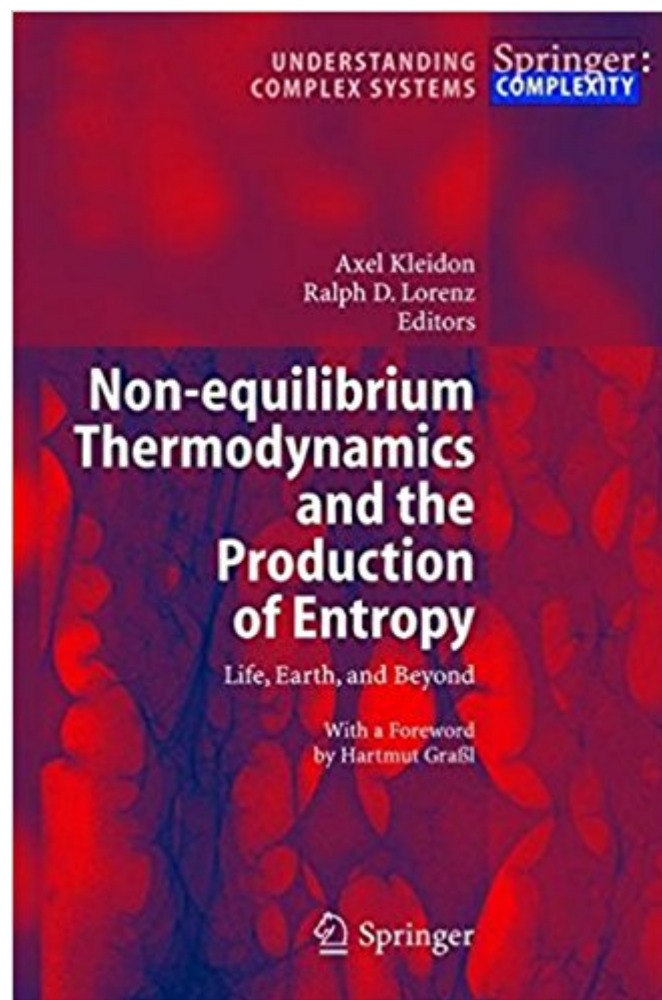




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# Non-equilibrium Thermodynamics And The Production Of Entropy: Life, Earth, And Beyond (Understanding Complex Systems)



## Synopsis

The present volume studies the application of concepts from non-equilibrium thermodynamics to a variety of research topics. Emphasis is on the Maximum Entropy Production (MEP) principle and applications to Geosphere-Biosphere couplings. Written by leading researchers from a wide range of backgrounds, the book presents a first coherent account of an emerging field at the interface of thermodynamics, geophysics and life sciences.

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## Customer Reviews

The present volume studies the application of concepts from non-equilibrium thermodynamics to a variety of research topics. Emphasis is on the Maximum Entropy Production (MEP) principle and applications to Geosphere-Biosphere couplings. Written by leading researchers from a wide range of background, the book proposed to give a first coherent account of an emerging field at the interface of thermodynamics, geophysics and life sciences.

This review will not introduce this very technical volume's content; rather it joins a side in the current lopsided argument as to its historic and scientific value. Note, the use of the English "Mr" as honourific intends to include those with PhD training, and is not in any way intended as pejorative. The Watts review, though admittedly helpful in a loose sense of the word, is biased and disingenuous on several counts. It therefore demands a partial rebuttal; not full, for the following reasons that the negative reviewer ignores. First, the scientific and mathematical concepts required

