly higher, the filter data were ~~in~~ in almost exact agreement with the final GSFC curve.

The ~~the~~ proposed standard curve is based on two independent scales, one ~~of~~ referred to the NBS quartz iodine lamp and the other referred to FPS 56. The Eppley GPL data are from a radiometer calibrated on FPS 56 and the filter factors are computed from transmittance curves and an idealized source spectrum. Thus there is no dependence on the NBS spectral irradiance scale.

The FPS 56 has ^{been} ~~an~~ international accepted internationally. No attempts have been made to compare the NBS spectral standard with similar standards

(*) spectroscopy to insist on agreement between three independent laboratories before a standard is accepted

used in laboratories outside the U.S. Attempts at a

revision of the NBS spectral standard ~~to~~ had been begun in 1966; no results have yet been announced.

It is difficult to say how ~~long~~ ^{many years} it will take before

an international spectral irradiance standard can

be established. It has been ~~the~~ ^a long ~~established~~ ^{standing} custom in (*)

Meanwhile should we be satisfied with the Johnson value of 139.5 mW cm^{-2} for the solar constant?

Should the ^{solar} spectral curve ~~be~~ continue to be a 6000°K

blackbody curve in the IR as was proposed by Parry

Moon in 1940 and was ~~also~~ adopted by F. S. Johnson?

The values which were proposed in ~~the~~ April 1970 at the IES meeting were based on ~~the~~ ~~best~~ data which were thought the best available at that time. No new data has since been published.

Matthew P. Sheehan

Memo

To Mr. Charles S. Duncan

From M. P. Takahara.

Ref. Arvesen's letter
Not used

With reference to the proposed standard values of the solar constant and solar spectrum Mr. J. C. Arvesen has written me a letter dated Aug 19. He asks that his results also be considered and ~~asks~~ that the ~~pro~~ definition of the spectrum be postponed. ^{This seems to be a} ~~A copy of the~~ ^{form} ~~letter~~ ^{which} was sent to Mr. Luce on Aug. 17, and perhaps also to you and to several others.

It seems to me that a few ^{are in order} comments on how the ~~of~~ standard values came to be proposed at the IES meeting at Boston on April 13, 1970.

Since ~~1956~~ 1954 the Johnson ~~value~~ ^{values} had been almost universally accepted in the U.S. and for the last six years at least, if not earlier, ~~more~~ most workers in the field had felt that these values needed significant revision for both the solar constant and the spectrum. The ~~Solar~~ Task Group on the ~~Solar~~ ~~File~~ this subject was formed ~~in~~ on June 25, 1969 under the sponsorship of the IES, Solar Radiation Simulation Committee. ^{I believe you} you were one of those mainly responsible for forming this task group. I was made ~~the chairman of~~ ^{Task Leader} ~~ness~~. We formed a small committee. We were given a very definite task: to propose standard values of the solar constant and zero air mass solar

spectral irradiance; we were asked to meet a dead line, namely, the IES meeting at Boston.

I believe that the standard values which were proposed are based on the best available evidence which was then available. Nor has any significant new evidence been obtained since.

These values have been proposed with a rather wide range of uncertainty. The large mass of data from different sources ~~and that~~ did not permit any narrower range.

A rather ^{detailed} ~~lengthy~~ paper published in the Proceedings of the IES Boston Meeting explains the thinking which preceded the ~~formation of~~ formulation of the new standards.

It is true that work is going on at the NBS on redefining the standard of spectral irradiance. This work started about ~~1955~~ 1965. It is in keeping with the great tradition of the NBS that ~~no publisher~~ to exercise extreme caution in replacing old standards with new. To my knowledge no date has been set for the replacement, nor are we quite sure that there will be one. Meanwhile should we still use 139.5 for the solar constant and continue with the assumption of 6000°K blackbody curve for the solar IR as was assumed by Moon in 1940 and adopted by Johnson?

^{a scale}
x Nor is it true that the proposed spectral curve is on
x based solely on the NBS spectral standard. ~~It was made of~~

1 amp

$$P = 6.08 \times 1$$

.4

.16

$$.16 \times 6.08$$

The Eppley-JPL filter data were also used. The scale for these is IPS 56 ~~of which~~ on which ~~the~~ is based the calibration of the detector. The ~~filter~~ filter factors are ~~to~~ computed from transmittance curves and an idealized source spectrum, without any reference to ~~the~~ a spectral standard.

The Cary 14 data (Atwood, Griffin and Pearson) were certainly studied very carefully before proposing the ^{new} ~~new~~ curve. A detailed comparison with the GSF curve was made. Cary 14 data were punched on cards and a normalized ratio graph was plotted.