Journal of Thermal Analysis and Calorimetry, Vol. 62 (2000) 873–881

Book review

Thermodynamic Optimization of Finite-Time Processes
by R. S. Berry, V. Kazakov, S. Sieniutycz, Z. Szwast and A. M. Tsirlin
Publisher J. Wiley & Sons, Ltd. Chichester–New York–Weinheim–Brisbane–Singapore–Toronto 2000, (ISBN 0-471-96752-1). Price: 205 US$

The promising title, the nice appearance and the high-quality printing arouse the curiosity of the reader. If you have heard of finite-time thermodynamics, but do not understand its point exactly, you may well be driven crazy.

Chapter I, the introduction, presents the scope and implicitly forecasts that the reader will learn the results of finite-time thermodynamics; he or she will be taught 'the necessary and sufficient conditions for the existence of extended thermodynamic potentials, appropriate to time- or rate-constrained processes; procedures for constructing such generalized potentials'. Two short introductory chapters are devoted to the basics of thermodynamics and optimization methods.

In Chapter II, some elements of classical thermodynamics are collected. The third subsection relates to balances and efficiency estimates. It was on reading this that I felt the first suspicion. Part 2.3.2, for instance, discusses 'Stationary Heat Exchange'. The usual treatment in undergraduate courses is broken when the logarithmic dependence of the entropy on temperature is introduced, referring to ideal gases but to constant specific heat. After a few lines, the logarithmic mean is explained dully.

The first sentence of Part 2.3.4 reads: 'Mechanical work can be generated not only from the cyclic operation of a heat engine, by transformation of heat from a hot body to a cool one, but also by transformation of mass from a body with higher chemical potential to one with lower chemical potential (sic)'; a reference to a Russian paper in 1984 closes the sentence. I somehow became reluctant to continue; I remembered that long before 1984 cars were coming and going on the streets and my father explained to me (then a very small child) the role of petrol in the tank of a car. In this part, an unnumbered formula relates an output power to something which is interpreted in the text as some total flux of energy, though one glance at the denominator demonstrates that it can be anything but an energy flux.

Chapter III, 'Optimization Methods', furnishes a brief introduction, which does not bore the reader with wording of high accuracy. It starts with the definition of an external problem; and it deals with very classical minimum problems, as well as with nonlinear programming.

1418–2874/2000/ $ 5.00

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Chapter IV, ‘Optimal Control Methods’, is really nice to read. The introduction to the theory and methods of optimal control is followed by some really complicated problems. This chapter is not easy, but it is well worth time if you have plenty. Section 4.13 presents a review on the variational principles of Onsager’s irreversible thermodynamics with a formalism as if they were control problems. This kind of presentation is really interesting; nevertheless, Onsager’s thermodynamics is somewhat truncated and forced into a Procrustean bed.

The last section (4.13) of this chapter is entitled ‘Towards Finite-Time Availability of Thermal Processes’. Why towards? The extensive list of references at the end of the book and the text itself suggest that finite-time thermodynamics has several decades of history. Are there no results to date? I am eager to know how much work can be obtained from a gallon of petrol within ten minutes; what percentage of the availability can be utilized? Can it be determined, or not? This question is not answered in the book, nor is any attempt made.

The further chapters (V–XI) comprise collections of several problems; they rather resemble the ‘solutions’ in a problem book. Some of the solved problems are interesting, and most of them are sophisticated, as in any average problem book. The mistakes in the notations and the diversity make the life of the reader more difficult. Moreover, there are erroneous cross-references, inappropriate and undefined notations, etc. Not only is the same quantity sometimes denoted by several symbols, but the same symbol stands for different quantities.

This holds not only for the different parts of the book, but even within some single formulae. For example, in equation (2.64), one can find two temperatures denoted by T^+ and T^-; the equation relates to a system with four reservoirs, two heat reservoirs and two for the moving chemical component (Figure 2.7; referred to in the text). The symbols T^+ and T^- refer to the heat reservoirs when in the numerator and to the component reservoirs when in the denominator. It is the reader’s task to select. The inconsequent usage of abbreviations is a minor problem; thus, the abbreviation NCA (Novikov–Curzon–Ahlborn) is defined on page 177, but later CAN is used, even in titles. The grammatical errors also cause difficulty; a singular noun with a plural verb is not rare.

The examples are not established clearly and many of them are awkward. The heads and subheads are much more general than the content they ought to reflect. As an example, subhead 5.1.3 is ‘The General Law of Heat Transfer’. The equation for the heat flow it refers to is $q=\lambda\ T^0 n - T^n$) which really covers Newtonian heat transfer with n=1 and the radiating heat exchange of grey bodies with n=4, but is far from general; it does not work for the radiation of non-grey bodies, for pool boiling, in fluids with natural convection, etc.

