

Interactive comment on “Constructal theory of pattern formation” by A. Bejan

Anonymous Referee #1

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In this paper, the author reviews the so-called “constructal law” that he formulated about 10 years ago. The author provides a broad range of examples where this law seems to explain emergent relationships. The manuscript is generally well written and reasonable to follow.

While I acknowledge the author’s great achievements in his research, I have three major problems with this manuscript that prevent me from recommending publication in its present form:

- First, the author ignores to mention a wide range of literature of alternative, and often similar explanations of the expressions that he comes up with. For instance, work authored and co- authored by Rodriguez-Iturbe started with a similar notion of minimizing dissipation in river networks that is very relevant to section 5 of the manuscript. His statement on page 5 (end of second paragraph) that “the duct architecture will

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evolve such that the entire system reaches equilibrium the fastest” sounds very much like Schneider and Kay’s theory of “nature abhors a gradient” (e.g. as described in their 1994 paper). Furthermore, the upper bound theory in fluid dynamics, developed by Howard, Malkus and Busse in the 1950ies and 1960ies makes very similar statements regarding turbulent flow. Likewise, the work on the principle of Maximum Entropy Production also makes similar statements regarding turbulent flow (see e.g. review by Ozawa et al., Rev. Geophys. 2003; contains references to Howard, Malkus, and Busse’s work as well). Since the latter principle has now a fundamental derivation from statistical physics/ information theory (Dewar’s 2003, 2005 work), I think that this is a more fundamental, and better phrased principle than the author’s “constructal law”.

- Second, the author promotes the “constructal law” as a fundamental, physical law. Yet on page 7 the author provides an example where the “best” flow configuration of the round cross section does not work (also, page 9, half circle shape for river beds). An objective physicist would then state this as a fact that falsifies the “law”. The point I want to make here is that one has to be very careful in formulating this. When the author then mentions that hexagonal ducts have the “advantage that they can be packed in parallel into bundles, to fill a volume”, then it seems to me that there are other relevant constraints at work in the optimization process as well that were not considered in the simple derivation.

- Third, the author makes rather broad claims that are not supported. Two examples are on page 22: first line: “Hydrology research is proving every day that science has hit a wall.” and second paragraph: “Needed are new laws of physics.” While I am supportive of these statements to some extent, the author needs to be more careful. What the author has shown in the reviewed work is that established relationships can be derived from a relatively simple approach, but he has not shown that theory is really necessary in that it improves existing relationships. Unless the author demonstrates a case where conventional hydrological theory clearly fails, but relationships derived from his approach do not, the author’s research does not support these claims!

In summary, I find it rather unfortunate that the author fails to relate his research to other, very similar theories. It gives the manuscript a certain flavor of ignorance and arrogance, even though these works add supporting strength to the author's work (or vice versa). If the author's objective is to reach a broader audience with this review, he would be better served by scaling down his claims and link his work to related research. This would make this manuscript much stronger as it will be more accessible to a wider audience rather than to alienate an unfamiliar reader.

Minor comments:

- page 8, 7 lines from bottom of page: "Once again, diversity of shapes \checkmark is demanded by the constructal law.". In the text I do not see an explanation for why this is so.

- page 10, 9 lines from bottom of page: "It is the reality of the continuum that is routinely discarded by modelers \checkmark ". I object to this statement. Modelers do this for practical reasons, not out of ignorance. And then the author describes a discrete scheme in the last paragraph of page 11. So what am I supposed to think of this statement (or his model), when he uses a discrete scheme himself?

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