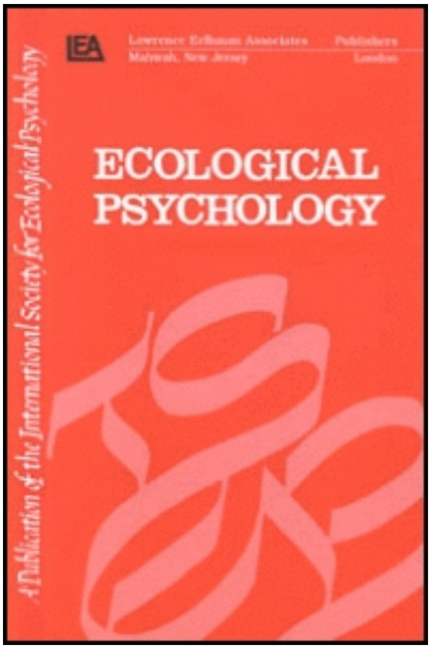
The Fourth Law of Thermodynamics: The Law of Maximum Entropy Production (LMEP)



An Interview with Rod Swenson

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Abstract

More than 20 years ago, Swenson (1988) proposed and elaborated the Law of Maximum Entropy Production (LMEP) as the missing piece of physical or universal law that would account for the ubiquitous and opportunistic transformation from disordered, or less ordered, to more highly ordered states. Given Boltzmann's (1974) interpretation, the Second Law of Thermodynamics has generally been interpreted as a “law of disorder.” Schrödinger (1945) and Bertalanffy (1952) had shown, however, that the Second Law, viewed from the classical perspective of Clausius (1865) and Thomson (1852), was not anathema to order. Ordered flow, including life, was permissible as long as it produced enough entropy to compensate for its own internal entropy reduction. The central problem remained, however: If the spontaneous production of order was “infinitely improbable,” as Boltzmann had surmised, then why were ordered systems such a fundamental and characteristic property of the visible world? LMEP provided the answer: Order production is inexorable because order produces entropy faster than disorder. In Swenson (1989d), LMEP was given expression as a precise law that could be demonstrated in falsifiable, experimental, physical terms. In Swenson and Turvey (1991), LMEP was tied explicitly to the progressive emergence of living things with their perception-action capabilities.

Introduction

In the spring of 2009, Mayo Martínez-Kahn (a thermodynamicist) and León Martínez-Castilla (an evolutionary biologist) conducted an interview with Rod Swenson in preparation for an article to be submitted to the Spanish language magazine *Educación Química*. The aim of the article was to inform students and educators about Swenson's identification and elaboration of LMEP. The interview is published here, in full, with references.

How did you arrive at the postulate? In other words, what was your creative path to getting a simpler view of nature that seems obvious once it has been pointed out but which no one had grasped before?

It is interesting to note that your question is from the point of view of someone who clearly grasps the idea (many still don't, of course), which, as you point out, seems so simple and so absolutely obvious in hindsight that it begs the question, “Why didn't someone figure this out before?” Indeed, when I began presenting the idea in public (e.g., at meetings of American Association for the Advancement of Science [AAAS] and **International Society for General Systems Research [ISGSR] in 1988** and in manuscripts shortly thereafter), even people who seemed to grasp the idea had trouble believing it. Their view was, “This can't possibly be true, otherwise someone would have figured it out before.” My impression, however, is that if we look at many of the ideas regarded as important or crucial pieces of the puzzle we are all presumably working on, namely, trying to understand and to know the natural world in a deeper way, we could say the same thing. That is, they *are* actually simple ideas (once we understand them, of course!), and they do seem obvious (in hindsight).

Take **Joule's (1845)** well-known experiment that I have used in several papers and often use when giving lectures on LMEP (**Swenson, 1997a**, **1999**, **2000**). I'm sure you remember it. It's the experiment where he had a suspended weight on a pulley that, on falling, turned a paddle wheel inside a box containing a thermometer. The experiment showed that as the weight fell, and the paddle wheel turned, the temperature of the water increased. It was used to prove the First Law of Thermodynamics, in particular by demonstrating the equivalence of mechanical energy and heat. But here's what's interesting and not always pointed out: It also demonstrates the Second Law. In fact, it requires the Second Law to work, and yet Joule, clearly a brilliant thinker and experimentalist, is not credited with discovering the Second Law. In fact, as far as we know he didn't see it in his experimentation (at least he never wrote about it). I think some of these things are actually so patently obvious that they are taken for granted in a way that makes them invisible—simply, one can't see them for looking. It was thus left to **Clausius (1865)** and **Thomson (1852)** (however you put this narrative together) to “discover” the Second Law.

