Table 1 Comparison of NASA Lewis and Munich Method theoretical rocket performance parameters

Rocket engine				Theoretical performance parameters					
	Operating conditions			I_{vac}, m/s		$\dot{m}$, kg/s		Momentum, bar	
Type	Chamber pressure, bar	Oxidant-to-fuel weight ratio	Area ratio	NASA	Munich	NASA	Munich	NASA	Munich
J-2	53.1	5.552	27.5	445.3	451.5	232.6	303.0	53.1	60.5
J-2S	85.9	5.85	39.8	452.7	458.4	266.9	288.3	85.9	89.4
SSME	207.9	6.00	77.5	466.2	469.9	468.8	497.0	207.9	213.2
ASE	140.0	6.00	400.0	485.9	489.1	19.0	19.4	140.0	142.2
HM7-B	35.9	5.30	82.9	467.2	470.2	13.2	13.8	35.9	36.8
HM60/1	103.6	5.70	45.0	457.1	461.0	234.1	253.1	103.6	107.5

Table 2 NASA Lewis and Munich I and II performance parameters

	NASA	Munich I	Munich II
I_{vac}, m/s	466.1	469.9	466.2
$\dot{m}$, kg/s	468.8	497.0	470.0

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[1]Zeleznik, F. J., "Evaluation of the Munich Method for Modeling Rocket Engine Performance," *Journal of Propulsion and Power*, Vol. 9, No. 2, 1993, pp. 191–196.

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Comment on "Evaluation of the Munich Method for Modeling Rocket Engine Performance"

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THE review of a book[1] addressed mainly to experts, which covers six pages and appears 4 yr after the book's publication is a spectacular event and demands comment. This comment consists of six narrow parts and intends to reduce the article's polemics to a fair, rational level.

1) In order to make space vehicles reusable, some extreme requirements must be met for the propulsion system. For the design of the Space Shuttle Main Engine (SSME) in particular, the LH-LOX combustion with gas temperatures above 3000 K is the sore spot.

The risks involved have already been recognized in working with SATURN V-engines. Therefore, some members of the NASA-Marshall Space Flight Center (MSFC), Huntsville, Alabama, recommended a fundamental reconsideration of the "Rocket Performance Theory" whose computation code in the version NASA SP-273 (March 1976) is in worldwide use today.

In an in-house study Lockheed (1969) tried to secure a realistic assessment of the gas temperatures which are experimentally not determinable in the nozzle throat. The results, documented in a technical brief not yet officially available, were unanticipated. In the representative test case of the J-2 engine, the agreement between theory and experiment allowed experts to conclude that the real effects were only insignificantly different from Lockheed's modification of the Lewis Code. Furthermore, the new theory explicitly includes the finite area combustion processes along the combustion chamber, thus predicting considerably lower and more manageable gas temperatures in the nozzle throat cross section. For the J-2 engine, Lockheed's "Constraint Entropy Maximization Concept" forecasts a combustion exit temperature which ranged about 700 K (!) lower than that indicated by the Lewis Code. This sensational result was—surprisingly enough—confirmed 12 yr later by Continuum Inc., Huntsville, Alabama, a contract with NASA.

In order to find a conclusive answer, NASA organized a first workshop at the end of February 1985, attended by delegates of the MSFC and Continuum Inc. as well as by U.S. experts from various universities, and S. Gordon as one of the two authors of the NASA report SP-273. I then accompanied Prof. Straub, the only European expert who was invited by the MSFC.

In a final communiqué unanimously passed by all the experts, the "Constraint Entropy Maximization Concept" was rejected as unfounded. It was recommended that the Gibbs-Falk thermodynamics should be investigated and, if found suitable, be used as the theoretical basis for an "Extended Lewis Code." The participants trusted, of course, that intellectual integrity and scientific honesty require proven theorems referring to the theory as a whole, or to concrete application to be deduced from a relevant hierarchy of general axioms. S. Gordon himself, who shared the committee's resolution, mentioned no results at all of any updated Lewis Code version with rocket performance calculations for finite area combustion chambers. Nor did he even try to impose ex cathedra any method of his own for the calculation of complex chemical equilibria in one-dimensional gas dynamic modeling processes. Dr. Zeleznik was invited, but he could not attend due to unapproved travel at Lewis Research Center.