to engage this book's subject matter, and so also a review of the subject matter, are beyond almost all that will come to this site, and so – in this reviewer's professorial opinion – the merits and shortcomings of this edited volume need to be judged with reading in venues other than as basis. Even so, we can establish a better understanding of this book and its "reviews" if we are aware: (1) that this volume was published in 2004, (2) that the source of the opening dismissive critique used by reviewer Watts, that of Mssrs Grinstein and Linsker [1], first appeared in 2007, and (3) that all practising scientists understand the limitations of published book-length treatments as up-to-date sources, relative to other literature, and would not misjudge a dated argument as being pernicious, as did the Watts reviewer. Then, the Watts argument for the ingrown nature of contributors to the volume, while always having some traction – see the preface of the book for the editors' explanation of its origins [2] – are unnecessarily accusatory and judgmental. On the one hand, editors often are added to chapters other than their own, in volumes with far-ranging geographic distributions of authors because editors sometimes make original contributions, including in their ensuring, chapter-by-chapter, that the written language is proper, understandable, and consistent. On the other hand, there is no gainsaying the credentials of others of those contributing, and they are much further reaching than Mr Watts would have us believe: while several contributors do make repeat contributions, a total of 18 distinct institutions, almost all universities or federal research centers, are the sources of the chapters (same in number, see [3] for chapter authors, and [4] for more comprehensive free access to content). Hence, while the editors and authors of this volume certainly have their view of their field, we understand it to be such (i.e., their view), and are not so offended as was the negative reviewer. And while they may have been only partially "correct" in their presentation of the content as it was understood at that time, the two editors offering the volume are, notably, established faculty members at the University of Maryland and the University of Arizona, and explain their less-than-nefarious intentions in the volume's preface. Other authors for this now-dated volume included faculty from Tufts, MIT, Princeton, Tokyo, Hamburg, UWashington, and many other institutions. This said, we do not know Mr Watts basis for claiming our trust. However, those among us with history in the sciences likely have clear reason for trusting the refereeing and editorial process of Springer books (a process that our literally inestimable Mr Watts unhelpfully denigrates). Finally, it is worth noting that in a published article in a referred journal (Entropy) – a context quite distinct from reviews – Prof Dewar, one of the principal castigates of the aforementioned negative review, has very clearly reported on the limitations of his early work, and has done so in a manner more informative (and scholarly) than the

Watts review: "In two papers... three different derivations of MEP from MaxEnt were attempted. Subsequently, some technical limitations of these three derivations were noted [here Dewar cites Grinstein & Linsker, and two other published critics]. Specifically, the first derivation... introduced an ad hoc assumption... However, Bruers... presented an example in which that assumption is false... In the second derivation... a Gaussian approximation for the MaxEnt macrostate probability distribution was used... But as Bruers... and Grinstein and Linsker... correctly observed, the Gaussian approximation is only valid close to equilibrium. In a third derivation... an upper bound constraint on EP was introduced... However, the specific mathematical argument employed contains some unresolved technical issues... and is unconvincing." That is, Dewar, quite to the contrary of the tone of the Watts review, is willingly self-corrective and honest about the limitations of his earlier formulations. I call interested readers attention to this "status report", which itself is only timely through late Fall of 2009, at the Entropy article itself (see [5]). Curiously, this article has been cited by other scientists on the order of 60 times since 2009. Perhaps Prof Dewar is on to something that Mr Watts has yet to learn. As to the matter of to whom the history of this field belongs, and whether this compendium makes undue claims, I have no opinion (nor should any reader based solely on Mr Watts' attempted arguments). Finally, I would suggest, in so many words, now, that the proper place for the hashing of these issues is in formal scholarly reviews, and in the secondary, refereed (published) literature, where affiliations and credentials appear and certainly not . In this vein, the Watts review is a "trust me" trust my passion, trust my anger, trust my distrust appeal, as much as it is a meaningful review. In short, to any well-trained scientist, it is clear other information is needed to evaluate this volume. In summary, the Kleidon and Lorenz volume is, contrary to Mr Watts opinion, a contribution written by an array of recognized scholars, based on refereed, published literature, and judged sound by well-informed reviewers. That its content was and will be further superseded by new formulations and results is clear, and in pointing readers to a small part of that, Mr Watts did a service here. He simply needn't have done it in such an accusatory and seemingly disingenuous manner. (Wounds thereof, self-inflicted, though I do pour the salt.) Otherwise, readers need to do the work of evaluating this and other high level books based on sound literature, and not our opinions here. Even so, we can strive here to be even-handed fair, if you will and thorough enough in our presentations of key matters, like date of publication vs. date of critique, to avoid disservice to readers. Ranking of 3-4 is my earnest but non-specialist opinion, 5 given to offset the perturbation to the small sample size resulting from the misguided, history-insensitive low rating. References follow.

