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Response to "Comment on 'A fallacious argument in the finite time thermodynamics concept of endoreversibility'" [J. Appl. Phys. 90, 6557 (2001)]

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In this reply, the author reiterates that his argument that there is a logical inconsistency in introducing the concept of endoreversibility into a finite-time thermodynamics model of power plants characterized with a presence of heat exchanger thermal couplings is valid. As long as the couplings involve finite temperature differences between working fluids across the suggested endoreversible boundary, the inherent irreversibilities of thermal couplings cannot selectively be eliminated due to the entropy flow across that boundary and entropy generation within a heat exchanger. Hence the concept of endoreversibility in such a case does violate a proper definition either of the system (e.g., power plant) or the component (a heat exchanger). A heat exchanger as a system component, if included in the endoreversible compartment, would operate reversibly, which is impossible for a finite size heat exchanger; if excluded from it the endoreversible part of the system becomes open, which constitutes a violation of the initial set of assumptions; and if it is split between the endoreversible part and the surroundings, each part carries its respective irreversibility. Hence the endoreversible compartment would become irreversible. [DOI: 10.1063/1.1415753]

In response to Sekulic,[1] Andresen[2] offers an opinion about several very broad issues regarding so-called finite-time thermodynamics (FTT). The key objective of the article by Sekulic,[1] however, was to focus on a fallacious argument in a specific FTT concept, i.e., the concept of endoreversibility. Hence in this reply I will refrain from commenting on broad issues emphasized by Andresen[2] because they are outside the scope of the main argument given in Ref. 1. Although important, the question of whether finite-time thermodynamics as a whole makes sense should be left for a discussion to be published elsewhere due to limited space available for this communication.[3] The Sekulic's discussion[1] uncovered a problem with the concept of endoreversibility consistency as used in a FTT model of a power plant. As explicitly indicated,[1] the goal of the discussion was "to scrutinize the set of logical propositions incorporated into a FTT power plant model," i.e., not to argue about the overall scope of FTT as "a limited body of work in the greater field of thermodynamic optimization which continues to expand."

In essence, the purpose of the discussion[1] was to confront the model of endoreversibility with the real system that this model attempts to mimic in an idealized situation. It was demonstrated, without a shadow of a doubt (in Ref. 2 there is no attempt to question the proof provided in Ref. 1), that if applied to a system in which thermal couplings between the working compartment and the surrounding (i.e., heat "source" and heat "sink") are established using heat exchangers, the concept of endoreversibility would violate either the system's or its component's rigorous definitions. It is important to notice that the heat exchanger must be clearly identified as a system component[4] (a fact carefully emphasized in Ref. 1). Put simply, if thermal couplings are accomplished by heat interactions, splitting an inherent heat transfer irreversibility caused by finite temperature differences would violate the set of assumptions used to define an endoreversible work compartment as free of irreversibilities. That is, reducing to zero the irreversibility contribution to the endoreversible compartment across a thermal coupling link (if it is caused by finite temperature differences between two working fluids) leads inevitably to a reduction to zero of the irreversibility on the external side, thus leading to Carnot's idealization. In other words, if a two-fluid heat exchanger (with the mass flow rates $\dot{m}_i$, $i=1,2$) is used as thermal coupling, the entropy generation within it (traditional idealizations invoked[5]),

$$\Delta\dot{S}=\sum_{i=1}^{2}\dot{m}_i\left\{\int_{T_{in}}^{T_{out}}\frac{1}{T}\left(\frac{\partial h}{\partial T}\right)_p dT+\int_{p_{in}}^{p_{out}}\frac{1}{T}\left[\left(\frac{\partial h}{\partial p}\right)_T-\nu\right]dp\right\}_i=\Delta\dot{S}_1+\Delta\dot{S}_2, \tag{1}$$

possesses an inherent feature of independent nonseparability of finite temperature contributions; that is, if the contribution to entropy generation on one fluid side goes to zero [if, for the sake of obviousness, the friction contributions in Eq. (1) are neglected], the same happens with the corresponding contribution on the other side,

$$\lim_{\substack{\Delta\dot{S}_1\to 0\\ p=\mathrm{const}}}\Delta\dot{S}=\lim_{\substack{\Delta\dot{S}_2\to 0\\ p=\mathrm{const}}}\Delta\dot{S}=0. \tag{2}$$

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So, allocating the total irreversibility of a thermal coupling manifested within the heat exchanger outside of an endoreversible compartment violates the assumptions adopted to write Eq. (1).