2) Prof. Straub's "rocket book"[2] is a response to the experts' resolution of February 1985. It was not financed by NASA but by the German Federal Ministry for Research and Technology; for that reason, the book contains passages concerning fundamentals of thermofluiddynamics that the sponsor insisted be included.

The essence of the book concerns the following problem: a gaseous combustion mixture flows under high pressure and high temperature first through a finite area combustion chamber and then through a Laval nozzle where it is accelerated up to supersonic velocity. The NASA report SP-273[3] gives

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instructions how to determine the resulting thrust of a rocket engine. The respective numerical codes presuppose as a rule chemical equilibrium of the mixture flow along the configuration and, furthermore, the combuster diameter is assumed to be of infinite extension. Clearly, it has always stated that throat stagnation conditions should be used. But people did not do this, since they did not know how to get the stagnation values. They used chamber conditions instead, i.e., at the inlet of the nozzle with flow speed being neglected. Consequently, static temperature and pressure were in fact the canonical variables of the free enthalpy to be minimized for equilibrium.

In contrast, the Lockheed expertise asserts that the high gas velocity resulting from the finite combustor diameter has a significant influence on temperature, pressure and gas concentration at the inlet of the nozzle.

Corresponding calculations are based on a theoretical approach for the combustor flow assumed to be frictionless. It relates the equilibrium concentrations to the maximum of the mixture's specific entropy s under the constraint of a constant total enthalpy h_{tot} and constant total pressure p_{tot}.

For this realistic approach, it is evident that thermostatics is questionable for flow dynamics. The problem was whether at all and under what conditions the maximum entropy exists and can be determined by observing the constraints h_{tot} = const and p_{tot} = const.

3) By use of traditional thermodynamics the problem cannot be solved. Gibbs'[4] approach to thermodynamic equilibria, for example, is just a variation method for unmoved systems. Now G. Falk has extended Gibbs' idea by a new concept for nonequilibrium states as the standard case.[5] It turns out that the thermodynamical potentials—in agreement with the Noether-Callen symmetry principles, too—usually depend on the momentum P of the system as well. Therefore, every realistic matter model has to assume that the system velocity v as constituted by the conjugate linear momentum P depends on the entropy even if the value of v is very small!

By means of the Gibbs-Falkian thermodynamics[6] for isentropic one-dimensional flows, Prof. Straub's book substantiates the calculation of the local equilibrium states for s_{max} with the constraints h_{tot} = const and p_{tot} = const.

It should be emphasized, however, that the Lockheed concept is wrong to postulate an additional feedback of the choking condition in the nozzle throat on the combustion chamber flow.

This solution for isentropic one-dimensional combustor nozzle flow problems leads to equations as explained in Prof. Straub's book. It is true that they can be transformed into the form presented by Dr. Zeleznik in his review. However, such a reduction of both the original balance equations and the prescription of process realization leads to a simplified set of algebraic relations only by pure chance.

Prof. Straub established this by means of the Alternative Theory (AT) which he developed from the Gibbs-Falkian thermodynamics for the nonequilibrium continuum theory of compressible fluids.

