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Maj. Gen. P. W. Clarkson, Commander, JTF-7 — 25 November 1953

A. C. Graves

COMMENTS ON THE WEATHER STUDY BY CDR. ELBERT W. PATE AND PROFESSOR CLARENCE E. PALMER DATED 30 JUNE 1953

J-22187

The subject report states conclusions which are not properly qualified, and others which cannot be made on the basis of experience gained in the operations of JTF-132.

The following comments are intended to supplement the conclusions of the subject report by specifying those conclusions which are not appropriate to Task Force experience and by giving sufficient qualification to the remaining conclusions to avoid misleading those who might have occasion to refer to the report. In view of this intent, it is requested that consideration be given to the attachment of this memorandum, with enclosure, to the subject report.

1. The title page indicates that the report is a joint effort of Cdr. Pate and Professor Palmer. This is not the case. Professor Palmer wrote part of Chapters 1 and 2 of the report. Chapters 3 and 4, and the Abstract and Conclusions were written by Cdr. Pate, and the combined report was not given to Palmer for review prior to issuance. A memorandum from Professor Palmer which clarifies the portion of the paper for which he accepts responsibility is enclosed.

2. The conclusion that "casual statistical analysis of the available weather records leads more often than not to erroneous operational conclusions" is obviously true. It is assumed, however, that this conclusion is not intended to imply that operational conclusions of Joint Task Forces engaged in the conduct of atomic tests were based on casual statistical analysis of available weather records. The fact that a reasonable number of operations have been brought to a successful conclusion without undue postponements or delays because of weather seems to me to be a sufficient indication that operational conclusions have, in general, been sound.

3. The conclusion that "operational weather requirements have been imposed . . . which are inherently inconsistent, almost mutually exclusive and capable of realization only for short periods separated by long intervals" is incompatible with the facts since the majority of overseas detonations have occurred on target dates selected months in advance, and since postponements because of weather have never amounted to more than a few days. It seems probable that the Task Force Weather Officer was not familiar with the real operational weather requirements of the Task Force. The latter conclusion is supported by the misstatement of requirements on past operations appearing in Section 3 on Page 8. For example, there has

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never been a requirement in past operations for "winds over the Marshall Islands up to 40,000 feet . . . from the southeast or south" or for "strong westerly winds (above 50 knots) above 20,000 feet in the Eniwetok-Bikini region". To my knowledge, no responsible member of any past Task Force has requested a twenty-four hour prediction of rain fall or cumulus clouds over a restricted locality. The last sentence of the section on Page 9 is an impertinence since the Atomic Energy Commission has never specified acceptable meteorological conditions to any Task Force. To my knowledge, the only requirements placed on the Weather Section during Operation Ivy were as follows:

a. To give weather outlooks at a number of times prior to a test.

b. To make a forecast the evening before a test.

c. To give a statement of the reliability of that forecast.

d. To indicate the next most probable weather conditions if the forecast were not verified.

e. To obtain weather data which would permit the Task Force to determine whether the forecast was, in fact, being verified or not.

f. To make routine forecasts for normal operations.

Once the Staff Weather Officer had filled these requirements, it was the responsibility of the Task Force Commander to determine that "suitable" weather would or would not be encountered on the day of the operation.

4. With regard to the conclusion that "it is entirely possible that a high yield detonation can 'trigger' a self-sustaining circulation which will derive its energy through the condensation process", I should like to point out the very different statement which appears in the report itself under '4' on Page 13 where the statement appears, "whether such a system could ever become self-perpetuating, through the supply of energy set free by condensation, is a matter about which it is impossible to reach definite conclusions as yet; at the same time, it must be said that the triggering of such a self-sustaining circulation is not at all impossible". I agree completely with this latter statement, but feel that a very different impression is conveyed to the reader by a statement that something "is not at all impossible" and a statement that something "is entirely possible". Moreover, the qualification contained on Page 13 that "it is impossible to reach definite conclusions as yet" is a qualification whose omission seems to me to be important. The further qualification appearing in the preceding sentence "provided larger bombs are exploded in an atmosphere with pronounced cyclonic shear" is also ignored.