Expanding on what was said earlier, I think this “failure to see” happens for a number of reasons—some having to do with hegemonic processes or groupthink in general (a topic beyond any full discussion here) and some having to do with the questions being asked and the problems being posed. It became apparent to me in the early 1980s (and this is nothing new) that the science of the time was divided (and still is, in many respects) into sharply defined disciplines, so much so that many of the deepest questions, in my view, were bracketed out from the individual discourses. For example, biology and evolutionary theory were unconcerned with the fact that their discourses segued poorly with what physics was saying, and vice versa. The biologists and evolutionary theorists, and so on, all took what was missing from physics to be a “property of life” or something of like kind. My impression is that most of them still think this way. As far as what the view from physics was supposed to be, it was the view that the Second Law—the law above all laws, as **Eddington (1928)** and others referred to it—was a “law of disorder.” By ignoring the distinction between the original nonprobabilistic Clausian view and **Boltzmann's (1974)** statistical view, people began beating the drum to the effect that physics, in simplistic terms, “likes disorder.” All I can say is that, to me, this just didn't feel right (“the problem of two rivers” with physics “flowing down” and life, culture, etc., “flowing up”). To say physics likes disorder seemed to me a failed reduction. I mean, if this were the case, then why do we see what we see in the world?

Whether it is in my blood, or learned from experience (wherever it came from, I don't know), I have never felt that nature, or “the world,” could possibly be discontinuous in that way (the incommensurable two rivers). Of course, it turns out that there are First Law symmetry reasons it can't either, but in any case, I was surprised to find that people were willing to just go along with the idea. Yet this has been an important part of the paradigm of modern science since its origins. So the problem of “two rivers” really was a problem for me. My inclination in a case like this is not to assume there's something wrong with the world but rather that there must be something wrong with our theorizing. At one point (and I'm not sure where because he is hardly read and referenced anymore), I stumbled on the extraordinary work of **Herbert Spencer (1862)** and his startling idea about the transformation of the “incoherent into the coherent” as being some kind of universal law. I wrote about this idea and quoted Spencer in some of the earlier papers (e.g., **Swenson, 1989b**). It was therefore remarkable when I met the culture theorist Robert Carneiro (1967, 1987a, 1987b)—whose trailblazing work on what could be called the “laws of form” or “allometry” of cultural systems (e.g., the circumscription theory on the origin of the state and the fissioning of autonomous hunter-gatherer groups) are discussed in **Swenson and Turvey (1991)** and **Swenson (1997a)**—to find that he, too, was a huge fan of Spencer and had, in fact, edited a Spencer anthology. Spencer's views flew in the face of ideas about ordering that would be advanced by Boltzmann some decades later. One day I had an odd thought that made me go back and read Clausius in the original. It was the way Clausius was typically quoted in contemporary introductory physics books, identifying the First Law and Second Law, respectively, as “the energy of the world remains constant; the entropy of the world tends to a maximum.”

One of the things we humans seem to do a lot is, if we hear something repeated enough, to go around repeating it some more without thinking about it. But on that day my thought was, “Wait a minute, the quotation sounds like a statistical form of the Second Law and Clausius didn't make a statistical argument.” Could he have really said that, I mean, even though it is almost everywhere, including the basic textbooks, could this really have been the way that he stated the Second Law? I tracked down the original German, and guess what? That's not what he said at all. Wow(!). What he said was not “tends” but “strives” (German verb *strebt*). Well, this was interesting in two ways: First it suggested the very active nature of the Second Law, which reinforced my own empirical experience; second, and this is just as important, it said that all the books that printed “tends” were wrong and what was really happening was that Boltzmann's view (his attempt to reduce the Second Law to a stochastic collision function) was being projected backward onto Clausius. Discovering errors of this kind was truly important to giving support to the view that there really was something missing or something wrong with the historical view of the Second Law. When we wonder why something isn't figured out sooner it is often, at least, partly to do with the view imparted to students, for example, that what we are handed is in some way unassailable, or perhaps worse, that all the fundamental stuff has been figured out. Of course this isn't true. We're still looking into a huge amount of darkness.

Whose work was useful one way or another as early references (e.g., Prigogine, Jaynes, Lotka, etc.)?

Well, I have already mentioned some of the people who were key for me, namely, Spencer, who had advocated for a “general” theory of evolution based on universal law and spontaneous ordering (although of course he had no explanation for where this “law of evolution” could come from), as well as the original texts of Clausius and others writing around the same time. You've asked about **Jaynes (1980)**, and because his followers have figured recently in this discourse I'll try to give a little more discussion on him later. For the present, in respect to his influence on me, the answer is “none,” except for this: given that the first thing I encountered from one of his followers began with “entropy is a measure of our ignorance about a system,” I was immediately put on guard to the problem of diametrically opposed meanings being attached to the same word. The tradition that influenced me was that of Carnot, Clausius, Kelvin, Mayer, and others where the ignorance of some person versus some other person, say about a mill wheel and the water flowing from a higher to a lower level, to use Carnot's example, has absolutely nothing to do with the extent to which water has the “motive force” to make the wheel turn. But I'll try later to touch on this a little more.