The text and the formulae are difficult to compare. The numerical examples help in the deciphering. Example 6.1 on page 230, for instance, gives the numerical details of a counter flow heat exchanger; hot petrol is cooled with water. The result of a not clearly defined ‘optimization’ of the entropy generation is more and cooler waste water. The example ends and the reader’s calculations reveal that the temperature of the cool petrol is higher and even the length of the pipes is changed. The reader is stymied. May the temperature of the leaving petrol higher? How is the length of the pipes changed? Are the changed quantities control variables? And finally, why is the colder water better? The higher amount surely increases the cost. I became curious about the subsection climaxing in this example, and I found some really interesting things; formula (6.11) states that the temperature of the petrol can be decreased to arbitrarily close to the absolute zero temperature (well under the input temperature of the cooling water) if the pipes are long enough. How could this result be obtained? I started to trace through the text with the same methods as used in the case of a very bad test of a student. As usual in teaching practice, the points of the calculations were hidden behind misprints, notations out of the blue, and worthless references and explanations. And finally I found the root; the subsection deals with a counter-flow heat exchanger (Figure 6.1 and some of the formulae), but the temperature distribution inside is calculated with a formula taken from a book and concerning direct flow. Combining the formulae without care leads to an 'optimum', but to one that can not be explained in any way. The initiating question of the subsection can be answered without any knowledge of control theory; the minimal loss of availability relates to infinitely long pipes and the output temperature of the cooling water equals the input temperature of the petrol. This means that no optimum exists at all. The 'computational programs' included are only algorithms (not even the best for the purpose) given in something like BASIC jargon; control of the signs and indices (the braces without the matching pair warn that a control is necessary) is more difficult than making a new one to your own taste.

Throughout the book, finite-time thermodynamics is mentioned as if it were a new branch of science. If it really had been, then some finite-time potentials would have been given. All of the considerations of the book are based on some source of work together with a method (a machine). The method with which work is obtained from a fuel is not restricted in any way in classical thermodynamics. In any real finite-time thermodynamics, the restriction would concern the duration only; the 'finite-time availability' would be a real fraction of the availability and would depend on the admissible duration, but on the particular machine. 'Finite-time availability' in reality is equal to the classical one: the entropy generation can be decreased below any positive limit without decreasing the speed of the processes; the conductivities have to be increased. The above fraction is one because reversibility and slowness are two different things. The Joule–Thomson effect is irreversible and the amount of entropy generated per unit mass is the same even at a zero speed limit. On the other hand, the equipment of pneumatic driving (which is so annoyingly noisy at road- works) is the closer to reversibility, the higher the intensity is. Other examples (direct current driving, galvanic batteries, foodstuffs, fuels that decay spontaneously, corroding materials, etc.) also prove that mixing up infinitely slow processes with reversible ones reflects a misunderstanding of thermodynamics. An amount of work arbitrarily close to the reversible limit can be obtained from the given thing even within an arbitrary duration if the method is not restricted. If it had not been so, some finite-time potential would have been established not merely in the last two decades, but even by the classics of thermodynamics in the nineteenth century. It is a pity that the promising title did not fulfill my expectations.

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Reply to the Verhas's review of Thermodynamic Optimization of Finite-Time Processes
by R. S. Berry, V. A. Kazakov, S. Sieniutycz, Z. Szwast and A. M. Tsirlin

The reviewer is not familiar with the history and literature of finite-time thermodynamics as it has developed in the scientific literature. Consequently he has a perspective oriented toward engineering practice and problem-solving, rather than the coherent, encompassing subject as it evolved in the scientific and mathematical community. Unfortunately some of the reviewer's statements are personal opinions which are simply wrong. Nonetheless, as the choice of a qualified reviewer is out of the authors' realm, we are left with no choice but to reply to the reviewer. Much of the picture of the field of finite-time thermodynamics (FTT) is significantly distorted in the review.

