

provided for as possible species, the generalized computer program will *insist* that the chemical equilibrium between them be satisfied unless *all* compositions are frozen. One can, usually, leave out nitric oxide (for example) entirely, but this is no answer to the problem of freezing it alone.

A means of performing thermodynamic calculations for partially frozen flows has been devised which is more or less exact, depending upon the particular circumstances. This involves creating, algebraically, a whole new periodic table of the elements and resultant chemical system. This chemical system "beyond the looking glass" is uniquely designated as the Q system, consisting of QH , QHe , QLi , etc. All members of the Q chemical system, of course, have identically the same properties as their normal counterparts (molecular weight, enthalpy, entropy, heats of formation), but only physically, not chemically, can the two systems coexist. The generalized computer program can react both $2 QNQO = QN_2 + QO_2$ and $2 NO = N_2 + O_2$, but neither $2 QNO = QN_2 + O_2$ nor $2 NQO = N_2 + QO_2$.

Now it is obvious that, if the existence of QN_2 and QO_2 is forbidden to the program by leaving out these species, none of the $QNQO$ nitric oxide can participate in the composition shift to these molecules called for by equilibrium. If the existence of NO nitric oxide is forbidden on the "proper side of the looking glass," no N_2 and O_2 can react to fill an algebraic void. In certain generalized programs, the equilibrium existence of the atomic species *must* be allowed for, since the gaseous atoms are the basic building blocks of the entire chemical system. Thus, the equilibrium adjustment of the reaction $QNQO = QN + QO$ still must be permitted. For this particular reaction, this involves the existence of a trivial amount of atomic nitrogen and atomic oxygen which thermodynamically ought not to be distinguished from their non- Q counterparts. Thus, it must be stated that in such programs the approach is only more or less exact; other programs avoiding the inherent necessity of the presence of the atoms as building blocks can apply the approach exactly. By definition, if no more of the frozen species is to be created or consumed, it need appear only on the Q side of the looking glass. Thus, one can avoid the anomaly of computing an entropy level for a mixture where both the Q species and its normal counterpart contribute separately to the entropy of mixing.

In application of this approach to the dissociated air example, a normal equilibrium expansion is carried out until the freezing of nitric oxide is indicated. At this point, equilibrium calls for the reaction $QN + QO = QNQO$ to lie very preponderantly to the right. Thus, virtually (but not *exactly*) no error is introduced by assigning all of the NO , and nothing else out of the present equilibrium mixture, as input $QNQO$ on a mass balance basis. From the same pressure, temperature, and (necessarily) enthalpy level, the expansion now is continued. All of the original species except NO are permitted in the non- Q system; only $QNQO$, QN , and QO are permitted in the Q system. Thus, atomic oxygen and atomic nitrogen may continue to recombine to their respective molecular states as the individual equilibrium relationships dictate. When these recombination reactions are indicated to freeze, the normal program option of total freezing can be invoked.

The Q approach has been used to attain thermal equilibrium, without chemical reaction, of hydrogen fuel in hot air. The QH_2 is, of course, allowed to undergo its own equilibrium dissociation: $QH_2 = 2 QH$. The same sort of treatment could, of course, be applied to equilibrium pyrolysis of hydrocarbon fuel in a heated air stream, even one already containing some equilibrium combustion products. By neglecting the entropy-of-mixing problem alluded to earlier, an application of the Q approach to the mixing of two reactive streams of fuel and air of different stoichiometries is envisioned, in order to separate thermal and molecular weight effects from chemical effects.

Comments

Further Comment on "Fundamentals of Boundary-Layer Heat Transfer with Streamwise Temperature Variation"

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COMMENTS by Tribus⁵ on a paper of Biot¹ were critical of Mr. Biot for having omitted reference to a body of articles previously written in the field. In defense of his position, Biot indicated that these references were permeated with errors and chose to support this point by quoting an alleged conclusion from one of my reports.² This requires two comments. First, the quotation cited by Biot does not appear anywhere in the conclusions of that report. Second, the alleged conclusion is not very damaging, in that it merely states that a plate having a variable surface temperature experiences local convective heating, which even Ref. 1 shows. I can only surmise, from these two bits of evidence, that Biot was careful neither in reading my report nor in writing his reply. Certainly, he did not misquote me intentionally, but perhaps he meant only to paraphrase my conclusions and had forgotten to insert the term "coefficient" after "heat transfer." But, this isn't a small point. It reveals that Biot has fallen into the same trap as I did in 1947,^{3,†} when I also chose to abuse the concept of the heat transfer coefficient under conditions of variable surface temperature. After reconsidering the problem, I intended to show in Ref. 2 that it is not really the heat transfer coefficient that is at fault in the Newton Law of Cooling expression, but that the problems arise from the use of a local temperature potential made up of the difference of the local wall and freestream temperatures. A proper temperature potential replaces the local surface temperature with a term that reflects the temperature variations along the plate from the start of the boundary layer in the form of a Duhamel integral. When this is done, the out-of-phase anomalies rediscovered in Ref. 1 no longer occur. The heat transfer coefficient becomes merely a measure of the flow conditions and the state and transport properties of the medium. The basis of this same conclusion emerges so vividly from Biot's own variational approaches to the problem⁴ that one wonders who really is replacing understanding with elaborate analytical procedures.