In any case, following my conviction of the importance of reading primary texts, especially after what I discovered with Clausius, I was then led directly to Boltzmann's own writings to see what he actually said rather than what others had attributed to him. In this case, his own words were clearer than clear when he stated (as I've quoted in several papers) that transformations from disorder to order, from his view, were “infinitely improbable” (**Boltzmann, 1886/1974**, p. 20). Now Boltzmann was a remarkable and interesting thinker, but he was completely wrong on this point. All we need do is simply look around. We see such transformations going on all of the time. So how can this be? Underscoring for emphasis, as Michael Turvey and I wrote in our 1991 paper, such transformations occur not with infinite improbability, as Boltzmann said, but rather with a “probability of one,” as evidenced in simple physical experiments (reliably, repeatedly, over and over again) as well as almost anywhere we care to look. If you look at the evolutionary record writ large, this is very close to the most deeply characteristic thing about it. So you ask yourself again, “Is there something wrong with the world or with our theoretical view of it?”

You asked about Lotka and if you look at the 1989b paper you'll see Lotka (1922a, 1922b) and others discussed, who helped give what was certainly indirect support for what I was by then, in the late 1980s, already proposing as a Law of Maximum Entropy Production. Lotka was like Boltzmann, a remarkable thinker, but he explicitly rejected the connection between evolution and the idea that might have connected him to LMEP, believing that autotrophs slowed down the process of the dissipation of the solar gradient and thus never following through to establishing a direct connection to the Second Law and from there the leap to LMEP. When looking at his arguments for his power principle, you'll see he views it as the consequence of the Darwinian competition for resources by living things, which he rightfully saw as energy transformers. So his evolutionary dynamic, and likewise that described for the most part by **Howard Odum (1983)**, who was also a prescient maverick thinker, was in the end the consequence of the *fecundity principle* (discussed in a number of my papers, e.g., **Swenson, 1997b**, **1998a**) and Darwinian selection, which follows from it. Of course, it was just this that needed explaining, and so the explicit leap to thermodynamics or the Second Law and then the connection to LMEP was never made by either him or Odum. (As an aside, some years later I was pleased to get a request from Howard Odum to use one of my photos of the Bénard cell experiment for one of his books.)

I want to go back to the 1989b paper briefly because it is often cited as a reference for LMEP and it is not a good one for that. This is because the law was not yet stated in the simple, direct, and falsifiable form that I was able to put it in a paper written later but also published in 1989 (**Swenson, 1989d**) and then in several other papers about the same time that reached publication over the next year and a half, followed by **Swenson and Turvey (1991)**. That 1989b paper is really more evident of my thinking a year or two before it was published; it was slow to get into press. So it is not a good place to go to see a statement of LMEP.[1] That said, the 1989b paper provides useful descriptive material and a full series of photographs of the Bénard experiment. The photographs were first made around 1988. They would also appear in another paper I did that same year (**Swenson, 1989a**) and a paper I did a couple of years later (**Swenson, 1992**). Most people writing about this kind of thing at the time I discovered it had never actually run or witnessed the Bénard experiment—just as they had not read Clausius or Boltzmann. The first time I did the experiment it was a nearly impromptu jury-rigged kind of attempt on the top of a warehouse building in New York City, where I remember setting my arm on fire briefly late one night. I did it later in a much more controlled environment. The full series of photos show not just the final state (the hexagonal array), which is usually the only thing people are familiar with, but also the development of the cells themselves. They yielded some very interesting results, demonstrating that, even in this simple nonliving system, there are generic processes of spontaneous fissioning, progressive determinism, and so on.

And this brings me to **Prigogine (1993)**. My entree into “spontaneously ordered” systems was not Prigogine but von Bertalanffy (1952; the developmental biologist and founder of General Systems Theory, which anticipated much of what was to come into science under the rubric of “open systems” or “complex systems” theory). He was a forward-looking and brilliant thinker who, in my estimation, was largely eclipsed by people like Prigogine because they were somewhat better at self-promotion. In any case, by that time I think Prigogine, largely coasting on his Nobel Prize, was giving fairly packaged talks, which were mostly a repeat of his Nobel Prize lecture (**Prigogine, 1977/1993**), and cranking out books with coauthors who mostly said the same thing without much involvement in the further development of the discourse. I was just beginning at this time to formulate an explicit statement of LMEP, and I had the task of giving a short introductory speech to introduce the keynote address by Prigogine at the very society Bertalanffy had founded and for which I was Managing Director for a brief period. I remember the dilemma for me at the time and concluded by saying, “It is hard to know where our present work would be without Ilya Prigogine.” I meant it quite straightforwardly. First, it is hard to know how much his discussion of “self-organizing systems” was responsible for getting people to take a look at these things, and that was clearly a very important role. Nobel Prizes can work both ways, but it gave more weight to the value of thinking about these kinds of issues and that was a good thing. The other side of the Nobel Prize, and similar prizes, is that people sometimes take their recipients to be less fallible in some way, or above criticism, or too influential on some issues, and that can be a problem.