First, we would like to express our general view of what finite-time thermodynamics is and how its issues differ from the view presented in this review. In our opinion, the major objective of finite-time thermodynamics is not only to take into account the duration of thermodynamic processes, but also to obtain efficiency estimates for thermodynamic systems with given rates, bounded sizes, and given mass and heat transfer laws. These estimates could be very loose but they still are generally more realistic than the corresponding reversible estimates. Sometimes an estimate based on reversible processes tends to zero but the corresponding irreversible estimate tends to some finite limit. An example is a process of separating a binary mixture when the concentration of the key component is very low, tending toward zero. Here, the reversible estimate tends to zero, and the irreversible, to a finite limit. This is a fairly typical result from FTT. Among others are: The limiting power and the limiting efficiency, for given power, of a heat engine with given (finite) heat transfer coefficients; the corresponding results for heat pumps and refrigerating cycles; the optimal cycles (time paths of pistons) of internal combustion engines; the limiting possibilities of absorption–desorption cycles with given rate; thermodynamic processes, distributed over time or length, that minimize average entropy production; the conditions that must be met in order to bring a real process closer to the minimal dissipation process; the minimal work of separation with given average rate. Some of these results and the general methodology that is used to derive them are described in this book. The reviewer asserts that finite-time thermodynamics is not a legitimately new branch of thermodynamics because no new thermodynamic potentials have been developed here yet. This is incorrect; the first papers published in the field included an existence proof, with necessary and sufficient conditions, and algorithms for constructing thermodynamic potentials for systems, the specification of which included time or rate constraints. While some specific finite-time processes had indeed been optimized in both the engineering and scientific literature, *thermodynamic* potentials for finite-time processes were simply never discussed prior to the advent of FTT.

Now, we address the question of finite-time potentials. The knowledge of the fact that principal functions of extreme solutions to variational problems are potentials depending on process time and state is nearly as old as analytical mechanics. Functions of this sort were first obtained as extremum actions. Yet, for an arbitrary variational problem, where an analytical solution does not exist and only numerical solutions are possible, the effective method of finding the potential functions A(x, t) numerically was established only in 1957, with the advent of Bellman's method of dynamic programming. The classical thermodynamic potentials, which are time-independent quantities, are special cases of such generalized (time-dependent) potentials in the case of identically-vanishing Hamiltonians of related optimization problems. Thus, the purpose of Section 4.12 (quoted in the review as 4.13) is not a truncation of the Onsagar's theory 'forced into a Procrustean bed', but rather the exposition of how the classical thermodynamic potential, entropy S(x), emerges for an irreversibly deterministic dynamics. Furthermore, we have also demonstrated the emergence of 'non-classical thermodynamic potentials' from the same general approach. One of non-classical potentials is finite-time availability and one of the sections where such an availability is obtained is entitled 'Towards the Finite-Time Availability...'. Why towards?, asks the referee. The answer is: because finite-time availabilities obtained to date are just beginning of the story; they refer mainly to systems which produce power by applying purely thermal inputs. Systems which use coupled inputs of heat and mass and/or those with chemical reactions are still under investigation. It is true that for complex systems there are serious difficulties in deriving (one-stage) power expressions which would then serve to evaluate availability functions via numerical or analytical integration. The reviewer interprets this and the abundance of recent publications as evidence of the weakness of the field. On the contrary, doesn't this demonstrate that the field is lively and that more research will be forthcoming? Yet, despite the reviewer's claims, in the book there are analytical expressions and charts for finite time potentials (e.g. Eqs. 4.425 and 4.467 and Fig. 10.19 on p. 432), and the general numerical method of finding these potentials is described in Section 4. It is thus possible to calculate now how much work can be obtained from a gallon of petrol that burns in a fixed time interval. When the reviewer expresses his reservation about the first sentence of part 2.3.4 and writes about his childhood and the role of petrol in the tank of a car, he demonstrates that he does not understand what is considered here. That is, he does not know that a cycle of a diffusion-chemical pump can be used to produce work and that this device is no less a thermodynamically feasible work-producing tool than a heat engine. Had the reviewer read the book carefully, he would have been able to find an answer to the question of work obtained from a gallon of petrol. In order to do that he needs the heat value of the petrol, the power of the engine and the laws of heat transfer and heat transfer coefficients, for hot and cold parts. He could then obtain the estimate (upper bound) of the work done and could investigate how this estimate varies when the engine power is higher or lower or when the size of the engine is changed. In spite of the reviewer's claim that 'this question is not answered in the book, nor is any attempt made' numerous formulae in the book (including those quoted above) answer the question for various physical conditions and diverse sets of data, in particular either for prescribed or undetermined but finite time. It is all there, for one who knows how to read equations.