References

- 1 Biot, M. A., "Fundamentals of boundary-layer heat transfer with streamwise temperature variations," *J. Aerospace Sci.* 29, 558-567, 582 (1962).
- 2 Rubesin, M. W., "The effect of an arbitrary surface temperature variation along a flat plate on the convective heat transfer in an incompressible turbulent boundary layer," NACA TN 2345 (1951).
- 3 Rubesin, M. W., "An analytic investigation of the heat transfer between a fluid and a flat plate parallel to the direction of flow and having a stepwise discontinuous surface temperature," M.S. Thesis, Univ. Calif., Berkeley (1947).
- 4 Biot, M. A., "Lagrangian thermodynamics of heat transfer in systems including fluid motion," *J. Aerospace Sci.* 29, 568-577 (1962).

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† Unfortunately without being aware of Leveque's pioneering efforts in this field.

⁵ Tribus, M., "Comment on 'Fundamentals of boundary-layer heat transfer with streamwise temperature variations,'" *J. Aerospace Sci.* 29, 1482-1483 (1962).

Reply by Author to M. W. Rubesin

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THE full text of assumption 5 in the conclusions of Rubesin's paper (Ref. 2 of the preceding comment) reads as follows: "The local heat-transfer coefficient determined on a plate having a constant surface temperature applies to a plate having a variable surface temperature when it is expressed by an equation based on the local flow and thermal boundary-layer thicknesses instead of the distance along the plate."

In the foregoing context, the concept of local heat-transfer coefficient embodies a physical model, which is fundamentally wrong. In addition to being inadequate to cope with the anomalies, it also misrepresents the real physical nature of the phenomenon. This is shown clearly and explained in my analysis of what happens inside the fluid by introducing a two-dimensional model.

In two recent papers (Refs. 1 and 4 of the preceding comment), I have developed a new approach to transient temperature analysis in structures in contact with a moving fluid in laminar or turbulent flow. The significance of this work is being obscured by comments that are focused on a side issue.

In order to restore true perspective and dispel some of the misconceptions evidenced by the comments, I should like to state more precisely what has been accomplished.

1) A correct formulation of transient temperature problems in a system that includes coupling between conduction in a solid and convection in an adjacent moving fluid leads to partial differential equations with integro-differential boundary conditions. They are impractical for use in structural analysis. However, the new Lagrangian variational procedure (Ref. 4 of the preceding comment) avoids this difficulty while retaining a correct representation of the physics. The problem is reduced to the solution of a system of ordinary differential equations with a few generalized coordinates. The treatment also brings to light some new and fundamental aspects of nonequilibrium thermodynamics for which Onsager's relations do not apply.

2) Additional simplifications of practical importance are provided by extending to convective phenomena the concept of "associated field." It is shown to be applicable in spite of the fact that the problem is not self-adjoint.

3) As a corollary, I have developed in Ref. 1 of the preceding comment a new approach, also based on variational procedures, for the conventional more restricted problem of boundary-layer heat transfer, including turbulence and non-parallel streamlines in two and three dimensions. The method combines simplicity with high accuracy and brings to light the significant parameters.

4) Without the use of new methods, a correct formulation of transient temperature problems with surface convection is so involved that it has been customary to represent the boundary condition by a local heat-transfer coefficient. The first portion of the first paper (Ref. 1 of the preceding comment) is devoted to an examination of the misconceptions involved in this procedure by analyzing very simple mathematical models that highlight the essential features and provide a deeper insight into the physics. It points to the key role played by the Peclet number as a measure of the distortion of the temperature field and its relation to a thermal flow reversal inside the

fluid. This is in contrast with the customary use of the Reynolds and Prandtl numbers, which are not representative of the physics involved.

5) This physical analysis of convective phenomena is based on the classical procedure used by Leveque, which amounts to introducing a "conduction analogy." Since this in itself is an approximation, its validity and limitations were examined in the first paper (Ref. 1 of the preceding comment), with particular reference to transient phenomena.

6) It is shown that the unorthodox behavior of the convective heat transfer is inherent in the physics and does not result from spurious mathematical properties. Any attempt to camouflage it by mathematical juggling misrepresents its real nature.

Comment on "Fundamentals of Boundary-Layer Heat Transfer with Streamwise Temperature Variations"

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THE author of Ref. 1 replies to a comment by Tribus² and maintains that the latter's statements "are in gross contradiction with the facts." I wish to confirm that these statements are not in contradiction with the facts.

Negative and infinite "traditional" heat transfer coefficient mentioned in Ref. 1 already has been predicted in the last reference cited in Ref. 2. The writer has treated^{3,4} the problem of sinusoidal heat flux and obtained another extension of Leveque's solution. The "traditional" coefficient is defined with the remark that "it is of little use since there is no connection with other relevant quantities of heat transfer."

References

¹ Biot, M. A., "Fundamentals of boundary-layer heat transfer with streamwise temperature variations," *J. Aerospace Sci.* 29, 558-567, 582 (1962).

² Tribus, M., "Comment on 'Fundamentals of boundary-layer heat transfer with streamwise temperature variations,'" *J. Aerospace Sci.* 29, 1482-1483 (1962).

³ Dzung, L. S., "Heat transfer in a round duct with sinusoidal heat flux distribution," *Proceedings, Second U.N. Conference on Atomic Energy* (United Nations, Geneva, 1958), Vol. 7, p. 657.

⁴ Dzung, L. S., "Heat transfer in a flat duct with sinusoidal heat flux distribution," Ref. 3, p. 671.

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IT should be obvious, after reading my foregoing reply to Rubesin's comments, that much more is involved here than a passing reference to unorthodox and often misinterpreted analytical properties. The real question is why does this happen? Is it more than a spurious mathematical property, and what is its more intimate physical nature? This I have

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