sistant S. Dirmeier has improved and simplified the computer algorithm, extended the determination of the reaction coordinate analysis to simultaneous chemical reactions. Above all, he exemplified the impact of Prof. Straub's "nozzle differential equation" on the incorporation of the nozzle cooling which cannot be perfectly done for the common calculation procedure.

6) At a second workshop in November 1991 in Huntsville, Alabama, Prof. Straub reported on the background, fundamentals, and improvements of the Munich Method (MM). S. Gordon and Dr. Zeleznik, too, were among the participants. Here, for the first time, Prof. Straub received itemized information about a TM note[11] published 3 yr after the first NASA workshop. This Note deals with "Finite Area Combustor Theoretical Rocket Performance" with a new option to the worldwide accepted Lewis Code SP-273 that Dr. Zeleznik is now calling an "early version." The theoretical foundation of this computer algorithm is unsatisfactory concerning chemical equilibria under flow conditions. No critical explanation of the Lockheed concept is offered.

Why Dr. Zeleznik did submit voluminous polemic 4 wk later without even mentioning this workshop (on which there is a comprehensive MSFC report), I can only speculate.

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Reply by the Author to R. Waibel and S. Gordon

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MY evaluation of the Munich Method was written with exact references to all of the relevant literature and quotations, explicitly stated assumptions, and sufficient mathematical detail to enable any reader to verify the correctness of my analysis. Finally, I confined my remarks to the mathematical and physical issues. In contrast, the comment from R. Waibel invokes several authors but gives only one explicit citation; ignores virtually all of the specific issues I raised and instead raises issues which are, at best, peripheral to the content of my paper; makes dogmatic, but unsubstantiated, assertions; and resorts to "it can be proved," "it is evident," and name-dropping to make its case. Finally, Waibel attempts to build his case by a personal attack on me, my competence and my reputation, as much by innuendo as by directly pejorative statements and intentional misrepresentations.

I requested a copy from the Lewis Research Center's library of the one explicit literature citation given by Waibel and described by him as "easily available." The library staff is adept in tracking down obscure publications. Yet more than 9 wk later I still have not received a copy even though many sources were explored. So much for being easily available.

Waibel makes much ado about two meetings held at the NASA-Marshall Space Flight Center in February 1985 and November 1991, and about some unpublished calculations by Lockheed in 1969 and subsequent "verification" calculations by Continuum Inc. in 1981, also unpublished. He uses these to justify the development of the Munich Method and to question my objectivity in evaluating the Munich Method. I made no mention of these things because they are unpublished and they are irrelevant except, perhaps, to supply an historical context for the Munich Method. My concern was with the correctness of the Munich Method and not its origins. However, since Waibel raises these issues I must point out that Waibel's discussion of these matters is misleading and incomplete. Furthermore, at the 1991 meeting I informed the author of the Munich Method of the essential content of my paper prior to its submission for publication; a courtesy not extended to us prior to the publication of the Munich Method. We became aware of it only long after its publication.

Waibel's discussion of the Lockheed-Continuum work is misleading because he neglects to mention that the Lockheed calculations and subsequent "verification" calculations were both the work of the same individual. This hardly qualifies as independent verification. This fact was certainly known to everyone who attended the 1985 meeting and so must have been known to Waibel. He also displays a curious ambivalence vis-a-vis Lockheed-Continuum. In one paragraph he raves over the 700 K lower temperature obtained in the Lockheed-Continuum calculations. But in a subsequent paragraph he says that the "Lockheed expertise is wrong to postulate an additional feedback of the choking condition in the nozzle throat on the combustion chamber flow." Yet, it is precisely

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this "feedback" which is responsible for the 700 K lower temperature. The finite combustion area produces only a small temperature difference, on the order of 10 K for the hydrogen-oxygen system.

He misrepresented the conclusions from the 1985 meeting. His statement that "the 'Constraint Entropy Maximization Concept' was rejected as unfounded" would more accurately be phrased as "The Lockheed-Continuum calculations were rejected as unfounded." He also incorrectly implied that the 1985 meeting endorsed the development of the Munich Method and disapproved of the Lewis method of Gibbs energy minimization. An accurate quotation of the pertinent conclusion from the 1985 meeting is ". . . that the Gibbs' free energy minimization is not theoretically correct in a flow system was stated, but not proved in this workshop. This should be formalized." Furthermore "Gibbs-Falk thermodynamics" was never mentioned in the conclusions. Finally, the only mention of the Lewis code in the conclusions was in the final item. It read "For the analysis of existing real nozzle flow and for the design of future systems, an extended code, based on rigorous theory, should be developed as a supplement to the NASA-Lewis code." My paper has adequately demonstrated that the Munich Method does not qualify for the job.