The reviewer seems to find it difficult to read the formal content of the book correctly making quotations out of context. In effect, he sometimes picks a single sentence or a formula which he feels he understands somehow, and then manufactures his own interpretation. All this happens within a hastily-arranged evaluation procedure from which both correct and incorrect conclusions follow somewhat accidentally. These are correct: his objection to the name ‘computational programs’ for ‘algorithms’ and the objection to too general a title for Section 5.1.3, which the reviewer immediately extends against all heads and subheads of the book. However most of the other objections out of context are incorrect and unsubstantiated: such is his objection against the unnumbered efficiency formula in Section 2.3.4 (which is correct although it does not contain the energy flux); such is his evaluation of (correct) examples of modeling and optimization of entropy generation. Of a slightly diffferent kind is his indignation about the ‘dull’ logarithmic mean. He fails to distinguish that the usual logarithmic mean (popular in the heat exchanger theory) is the logarithmic mean temperature difference, which involves the temperature drops between two streams, whereas the book occasionally uses a different mean quantity, the logarithmic temperature of a single stream, also called the stream’s effective temperature. The product of total heat load and reciprocals of these (logarithmic mean or effective) temperatures yields the total entropy production in the exchanger, an important quantity.

The final paragraph of the review is a mixture of weakly correlated or uncorrelated sentences. It seems that its task is to convince the reader that the authors identify slowness with reversibility; this is simply an unjustified supposition. Let us then repeat that the two properties cannot be identified. Regarding the alleged identification, in the book we have shown that for a vast and most popular class of irreversible processes, those without d’Alembertian forces, slower processes are closer to reversibility. Processes in which such forces predominate (as his noisy equipment of pneumatic driving, etc.) can run very intensely in the vicinity of the so-called resonance point; far from that point the intensity decreases for both low and high frequencies. Yet the calculated dissipation has a maximum just around the resonance point (see, e.g., Landau’s and Lifshitz’s volume on ‘Mechanics’, Pergamon, Oxford, 1976). Thus the referee is not right when he states that the equipment of this sort is ‘the closer to reversibility the higher the intensity is’. The statements of this part of review show that the reviewer interprets the term ‘finite-time thermodynamics’ quite literally and narrowly. Constraining the process duration is only one of many possible ways of fixing the average rate of the process. The assumption here is that machine sizes are finite and, therefore, the conductance coefficients are also finite. It is evident that processes in infinite time could also be irreversible. A case in point is a process of mixing by slow diffusion. But, if these coefficients are finite and the rates of processes are also finite, then thermodynamic processes are always irreversible. The development of finite-time thermodynamics and a large number of applied problems, which were solved using this approach, have proved its usefulness.

We will now consider in detail the part of the review, which deals with Example 6.1, because here the reviewer criticizes some concrete material. This makes it easier to explicitly demonstrate the reviewer’s limited understanding of the book and the subject. The optimization process that seems unclear to the reviewer is clearly defined and described in Section 6.1. It includes three logical steps:

1. First, the problem of the entropy production minimization in a two-flux heat exchanger is formulated (6.5)–(6.7). This minimization is achieved by controlling the temperature of the cooling flux along the heat exchange surface (that is, the control is the dependence of the temperature of the cooling flux T2(l) on the length). The solution of this problem gives an estimate (bound), because it is never worse than any real operating re-

gime of a heat exchanger with the same parameters. The following assumptions were used (and stated) in the formulation of the problem: The input and output temperatures of one of the fluxes (which is being heated/cooled) are given. The length of the heat exchanger is finite and given. The heat exchange coefficients are finite and fixed. No assumption is made about the form of the heat transfer law. No assumption is made about the type of heat exchanger.

2. During the second step, the formulated estimating problem is solved using the transformation that replaces the problem's independent variable, the distance from the beginning of the heat exchanger to its current section, with the temperature of the flux being cooled. How it is done and how the derived transformed problem is solved is described in full detail in Section 4.4. Note that this transformation does not change the length of the heat exchanger into a new control variable of the problem; the solution of the transformed problem still obeys all the constraints (a)–(c) as does the solution of the initial problem. Let us emphasize that this is essentially an elementary technique that does not require from the reader any knowledge beyond some elementary calculus and variation calculus. An engineer familiar with optimization should be able to follow this derivation. This solution is important, because any heat exchanger, which changes the temperature of the flux being cooled from the given input value to the given output value and whose size and material (heat transfer coefficient) are finite and given, can not operate without producing at least the same finite amount of entropy, that is produced by this solution. Any heat exchanger can produce this minimal possible amount of entropy only if the condition of minimal dissipation (6.8) is met. This condition is written for each moment of time, when the element of the cooled flux is inside the heat exchanger. If this element is moved in the regime of the ideal displacement then this condition becomes applicable in each section of the heat exchanger.

3. During the third step, the linear law of heat transfer is considered. It is shown that in this case the condition of minimal dissipation requires the ratio of the temperatures of the cooling and cooled fluxes to be the same in any section of a heat exchanger. Then it is shown that for a counter-flux heat exchanger with linear heat transfer (the real one, where only the mass flow rate and the inlet temperature of the cooling flux are controllable) it is possible to choose these parameters to minimize entropy production. In other words, it is possible to arrange working conditions for the counter-flux heat exchanger to produce the minimal possible amount of entropy.