Cheers, and with regard, I am (as always) unrelated in person or finances to any party or institution named herein. References. Add the usual "http://" before web addresses that appear. [1] G Grinstein & R Linsker, 2007, "Comments on a Derivation and Application of the 'Maximum Entropy Production' Principle," J. Phys. A: Math. Theor. 40:9717 (DOI 10.1088/1751-8113/40/31/N01), accessible here as of 17 March 2015: [iopscience.iop.org/1751-8121/40/31/N01/](http://iopscience.iop.org/1751-8121/40/31/N01/). [2] The preface of this book is downloadable here: [www.springer.com/us/book/9783540224952](http://www.springer.com/us/book/9783540224952) (access date as above). [3] The chapter list of the book, and access to the first pages of all chapters (which include author affiliations) can be found here: [link.springer.com/book/10.1007%2Fb12042](http://link.springer.com/book/10.1007%2Fb12042) (access date as above). [4] For as extensive of a free perusal as is available, see: [books.google.com/books?isbn=3540224955](http://books.google.com/books?isbn=3540224955) (access date as above). [5] See Roderick C. Dewar, 2009, "Maximum Entropy Production as an Inference Algorithm that Translates Physical Assumptions into Macroscopic Predictions: Don't Shoot the Messenger," Entropy 11:931-944, DOI 10.3390/e11040931, see: [www.mdpi.com/1099-4300/11/4/931](http://www.mdpi.com/1099-4300/11/4/931) (access date as above); note this articles context, a full journal volume of articles on this subject.

The authors Kleidon and Lorenz of this book would have had to have known for some time now that the centerpiece of the volume Dewar's supposed derivation (the "MAXENT derivation of MEP) as Grinstein and Linkser have shown is invalid due to mathematical errors or worse. Worse because there is no warning or notice to the reader that this, which is touted in grandiose terms by Dewar and throughout the book (e.g., by Lineweaver, Chapter 6) as "ground-breaking work is false and misleading. Until these, and other serious errors as well as the gross historical omissions in the book's historical narrative are fixed it should be taken off the market. People are entitled to expect more from a well-know scientific publisher and usually do. As it is it serves to promote a lot of false information. The volume, to begin with, seems to be little more than a self-promoting work for its core participants in any case. Between them the Editors have their names on nearly a third of the Chapters and one wonders if there was any real peer review at all. Apparently no one of the group actually read Dewar's mathematical derivations on which the book hangs its hat with sufficient care or had the technical ability to see the errors. Same for other elementary mistakes, and the blatant historical omissions by which Dewar and the core group seem to want people to believe that the idea of the law or principle of maximum entropy production (MEP), that explains the relation between physics and biology or the emergence of biological order and evolution of the biosphere, originated with them. There is even confusion between them as to what is meant by MEP. In the first Chapter Editors Kleidon and Lorenz giving an overview of the book and theory say: "The tendency

of systems which are in a steady state, held away from equilibrium by an external input of energy to produce entropy at a maximum possible rate is what we mean by "Maximum Entropy Production" (MEP). But Lineweaver, in his chapter makes no reference to a steady state saying: "The Maximum Entropy Production (MEP) Principle suggests that structures that destroy gradients will arrange themselves such that the maximum amount of entropy is produced (under the given circumstances)". The most glaring technical error (other than Dewar's derivation) is the mistaken view presented by Kleidon and Lorenz of the supposed opposition between Prigogine's theorem of minimum entropy production or "MinEP" which applies in the linear range near equilibrium and MaxEP or MEP which applies to "steady states...far from equilibrium". This is a very serious error since it means on their view MEP is not a universal principle which completely undermines Dewar's claims in his chapter that "the MAXENT (Dewar's information theoretical) derivation of MEP explains the selection of highly ordered states from disordered ones." Of course his whole derivation is invalid but even if it were valid as a non-universal principle it can't do what he says it does. It can't explain why near equilibrium systems are selected that minimize the rate and away from equilibrium they maximize it. Of course the construal on their part is entirely wrong. The big question (above) which they cannot answer (not only because Dewar's derivation is invalid but because Kleidon, Lorenz and Dewar all are making the same mistake regarding minEP) was answered more than two decades ago by Rod Swenson who by 1989 had developed the "Principle of Maximum Entropy Production" (MEP, where the name originated) into a law, the "The Law of Maximum Entropy Production" (MEP, LMEP) which states: "a system will select the path or assemblage of paths out of available paths that minimizes the potential or maximizes the entropy at the fastest rate given the constraints" (see Swenson refs below and also Mahulikar & Harwig Physica Scripta). Unlike the failed view presented in this volume, the actual law as given by Swenson is universal, and there is no conflict between it and Prigogine's theorem; both are true. In addition unlike the invalid "derivation" of Dewar from information theory Swenson's proof is entirely physical, thermodynamical, and demonstrable (reproducible) in Popper's falsifiable sense for true theory testing. Prigogine's theorem, as Kleidon, Lorenz and the others here don't seem to understand says that as a near equilibrium system in the linear range moves towards equilibrium and the "forces" (potentials/gradients) are dissipated the entropy production goes steadily down (being linearly dependent on the forces as we'd expect) with the limiting case being zero entropy production at equilibrium or the steady state as close to it as it can get if one or more of the forces or potentials are fixed. What it does not tell us and does not ask is which paths out of available paths the system will take to get there. And this is the remarkable insight Swenson and colleagues made more than