In light of our current discussion, this takes us to the other side of the Prigogine legacy. Principally here I'm talking about the endless promotion by Prigogine and his school of the "Theorem of Minimum Entropy Production" in a way that helped spawn a lot of confusion about what it actually meant and which, in my opinion, Prigogine did not work sufficiently to reign in. As someone who had to swim fairly strenuously against this tide for a while, it is fair to say that the promotion of the theorem set the discourse off on a very misguided path for some years, and in fact this confusion remains fairly common today. If you look carefully at his own core papers, to be fair, he was quite clear about what it meant (e.g., see his Nobel Prize lecture, 1977/1993), but the misconception has had the impact of muddying the waters and slowing the forward movement of the discourse. To my thinking, many people are still confused.

The main problem, of course, as you know, is that people think that "minEP" runs counter to LMEP, which is not in any way the case. Both are entirely true. MinEP is not a universal principle, applying only to a narrow range of near equilibrium systems, whereas LMEP is entirely universal and applies to all ranges. But, to repeat, they are not at all about the same thing in any case. MinEP says the following: Consider a system, near equilibrium, specifically in the linear range, consisting of a number of thermodynamic "forces" (gradients of potentials or disequilibria), X_i and their corresponding flows J_i, and where the X_i are not replenished. Without replenishment the X_i are progressively dissipated so that they would all go to zero except that some, say at least one, say a temperature difference over the system, is maintained. In such a system the entropy production will go monotonically down as the forces are dissipated until the system gets as close to equilibrium as it can where, as long as the heat gradient used in this example is maintained, it will remain in a steady state near but not at equilibrium. The entropy production will now be at the lowest point of the whole process (and it will have gone monotonically down to get there). This, in essence, is all MinEP says. It may be seen that this is really nothing more than the Second Law (that potentials are spontaneously minimized) and the fact that in the linear regime the entropy production is given by the sum of the product of the various flows and forces. This means that as the forces are monotonically dissipated according to the Second Law, the entropy production goes down monotonically. In fact the theorem has been called trivial because it really doesn't add anything much new. Of course, if the heat gradient in this case were not maintained, and the remaining force thus were allowed to dissipate, the entropy production would continue going down until the system reached equilibrium where the gradient or potential would be gone and the entropy production would be minimized in the limit (i.e., the entropy production would be zero, and the entropy would be maximized).

What the theorem doesn't apply to, and which Prigogine and his school correctly underscored, is the case where the system is sufficiently far from equilibrium that you get autocatakinetics or spontaneous ordering, for example, like the classic Bénard case. The reason is that, at that point, rather than going down monotonically, the rate of the entropy production dramatically goes up. But most significantly, with respect to LMEP, what minEP does not address at all with respect to minimizing those forces (whether in the linear range where it is valid or the nonlinear range where it is not) is *which paths, out of otherwise available paths, a system will take*. That's exactly the question that was not at that point asked and the one that LMEP specifically answers. In particular,

> LMEP: 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.

LMEP is valid in the range minEP is valid in and the range it is not. LMEP is valid in *all* ranges. It is an entirely universal law. And it answers the question Prigogine's theorem doesn't answer and Prigogine never asked, but it was just *the* question which, by the late 1980s, seemed to me in need of answering ("which paths or assembly of paths out of available paths …"). The answer by that time, I would say, was being telegraphed to me and actually seemed to force the facing of the question. In other words, all of this "problematic" or "anomalous" behavior (viz., spontaneous ordering vs. the generalized Boltzmann view) was *always* accompanied by an increase in the *rate* of entropy production. That's the one thing that jumped out to unify this behavior when one started empirically to look at these systems, whether the Bénard cell experiment or any other. I mean to say, it actually makes one feel a little slow in hindsight not to have grasped it earlier. Because it turns out it must be so. You're collecting all this data without realizing that the transformation from disorder to dynamic order *must* be accompanied by an increase in the rate of entropy production or else the Second Law (as recognized by Clausius and Kelvin) *would* be violated. You'd be violating the balance equation of the Second Law. But, even if you didn't grasp it, you would think that it would have been obvious because Shrödinger and Bertalanffy had already made the point, although *implicitly*, when they noted that as a requirement for these kinds of systems to exist or maintain themselves they *had* to increase the rate of entropy production in direct and lawful relation to the amount of order (or entropy reduction) that defined their existence.

But back to Prigogine for a moment and another example, in addition to the confusion over minEP, of the kind I think helped throw people (unintentionally of course) off the track. It was Prigogine's writing over and over again in books and papers (and you can still find this in press) that “it came as a great surprise when it was shown that for systems far from equilibrium the thermodynamic behavior could be quite different … even directly opposite that predicted by the theorem of minimum entropy production” because the rate of entropy production went up instead of going down (**Prigogine, 1980**, p. 128). He said it in his Nobel Prize lecture too if memory serves.[2] But what is amazing to me, reading these words some decades later, is how this could possibly have been “a surprise” at all, not to mention a “great surprise” even just given an understanding of what Bertalanffy and Shrödinger had noted some years before, let alone a basic understanding of the First and Second Laws themselves. If the entropy production *didn't* increase, now that would have been a *real* surprise because you would have violated the Second Law, you'd have perpetual motion machines and all the rest. The problem, however, was to understand why such transformations occur and why they occur ubiquitously whenever they get the chance.