Waibel displays a predilection for trotting out "experts" (in alphabetical order Bray, Callen, Falk, Gibbs, Noether) to buttress his arguments, but assiduously avoids specific literature citations. Typical of this writing style is his invocation of "the Noether-Callen symmetry principles." Now I am aware of Amalie Emmy Noether (1882–1935) and Noether's theorem on the connection between symmetry and conservation laws. I am also aware of the physicist-thermodynamicist Herbert Bernard Callen (1919–?) but I am unaware of any collaboration between the two. I am also unaware of any other reference to the "the Noether-Callen symmetry principles." If such a collaboration existed, it would certainly demonstrate that Callen was a precocious youth since he was only 16 at the time of Noether's death.

In the course of his comment Waibel disparages my axiomatic treatment of the mathematical structure of thermodynamics and its relationship to continuum mechanics. Yet he fails to include a reference to the two papers which so inspired his indignation. He invokes one of his "experts" (G. Falk) at least six times, without a citation, to justify his position. Even the venerable Gibbs is pressed into service. I chose not to refer to my papers[1,2] because they were irrelevant to my discussion of the Munich Method. Had they been relevant I would have cited them. I cite them now only because Waibel chose to make them an issue and because I want to give every reader the opportunity to examine them and make an informed judgement on the validity of Waibel's claims. The reader should note that in my introductory remarks in the first of these papers, I cited the work of Falk and compared it to other treatments of the structure of thermodynamics. Waibel attributes a quotation to me which he describes as "one of the theory's general axioms." I could not find that quotation associated with any of my many axioms and theorems. Instead, I was able to locate the quote (p. 175, following Eq. II.17), in the penultimate section of the second paper, a section devoted to three simple examples illustrating the application of my formalism. The quote came from an example which deals with the simultaneous effect of electromotive force, gravitational potential and centrifugation on thermodynamic properties. Waibel intentionally misrepresented the quote. Also the context of the quote shows that it has no bearing on my discussion of the Munich Method.

Waibel does acknowledge that there were some errors in the Munich Method. But only in "a wrongly computed balance of the total pressure" and "a simplified reaction scheme." Please do ignore the isentropic combustion, discarded equation, flawed numerics, mathematical errors, and a limitation to the hydrogen-oxygen system. He has the effrontery to insist that "By use of traditional thermodynamics the problem cannot be solved." This in spite of my derivation of both a corrected Munich Method and the Lewis code equations, from the same starting point, using only "traditional thermodynamics." That starting point was entropy maximization and not Gibbs energy minimization as Waibel states. He uses the specious argument that my derivation "does not work even with multidimensional isentropic flows." Naturally it doesn't since it uses the one-dimensional forms for conservation of energy and momentum. But then I was only trying to reproduce the Munich Method, which is one-dimensional, and not the general case.

Waibel attempts to palliate the strident comments made by the author of the Munich Method with the statement that "the book contains passages the sponsor insisted be included." But the author always has the ultimate responsibility for the content and the tenor of a publication bearing his name. In fact, a disclaimer in the book says "The author is alone responsible for the study's contents."

There is one amusing aspect to Waibel's comment. That is his contention that my derivation of the Lewis code's equations from the Munich Method's starting point happened "only by pure chance" and "pure coincidence." I guess that just proves that it's better to be "pure" and lucky than talented.

I do not think it profitable to compare Lewis code values with calculations from the Munich Method as is done in the Comment by S. Gordon. The Munich Method has serious afflictions featuring both "isentropic combustion" and non-isentropic expansions which ought to be isentropic. Consequently, the Munich Method calculations are incorrect in principle. Furthermore, the original Munich Method calculations do not even accurately reflect the "real" Munich Method since they assign an entropy value to the combustion state which differs drastically from the value proposed as correct by the method's author. The Munich Method gives a remote approximation to the correct results only because the author of the method chose, as a substitute value, the entropy from an adiabatic combustion calculation done with the Lewis code. Hence, any comparison of values seems pointless.

I cannot accept at face value the numbers Gordon represents as being from a "corrected" Munich Method. He well knows that 1) we only saw a listing of values which purportedly came from a "corrected" Munich Method; 2) we were never told just what corrections were made; 3) we were never told that the author of the Munich Method had retracted his concept of "isentropic combustion" or replaced his inferior method for calculating nozzle expansions; 4) we did not see the computer in the process of doing the calculations; 5) we did not have the opportunity to use the program ourselves to verify that it could do similar calculations for cases of our choosing; and 6) we did not have access to the source code to verify that, in fact, any corrections had been made. Since the Munich Method has been so thoroughly discredited, only independent verification of its calculations is acceptable.

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