Then it is suggested that the performance of an industrial heat exchanger be evaluated by comparing the actual entropy production within it with the minimal possible entropy production. This comparison is carried out in Example 6.1 for the particular industrial process. First, the actual entropy production is calculated using the real parameters of this process and then the minimal-possible entropy production is found. Since it is assumed that heat transfer is linear, for the minimal possible entropy production operating regime the cooled flux temperature was set equal to the cooling flux temperature multiplied by some constant everywhere along the heat exchange surface. This constant is calculated and finally the minimal entropy production (allowed by the given heat loads and given size and material of the heat exchanger) is calculated. The length of the heat exchanger, the input/output temperatures of the petrol flux and the heat exchange coefficient are all the same in the real industrial (initial) and in the minimal entropy production operating regimes. So when the reviewer writes ‘The example ends and if you calculate the temperature of the cool petrol you find it higher and even the length of the pipes changed. The reader is stymied; may the temperature of the leaving petrol be higher? How to change the length of the pipes?’ he is simply wrong. We are still puzzled how he managed to conclude all that. Perhaps he was confused because all the calculations in Example 6.1 (e.g. the expression for the entropy production) are carried out for the transformed problem, where the temperature of the petrol flux is the new independent variable. This misunderstanding could only happen if the reviewer completely skipped all the derivations in Section 4.4. Again let us emphasize that the calculated minimal-possible entropy production is equally valid (cannot be lower) for any type of heat exchanger.

Next, it is shown that – if the heat exchanger in Example 6.1 is the counter-current one – then it is possible to choose an operational regime to minimize its entropy production. In order to do this, it is indeed necessary to reduce (to some particular value) the temperature of the cooling flux and to increase its flow rate (again, to some particular value, completely determined by material, size and heat load of the heat exchanger). Therefore, the answer to the reviewer’s question ‘..why is the colder water better? The higher amount, surely, increases the cost’ is that the use of this larger amount of colder water makes the entropy production here approximately one half of the entropy production in the real industrial regime. The optimum not only exists but it is actually found and explained here. The problem of the minimizing the exchanger’s running cost is neither solved nor considered in this discussion. Thus, this rhetorical question itself is irrelevant in the context of the problem considered and it simple shows that the reviewer does not understand what is being done.

Let us consider formula (6.11), which is used to compute the temperature of the petrol flux, since it amuses our reviewer so much. He writes ‘I, really, found interesting things; formula (6.11) says that the temperature of the petrol can be decreased arbitrarily close to the absolute zero temperature – even well under the input temperature of the cooling water – if the pipes are long enough. How could this result be obtained?’. Our answer is: This formula is the solution of equation (6.5) (which is a simple heat balance for the elementary interval dl on the heat-exchange surface) joint with the condition of minimal dissipation (6.9) (the condition that the ratio of cooling and cooled fluxes’ temperatures is the same in all sections). So yes, the reviewer is correct – if an infinitely long heat-exchanger is considered, the heat exchange law is linear, the heat exchange coefficient is finite and fixed, the inlet temperature of the cooled flux is given and such a temperature profile of the cooling flux along the heat-exchange surface is established that in each section the ratio of the petrol and water temperatures are the same, then the outlet temperature of the petrol will be arbitrarily close to absolute zero (but nowhere lower than the temperature of the coolant). In this problem it is assumed that the length of the heat exchanger is finite, and given and the value of the constant m depends on this length (see, (6.10)). Therefore, there is no contradiction and again the reviewer’s amusement can only be explained by his failure to comprehend the material.

Of course, everyone is entitled to their opinion. And one would be quite right to criticize the approach, the methods, etc., to point out the misprints or errors in formulae/derivations or even to simply say that they do not like the book. But the criticism should be fair and substantiated and the reviewer should at least read the book carefully first and try to comprehend its subject. On the contrary, instead of carefully reading the book the reviewer simply picks a few formulae and then manufactures his own ‘analysis’ of what he would like these formulae to mean. In doing so, he ignored the text written about these formulae in the book, and with it, the logic of derivations and the constraints. Finally, the reviewer engineers his own caricatured interpretation of the content of the manuscript, supported by ‘explanations’ such as the ‘root’ – the hidden use of unrelated formulae – he claims he found. This interpretation is nothing like the actual contents of the book. We would like to express our disappointment with the low standard of scientific discussion demonstrated by this review.

The Authors