In any case, the major clue that this behavior was throwing out, and this is the main point, was that the thing that characterizes *every single one* of these transformations (disorder to order) was that the rate of entropy production increases, and this is what I meant earlier by “being telegraphed to me.” Because at this point the remaining pieces to the puzzle were staring me in the face. And it was haunting me. But I think some part of my unconscious kept pushing it away, saying, “Well that's so obvious it can't possibly be true.” But then at some point as I started to do *Gedanken* run-throughs over and over again, I found no way to refute it. Here's a case of where these things that should otherwise be anomalous or problematic start popping up everywhere as soon as they get the chance. If the reduction of potentials or gradients, the Second Law, were governed by a rate principle, namely, if it were the case that the world (as all of this is saying to me now) acts to degrade these potentials as fast as it can, given the opportunities or constraints, then the whole thing is explained! And then it had to be a matter of developing an experimental model that could show (or not) if this were true, believing by this time intuitively anyway that it absolutely was. Because once one understands the law, as you've said in your original question, it seems like the most obvious thing in the world. Graduate students (or anyone else, I would say) once they get it can proliferate examples to illustrate it, or test it, or prove it, all day long.

Prior to this (**Swenson, 1989b**, **1989c**), I had come to realize that you could not get a strong proof by looking at far-from-equilibrium nonlinear systems themselves. Why? First, because in these types of systems you are already dealing with such complexity that you cannot honestly make a nonsubjective assertion about alternative pathways. It's always going to be ad hoc. The original laws of thermodynamics were all demonstrated and proven using falsifiable, reproducible, physical experiments, and that was what was called for here. Of course, as **Popper (1985)** recognized and people like **Lakatos (1970)** developed further (e.g., see **Swenson, 1997b**), you can't ever prove a theory, you can only falsify it. Meaning, to put it simply, because we cannot exhaustively know everything everywhere we cannot actually prove that something is true.

When we take something to be a “true” or “proven” theory (or fact) we mean that we have stated the theory in falsifiable terms, terms such that it would be possible to prove the theory false or wrong, and then demonstrated it by (in the best case) setting up the conditions that would falsify the theory in repeatable, reproducible ways. Such a methodology is the most rigorous possible way of performing what we call a “proof.” Take the First Law, for example, a law on which all other laws depend. So we say “energy is conserved” and this is taken to be proven or demonstrated, say by Joule, with the experiment we discussed earlier. And it *is* proved, and it is proved as absolutely or rigorously as any thing or any law is, but in the sense just described. Joule's experiment on the First Law, and those done by others, is an example of a repeatable demonstration of a falsifiable assertion. And every time it is done it holds. So we accept this as a law, and with good reason: this is a very rigorous test. But of course we can't say with metaphysical certainty what goes on in places we're not measuring or checking at any given time; maybe it isn't being conserved there. But at some point, from the principle of parsimony, this begins to be a fairly useless way to think. What we do know is that wherever we do check the law, it holds. So we take it to be true.

So this is where I was in 1989 when I went from the "*Systems Research Stage*" to the "Gauss-in-a-Box Stage." It was a matter of finding an experimental model similar in simplicity and clarity to the experimental devices of the early thermodynamicists but with alternative pathways and rates. And so that's what we did. Borrowing some tools from the early thermodynamicists we conceived a gas in a box separated by an adiabatic wall where the temperature of one side, *A*, was greater than the temperature of the other side, *B*, creating a thermodynamic force between them. If you imagine a number of places in the wall where you can peel off the adiabatic seal exposing, say, from one to four pathways for the heat to flow from *A* to *B*, and each with a different coefficient of conductivity, then you've got a measurable, reproducible, falsifiable test. This test was published that year in more detail in the *PAW Review* and also in a series of papers published that followed, including the one done with Michael Turvey published in 1991 (and, e.g., **Swenson, 1991a**, **1991b**, **1992**, **1997a**). Rather than "Gas in a Box," the *PAW Review* paper was called "Gauss-in-a-Box" in honor of (Carl Friedrich) **Gauss (1829)**, who developed a "Principle of Least Constraint" for mechanics.

In any case, what the model shows is that if you have a system where you set up some gradients of potentials or thermodynamic forces, and, say, you provide a single pathway, then you make another available that provides a faster path, the system will "choose" the faster path. At the same time, because this will typically not eliminate all the potential instantaneously, then what remains will be "allocated" to the slower path, and of course you can add as many paths as you want with different "coefficients" of conductivity (or whatever) and the system will put together the "assembly of pathways" that minimizes the potential (reduces the gradient or maximizes the entropy) at the fastest rate given the constraints. And that's the law. That's LMEP, and totally falsifiable, meaning set up a system and show where it doesn't happen. No one has ever been able to do it. Just like no one has been able to show the failure of the conservation of energy or have Joule's experiment come out any different from when it was done the first time. So it's falsifiable and reproducible. What's more, of course it can be done with any energy gradients or forces (chemical, electric, etc.) or combination, not just of course a temperature gradient. It's the same in every case and entirely universal. And as you said in your first question, it is so remarkably simple, so obvious, yet so clearly remarkably powerful in its explanatory capacity. The box with the different paths then led to the heated "cabin in the woods" kind of real-world example in **Swenson and Turvey (1991)** that put the law in intuitive terms anyone could understand and grasp when you explain it (where the alternative pathways are doors, windows, the conductivity of the walls, cracks in the walls, etc.). The cabin example invites people to start coming up with their own examples or illustrations.