two decades ago and then demonstrated with simple repeatable physical experiments (e.g., see Swenson and Turvey) in the mode of Joule and others in their demonstration of say the first law. By setting up a system with multiple alternative pathways where paths can be easily made available or eliminated for the system to "choose" from they showed that in each and every case the system will select the paths or assembly of paths that brings the system to equilibrium at the fastest possible rate (the LMEP above). Then, coupling this universal principle to Schroedinger's point that ordered states must produce entropy at a faster rate than disordered ones to satisfy the balance equation of the second law, the problem of why order is opportunistically produced from disorder was solved. And with respect to biospheric evolution as a whole and the "Gaia debate" where Lineweaver opines "a central issue...is whether the biosphere without other biosphere's to compete with, can evolve....However, if...maximum entropy production (is)...used to describe evolution" then the problem is solved." Well, yes, but solved a couple of decades ago and articulated in numerous journal articles (refs below) with one entire paper dedicated explicitly to it (Swenson's 1991 paper, "End-Directed Physics and Evolutionary Ordering: Obviating the Problem of the Population of One" ) where at the end in conclusion it says; "The Law of Maximum Entropy Production provides the selection principle that accounts for...evolutionary ordering and obviates the problem of the population of one." Bottom line, this book is an utter disaster and should be pulled by the publisher and libraries until it minimally contains a very explicit warning or can be re-edited to eliminate the deep technical errors, especially Dewar's derivation, as well as other errors like the editors' elementary misunderstanding of Prigogine's minEP and corrects the remarkable lack of legitimate attributions to the people who have really pioneered and developed this work. Of course that would make it an entirely different book.

Sample Refs.: Grinstein, G. & R. Linsker (2007) *J. Phys. A: Math. Theor.* 40 9717-9720; Mahulikar, S.P, & Harwig, H. (2004), *Conceptual investigation...Physica Scripta*, Vol. 70, 212-221; Swenson, R. (1991). *End-directed physics...problem of the population of one*. In *The Cybernetics of Complex Systems*, F. Geyer (ed.), 41-60; Swenson, R. (1991). *Order, evolution, and natural law...In Cybernetics and Applied Systems*, C. Negoita (ed.), 125-148; Swenson, R. and Turvey, M.T. (1991). *Thermodynamic reasons.. .Ecological Psychology*, 3(4), 317-348; Swenson, R. (1997). *Autocatakinetics, evolution, and the law of maximum entropy production*, *Advances in Human Ecology*, 6, 1-46; Swenson, R. (1998). *Thermodynamics, evolution... In The Handbook of Comparative Psychology*, G. Greenberg and M. Haraway (eds.); Swenson, R. (1999). *Epistemic ordering and the development of space-time...Semiotica*, Vol. 127 - 1-4 , pp. 181-222; Swenson, R. (2000). *Spontaneous Order...Development of Space-Time*. *Ann. New York Acad. Scien.*, v. 901, pp. 311-319, 2000.

I saw this book reviewed in Nature magazine. It had a rave review there, so I bought a copy. I wasn't dissatisfied, this is a reprint of all the seminal papers on the subject of MEP (Maximum Entropy Production). It is a modern version of Schroedinger's famous book - "What is Life". My only wish is that the book could be a little MORE mathematically complete.

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