5. The statement "analysis of bomb cloud dynamics points to the reason for the inadequacy of present techniques in high yield cloud sampling" assumes that present sampling techniques are inadequate for high yield devices. The evidence for this statement is not made clear anywhere

in the report. On the contrary, all evidence indicates that adequate samples were, in fact, obtained. Ivy experience (a) does not confirm that present sampling techniques are inadequate for sampling high yield weapons, (b) does not give reliable information on cloud turbulence, and (c) does not show that the bulk of the bomb material is carried into the stratosphere. Furthermore, the author, on Page 10, restricts his analysis of bomb cloud dynamics to "detonations at some distances above the ground", whereas, the Mike shot was a surface shot.

The statement made in the last sentence on Page 14 of the report that much bomb material may be carried up considerable distances into the stratosphere is very different from the statement that the bulk of the bomb material is forced into the stratosphere.

6. The reader is cautioned to read the second conclusion under the heading "With Respect To Evidence From High Yield Detonations" with the qualifications of the second and third sentences on Page 13 in mind. It is also constructive to compare the two reports of Mike weather given in Items 1 and 2 on Pages 16 and 17. The first report makes such statements as the following: "The general susceptibility of the atmosphere to convection is illustrated by the reports of cumulus and towering cumulus clouds at both Eniwetok and Kwajalein as well as by reports of showers and lightning". The second report that "personnel in the WB-29 informed me that there were only scattered low clouds in the vicinity of the atoll, whose bases were estimated near 1800 feet and whose average tops were estimated to be near 4,000 feet". Just why the author of the report chose to ignore the first of these reports, and base his conclusions completely on the second, is not clear from the report itself.

The conclusions on Page ii "With Respect to Certain Operational Problems" have been discussed above, however, it is worthwhile to point out that conclusion 2.b. on a high latitude, low stratosphere, continental, winter situation cannot be drawn from the experience of JTF-132 which was restricted to a low latitude, high stratosphere, mid-ocean, fall situation.

On Page 1 of the report, the authors point out the fallacy of making operational decisions on the basis of ideas solely conceived from meteorological experience obtained close to home. In view of the knowledge of this pitfall, it is surprising that, in this last conclusion, they would be guilty of committing the same fallacy in reverse.

Finally, it should be emphasized that, as stated on the title page, this is a report to the Commander of the Task Force, not by him. As such, it represents the opinion of two individuals and should be given weight accordingly. In my opinion, the portions written by Dr. Palmer are excellent and should be accepted as authoritative. Unfortunately, Cdr. Pate failed to be objective in those portions attributable to him and, for this reason, much of his part must be heavily discounted.

Original signed by
ALVIN C. GRAVES

ALVIN C. GRAVES

ACG:vw
Enclosure (1)

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By William W. Hesock 25 Aug. 1954
(Signature of person making the change, and date)
By Teresa E. Baca 27 Aug. 1954
(Signature of person making the change, and date)

Dr. A. C. Graves
Joint Task Force Seven
Washington 25, D. C.

Dear Dr. Graves:

Thank you for providing me with the opportunity of reading the definitive text of "A Study of Certain Operational Weather Considerations Involving the Test and Delivery of High Yield Weapons". Since I did not understand, at the time the manuscript was being prepared, that my name was to appear on the title page and since it would be an impertinence on my part to comment upon those matters in the study which lie outside my field of knowledge I should like to state my position unequivocally.

1. I wrote the sections "Weather over the Marshall Islands" (Chapter 1) and "The Dynamics of Bomb Clouds" (Chapter 11). I have not as yet any reason for repudiating the statements contained in these chapters. However, certain misprints occur and these should be corrected in accordance with paragraph 3 of this letter.

2. Any conclusions in the report, other than those contained in the chapters referred to above, lie outside my field of competence. For example, sampling techniques and the delivery problem are matters about which I have no knowledge.

3. On page 6 the diagram at the bottom of the page is out of register. The 10 knot isopleths surrounding minima of speed should lie over their respective singularities in the wind field. On page 11, line 4 should read "$\bar{T}_\alpha$, $\bar{T}_\beta$, $\bar{T}_\gamma$ represent the mean temperatures along the verticals AF, BE and CD,".

Sincerely yours,

/s/ Clarence E. Palmer

Clarence E. Palmer
Professor of Geophysics, UCLA

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Carl Wilson 6/1/83
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