That, in a very rough way, traces the steps I think you were asking about. At least I hope so. To sum up with a punctuation mark, although I know I needn't do this on your behalf, the question we originally began with (why dynamic "order" seems to be produced from disorder or less ordered states opportunistically wherever and whenever it gets the chance) is answered, of course, like this. LMEP tells us that the world acts to minimize potentials or to maximize the entropy at the fastest rate given the constraints. If we couple that fact to the knowledge (given the balance equation of the second law) that the production from disorder to order increases the rate, then we know why, as we say, "the world is in the order production business." Ordered flow produces entropy faster than disordered flow, and so the world can be opportunistically expected to produce it whenever it gets the chance.

What do you think today about the general validity of the postulate?

In my view, it's about as solid as anything we call a law can be. It is as metaphysically certain as a law or anything can be. Pigs may fly someday but they don't fly yet. Recently, asking for my comments, someone sent me a draft of a paper using

LMEP to develop some ideas in economic theory where at the beginning in the abstract it said, “LMEP is a universal law, as universal as Newton's.…” I suggested removing the “as Newton's” for the reason that it is generally accepted now, following Einstein, that Newton's laws are “approximations,” and I don't take LMEP to be an approximation. So, one should say, “LMEP is *more* universal than Newton's laws.”

As I've written, I view the laws of thermodynamics as the most fundamental laws of all; in fact, they are best considered “special laws” or laws on which all the other laws depend, LMEP included. As with the First and Second Laws, LMEP, I would argue, is actually implicit in, or a requisite of, the action of all other laws as well as the cognitive or epistemic act of the person(s) studying or measuring those laws. A number of papers of mine focus on this discussion (e.g., **Swenson, 1998b**, **1999**, **2000**).

Do you think that we are facing a “Fourth Law of Thermodynamics”?

Yes, for sure. Some people have misstated that LMEP is part of the Second Law. It's not. The Second Law explicitly says absolutely nothing about rates. So either we change the Second Law or we recognize LMEP as a new law. Honestly, I don't see how the former works very well because it is then, roughly, “Entropy always increases (or remains the same at equilibrium) and it does so at the fastest rate.” That's clearly two statements and clearly two laws, which together with the First and Third Laws, give a total of four!

There appears to be some difference in views on the epistemological status of LMEP. How do you regard this?

Well, I'm going to assume here that we're talking about the general issue of whether, as some would like to imagine, irreversibility (the Second Law and LMEP) is taken to be somehow a human construct, say of “mind,” a consequence of “ignorance,” the way we model systems or the like. Honestly, why it is that a very large number of humans are drawn to this point of view is a complete mystery to me. The reversibility of quantum mechanics, and the theories of **Shannon (1948)** in particular, and then **Jaynes (1980)**, as I'll discuss further, clearly have played a huge proximal role in helping people to slide into this way of thinking. But it is a human propensity to do so that goes back at least as far as Parmenides and then finds its way into the foundations of modern science with mind-matter dualism of Descartes. Parmenides, you'll remember, postulated a true reality of perfect symmetry, with change of any kind, therefore, being thus an illusion (all the arguments of the Eleatics). The central problem was that he'd postulated a reality that had no place in it for him as a postulator, even as an illusion (and I'll here avoid the discussion of the further problems of intersubjectivity).

In any case, as soon as he opened his mouth, set down his pen, or had a thought, such an action entailed irreversibility. The same is the case for quantum mechanics and the so-called measurement problem, certainly the most publicized form of quantum mechanics. The fatal problem for “quantum theory realists” who believe that quantum reversibility is the “true” and exhaustive reality, and thus irreversibility is somehow an illusion or a creation of human “mind,” is that quantum theory needs the action of an irreversible measuring device to work. And thus this, and all that it entails (which is huge), sits somehow either dualistically, or in some fantastical “transcendental” scheme, outside the “real” world. A lot of completely awful popular quasi-science books have been written from this point of view.

But from the standpoint of the philosophy of science, then, because I'm assuming we're talking science, the question is, “What are your demarcation criteria going to be?” There is not a theory, a point of view, an assertion, whether or not it asserts or postulates the reality of irreversibility, that doesn't at the same time entail it. So by what upside-down version of the most ordinary, rigorous, and widely accepted demarcation criteria (criteria for truth or theory selection) are you going to say, “Well, I'm choosing to bracket all that outside my theory or view of reality”? For the weaker test of “sophisticated falsificationism,” where the goal is to be able to decide on the validity or utility of a theory where falsificationism in the strong sense may not be available (as it is, to underscore again, in the case of the First and Second Laws and LMEP), additional criteria such as the ability to postdict instead of necessarily predict, arguments from parsimony, such as the extent to which a theory reduces the number of explanatory devices (rather than increase them), are also allowed.