Hence the discussion provided in Ref. 1 does identify a logical fallacy. The argument advanced in Ref. 2 that a ''flow-type'' heat exchanger should not be considered in judging the logical fallacy of the concept is irrelevant for two reasons. First, the heat exchanger is used in a power plant design for which this particular analysis is performed, as indicated above. Second, any heat transfer interaction across a finite temperature difference must be, indeed, inherently associated with entropy transfer. More importantly, an allocation of irreversibilities only on one side of the endoreversible component boundary would violate the initial assumptions of thermal interaction across that boundary conducted at finite temperature differences regardless whether one, both, or none of the fluids undergo isothermal change of state. So, the fact that one or both temperature levels involved may or may not be isothermal is irrelevant as long as the finite temperature difference between them exists. Introduction of a heat exchanger as a coupling element assumes its definition to be declared,[1] including all the accompanied idealizations. So, the argument provided in Ref. 1 is related to a system for which the model presented in Fig. 1 of that article is valid, i.e., for the model that resembles the actual design of a power plant, and not necessarily for a hypothetical system, e.g., as presented in Fig. 1 of Ref. 2. That issue is not a matter of comparing one idealized model with the other idealized model, it is about modeling a real system. Otherwise, the whole study would be only a *l' art pour l' art* exercise.

It should be noted that the conventional Carnot's reversible model of the same system does pass a test of logical scrutiny regarding thermal couplings between the system and its surroundings in a reversible limit (because zero irreversibility implies no entropy generation on both sides of the reversible system boundary). In such a case, the type of coupling interactions is irrelevant, which is not the case if a concept of endoreversibility has to be used in conjunction with a finite temperature difference heat exchanger. Furthermore, in Ref. 1, it is explicitly stated that ''a key idea in modeling an endoreversible system (i.e., defining a model in which one part is reversible and the rest is irreversible) is in principle a legitimate method of thermodynamic analysis,'' as long as the analyst does not violate the definition of the system boundary and/or the selection of included/excluded irreversibility sources taken into account (say, friction vs finite temperature difference).

The broad discussion in Ref. 2 does not disprove the main point of the argument given in Ref. 1. The argument in Ref. 1 is offered to the reader to challenge it, but to do that successfully, one has to prove the logical fallacy in it as well, following the same rigorous procedure as in Ref. 1. A personal opinion, as rightly pointed out by Andresen,[2] does not do it. So, the argument that there exists a logical fallacy in the concept of endoreversibility in modeling a power plant system with heat exchangers still waits to be challenged.

[1] D. P. Sekulic, J. Appl. Phys. **83**, 4561 (1998).
[2] B. Anderesen, preceding paper, J. Appl. Phys. **90**, 6557 (2001).
[3] This topic may raise an interesting discussion in which both parties may find lots in common regarding some issues, but also disagree about the others. It should be noted that since the work of Sekulic (Ref. 1) appeared, in addition to a presence of a continuous stream of publications regarding FTT [L. Chen, C. Wu, and F. Sun, J. Non-Equilib. Thermodyn. **24**, 327 (1999)], a renewed and very eloquent criticism of the FTT approach was published as well, see E. Gyftopoulos, Energy (Oxford) **24**, 1035 (1999), which initiated a further debate, J. Chen, Z. Yan, G. Lin, and B. Andresen, Energy Convers. Manage. **42**, 173 (2001), with more to come. That discussion was preceded by E. P. Gyftopoulos, Energy Convers. Manage. **38**, 1525 (1997).
[4] D. P. Sekulic, Int. J. Heat Mass Transf. **33**, 2748 (1990).
[5] R. K. Shah and D. P. Sekulic, in *Handbook of Heat Transfer*, edited by W. M. Rohsenow, J. P. Hartnet, and Y. I. Cho (McGraw-Hill, New York, 1998).