4) Dr. Zeleznik is obviously not in a position to refute the claim made by the Lockheed expertise. As mentioned, the Lockheed theory predicts a combustor temperature of 700 K less for the J-2 engine, while maintaining an excellent comparison of the theoretical thrust values with the experimental evidence. Luckily this shortcoming is easy to explain: between 1976 and 1981 Dr. Zeleznik has published a monograph which covers up to 48 double column-pages and carries the programmatical title "Thermodynamics."[7] In this paper he endeavoured to found an algebraic core of thermodynamics in a way which G. Falk had abandoned as the wrong track twenty years before: the partition of the variable set into thermodynamic and non-thermodynamic variables is just as inadequate for the realistic description of a system as it is inadequate for the internal energy increment to reduce the interaction of the system with its surroundings to a differential form of the First Principle. This means an arbitrary and indefensible partition of the dynamics to which a system is exposed into kinematics of motion and Gibbsian thermostatics.

These old and primitive concepts and misinterpretations may only help to thwart the understanding of the urgent debate of the fundamentals (which Prof. Straub is right to invoke), or to cloud any insight into the very problems the Lockheed concept entails. The main obstacle, however, is Dr. Zeleznik's ideological position which nips all reasonable and self-critical discussion in the bud. As Dr. Zeleznik states in his apodictical way: ". . . the potential and the velocity v are nonthermodynamic quantities and imply that they are to be determined by nonthermodynamic considerations. Thus they are to be regarded as 'external' fields which can affect the thermodynamic state but which cannot be affected by the state directly."[8]

It should be emphasized that this quotation doesn't summarize a relevant part of Dr. Zeleznik's thermodynamics. Far from it: in fact it is one of the theory's general axioms! Accordingly, thermal processes would be always dominated by kinematics and reduced to pure static "real effects." Therefore, Dr. Zeleznik's "thermostatics" is completely in contrast with the Gibbs-Falkian thermodynamics.

5) Admittedly, there are errors in Prof. Straub's book. The most regrettable of them occurs in the basic computer program: the wrongly computed balance of the total pressure for the calculation of the finite area combustion along the combustor.

A second, more principal computational error affects the derivatives of the reaction coordinates for pressure and temperature. It is related to a simplified reaction scheme (Eq. 3.34/3.35, p. 146).

Nevertheless all relevant statements of the book continue to hold true; especially, the following items should be noted:

a) It can be proven that a complete chemical equilibrium exists only in the hypothetic limiting case of one-dimensional isentropic flow with finite area combustion—the ideal comparison process.

b) It can be proven exclusively for this stream tube flow that the determination of the mole fractions χ can be performed by means of the entropy maximization algorithm with the conjugate constraints of constant total enthalpy h_{tot} and constant total pressure p_{tot}.

c) It can be proven that the mass stream $\dot{m}$ results as an eigenvalue under prescription of the area section of the combustor as well as of the throat and the exit of the Laval nozzle.

In consequence of a, the comparison process provides, strictly speaking, not just an absolute standard for the quality evaluation of a rocket engine performance, but also the interdependencies of all important variables under optimal conditions. However, if one uses the algorithm for the simulation of real processes, one is committing not only a rather unavoidable practical mistake, but an irrefutable conceptional error, too. Apparently, Dr. Zeleznik does not address that difference.

As mentioned above, it is by pure coincidence that the procedure described under item 2 can be derived from the well-known algorithm of free enthalpy minimization. Such a formal transformation does not work even with *multidimensional* isentropic flows: For this case an equilibrium condition of the kind $(\partial s/\partial \chi) = 0$ does not exist with reference to the constraints $h(\text{tot})$ = const and $p(\text{tot}) = p + \rho v^2$ = const, and related to the mole fractions χ! But Dr. Zeleznik does not take any notice of this fact.

It should, however, concern him, though, that $\dot{m}$ is a parameter which cannot be chosen unrestrainedly, if item c is observed, as the famous K. R. C. Bray (who introduced the term "eigenvalue")[9] has already pointed out.

The errors mentioned have meanwhile been corrected. In an easily available research report[10]my former scientific assistant 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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[1]Zeleznik, F. J., "Evaluation of the Munich Method for Modeling Rocket Engine Performance," *Journal of Propulsion and Power*, Vol. 9, No. 2, 1993, pp. 191–196.

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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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