The "product of mind" view of the Second Law or irreversibility fails on every one of these as well. We now have to take everything entailed, say, in a generalized measuring device, which includes nothing less than directionality, the ability to discern differences, intentionality, meaning, and the like, and put it outside the physical world. And this is simply the old Rylean "ghost in the machine" or "homunculus" redux. It's great for selling pseudoscientific new-age pop science books but a disaster for science.

Without doubt, when **Shannon (1948)** used the word "entropy" in communication theory in the 1940s for a completely subjective and nonthermodynamic measure of the Second Law, the floodgates for would-be subjectivists burst wide open and the damage has never been undone. Ironically, no less than Jaynes (e.g., **Jaynes, 1980**) himself rightfully complained about Shannon's nonthermodynamic usage of the word "entropy," a measure of an observer's ignorance, and how the misuse of the word had now reached "disastrous proportions" to become the most "abused word in science" as a consequence (p. 593). Ironic, of course, because Jaynes, following Shannon's lead, used the term "entropy" himself to refer to the nonthermodynamic measure of an observer's ignorance, and many of his followers have hardly made it clear that there is a radical distinction between the two very different kinds of meanings and in many cases implicitly if not explicitly encouraged the conflation of the two meanings to produce truly fantastical subjectivist interpretations. So at the same time that Jaynes rightfully pointed out that using the same word with "different meanings … prevents many from seeing the meanings are different," he was calling his formalism the "Maximum Entropy" or "maxENT" formalism, where there is no distinction in the title in any case between it and actual physical, thermodynamic entropy (p. 583). In his lexicon he does clearly draw the distinction between thermodynamic entropy S_E and "information entropy" S_I where he says clearly that S_E is the term referred to by the Second Law and "observed in physical experiments" whereas S_I is simply "a property of any probability distribution" and not tied to any physical quantity (p. 584). Those are very important words that many of his and Shannon's followers sometimes seem to forget.

What Jaynes does not point out, in addition to Shannon's (and his) upside-down use of the term "entropy" (from a measurable physical quantity to a subjective nonphysical property determined by how much someone knows about something), is the upside-down use of the word "information" by both of them. In the sense they use it, it is not "meaningful information" at all (and isn't that what the ordinary person thinks "information" means?). Instead, it's effectively just pure syntax. Imagine how hard it is, as Jaynes himself correctly pointed out, for people, in order to avoid the serious error, to keep reminding themselves roughly, "Okay, by 'entropy' here it doesn't mean 'real' entropy," and then imagine the same kind of thing for the word "information" and it is easy to see how this got so bad so fast. There must be volumes and volumes of articles, and they're still being written today, that continue to make remarkably remiss assertions based on these conflations or errors.

Recently, **Dewar (2005)** claimed to have derived a "principle of maximum entropy production" or "MEP" using Jaynes's maxENT formalism and then with a small, mutually citing group, published a book featuring the group with Dewar's "derivation" at its foundation (**Kleidon & Lorenz, 2005**). Jaynes himself should not be held at fault for the variety of errors that were made in this work or for its other distinguishing feature—a pronounced failure to adequately cite the work that had been done on the subject during the previous few decades. The errors include the misconstruing of Prigogine's minEP theorem, failing to adequately deal with the problem of reproducibility as required by the Jaynes method for their claims, and failing to appreciate the inability (except in an ad hoc way) to delineate alternative pathways in their model that could validate their assertions. The preceding list is partial at best. All of this is moot, of course, because not long after Dewar published his "derivation" on which all this builds, **Grinstein and Linsker (2007)** showed that Dewar's derivation failed or was invalid on mathematical grounds alone. My point before this was, to be clear, that even if the math were correct, it would have failed in any case on other technical issues that the Jaynes method itself requires. Of course, finally, even if the math were valid, and even if all the other conditions were met, it would have only confirmed, but added nothing to, the fact that LMEP has already been proved for approximately 2 decades in a falsifiable real-world physical way. To the extent that the Jaynes method is a useful method, it is only useful, as Jaynes himself underscored, as an inference method proceeding from incomplete information in the case where direct experimental or verifiable (or falsifiable) physical results are not available or possible. The work of Dewar and the small group around him fails on all these counts.

Remarkably, this suggests that none of the contributors to the book, who built their work on **Dewar's (2005)**, particularly the editors, could have paid much attention to the derivation, or perhaps even read it at least in detail sufficiently, as did **Grinstein and Linsker (2007)**, to see the errors. What is more to this point, even after the bottom was pulled out from under the book there has been, as far as I know, no reediting, no new foreword, or anything, warning the reader that the core claim of the book is invalid. So, not only are people misled by citations that made it into print and remain there but also people continue to be misled by the book still being in print.

Returning, though, to conclude on the epistemological issue: yes, there are questions raised about the "objectivity" of irreversibility or the Second Law or LMEP, but these to me are not legitimate scientific questions. Beyond quantum mechanics "theory realists," **Shannon (1948)** opened the floodgates with his use of the extraphysical or subjective use of the word "entropy" and **Jaynes (1980)** followed suit, using the word although making the distinction. I side with Prigogine and Eddington[3] before him on all this. Despite the vast misuse of the term "entropy" or bizarre assertions that have been made as a result of these unfortunate usages, it does not change the fact that if I put a cup of hot tea in another room where I can't see it the temperature of the tea will come to the temperature of the room regardless of my knowledge or ignorance of the situation. If it did not, then we would have perpetual motion machines. And that would be just the beginning. Consider that there would be no "we" to know about it.

Have you recently changed your view on Boltzmann's approach to entropy?

No, not at all. But I hope to be clear this has nothing to do with claiming that his formalism is not useful (or that he was not a brilliant and historic thinker). It is rather his underlying claim from his own writing that transformations from incoherent to coherent, or disorder to order, are the most improbable things conceivable—"infinitely improbable," in his words. That's simply wrong. So all of his immense contributions aside, in the case of his attempt to reduce the Second Law to a stochastic collision function, he was wrong. **Prigogine (1980)** was right on this and out in front of the pack in stating it. As Turvey and I wrote in our 1991 paper (and as I've noted elsewhere) these transformations that we see all around us happen not "infinitely improbably," as **Boltzmann (1974)** said, but with a "probability of one" each and every time. Boltzmann was fairly wildly wrong on a number of other things too; for example, at one point he advanced postulates aimed at preserving the notion that the world or universe was actually in equilibrium (e.g., the idea of opposite worlds, roughly some other planet where evolution would be having to work in reverse in order to balance the upward ordering on our own planet, etc.). The development of his ideas is extremely interesting, to say the least, but clearly beyond discussion here. On the development of the improbability of order, however, we should recognize that it was taken and elaborated and repeated and reified (at that point, the fault of others, not his!) into *being* the Second Law, a "law of disorder." It is an idea that has been a huge weight or obfuscator that needed to be thrown off (you can still read this view of the Second Law in a lot of places including, unfortunately, a lot of introductory or popular texts). It was precisely upside down: not improbable … but "probability of one" and expected as a consequence of natural law, not, in any sense, either working against it or a surprise in the face of it. That's the main point brought to us by LMEP.

In your opinion, what is the relevance of LMEP to the evolution-creation debate?

Yikes! You know, to do justice to any one of your questions would take a book. So with apologies for being far less than thoroughly thoughtful I'll try to give you a (necessarily shallow) response.

In the end, at any halfway deep level, LMEP has no relevance to the debate at all. "Halfway deep" meaning dogmatists on each side will simply adjust their arguments (auxiliary assumptions or definitions). Because that's what they are, mostly, dogmatists, meaning there is no real debate. On a different level, you could say of course that by providing a law-based account of that which ordinary evolutionary theory could not explain (actually the fundamental properties of life, etc.), it's a boost for the evolutionists. Evolutionary theory is a lot, lot stronger with LMEP, now unifying the two rivers or getting rid of the incommensurability between physics and biology, giving us a way to see evolution as a planetary phenomenon (e.g., see **Swenson, 1991a**, **Swenson & Turvey, 1991**). Ironically, however, mainstream evolutionists (neo-Darwinists of some stripe) don't want to know about it or don't get it either. I think they see it as encroaching on their territory in some way or think that physics is then going to replace biology. I have clearly been deeply critical of neo-Darwinism, of course (where this view, say, would be characterized by people like **Dawkins and Dennett [1989**, 1995]). For example, see **Swenson (1997b)**, in which I critique this whole way of thinking that, in my view, is as dogmatic, compartmentalized, and/or “magical” as any dogmatist of the other side. It is ironic too that the reductionists like Dawkins and Dennett become the flag-wavers or standard-bearers for the “evolutionists” or scientists in the “Creation vs. Evolution” debate. I actually don't think they are good representatives for either of these roles. But LMEP certainly takes a big step in providing a way richer, fuller, and more universal theory of evolution, thus making us feel more deeply connected to the world in a relational and intelligible way. For me, that's a good thing.

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Notes

[1]Swenson's aside: “At its origins and for more than a decade and a half 'MEP' was used by me and others for LMEP. Unfortunately, in the last several years some authors have adopted 'MEP' indiscriminately to make different, sometimes vague, and often wrong, assertions about entropy production. In an effort to try to avoid the confusion that has resulted I generally use simply 'LMEP.'”

[2]In his Nobel lecture (1977/1993) Prigogine said, “It came as a great surprise when it was finally shown that far from equilibrium the thermodynamic behavior could be quite different, in fact, even opposite to that indicated by the theorem of minimum entropy production” (p. 88).

[3]**Eddington (1928)** wrote, “If someone points out to you that your pet theory of the universe is in disagreement with Maxwell's equations—then so much the worse for Maxwell's equations. If it is found to be contradicted by observation—well these experimentalists do bungle things sometimes. But if your theory is found to be against the second law of thermodynamics I can give you no hope; there is nothing for it but to collapse in deepest humiliation” (p. 74).