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Hydrology and Earth System Sciences Discussions

Interactive comment on "Constructal theory of pattern formation" *by* A. Bejan

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I read this paper with interest. The author proposed the third law of physics in his early work and summarized his applications based on the theory. Although this review paper is interestingly written, I cannot recommend its publication at the present form. This is because the presented theory and applications raise several questions, big and small, as described below.

At the first reading, the author's theory seems akin to earlier optimality-based theories that attempted to explain natural phenomena. Even only in the area of surface water hydrology, we can find many such theories, e.g., minimum unit stream power [Yang (1973). Journal of the Hydraulics Division (ASCE), 99, 1679-1704]. Extremal theories have been used to explain meandering [e.g., Chang (1979). Journal of Hydrology, 41, 303-327], the formation of riffle and pools [e.g., Yang (1971). Water Resources Research, 7, 1567-1574], hydraulic geometry relationships [e.g., Langbein (1964). Journal of the Hydraulics Division (ASCE), 90, 301-312.], river network forma-

tion [RodrIguez-Iturbe et al. (1992). Water Resources Research, 28, 1095-1103], etc. There have been many debates over these theories in '70 and '80 [e.g., Griffiths (1984). Water Resources Research, 20, 113-118]. The author's theory is similar to these theories in that explaining natural phenomena based on a certain universal principle. None of these early theories is cited in the manuscript.

I feel that the author wants his theory to be regarded as being independent from early extremal theories. For example, the author mentioned that 'no configuration in nature is predetermined or destined to be or to become a particular image. (p3)' which argues against many earlier optimality principles. The proposed theory is general, in fact too general. For example, he said 'diversity goes with the constructal law, not against it (p7)', which means that deviations from optimal shape are a part of the theory. He even mentioned that 'diversity of shapes on the podium of high performance is DEMANDED by the constructal law. (p8).' The author even tries to explain 'biological evolution' within the framework of his theory (p5). Proposing the theory to the level of a general concept, the third law of physics, may help it avoid critiques made on earlier extremal theories. However, this vague generality is a problem to me.

The author asked: is there a single physics principle from which form and rhythm can be deduced, without any use of empiricism? The biggest problem I have is that the theory is too general and, in fact, fails to answer the question of himself. The theory, if true, only provides a rough guideline and all detailed patterns cannot be explained, but merely treated as 'diversity' or 'random.' Since the proposed law cannot 'deduce' actual patterns and just be a general theme, I may ask what is the use of this law?

I also have problem with his comment on biological evolution. Even if this theory is general enough to be a law of physics, there are still huge differences between physical and biological processes. In biological evolution, the evolution does not occur within the lifetime of a living being but is carried out over many generations with genetic signals transferred between generations.

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Regarding many applications that the author mentioned in the paper, I believe that process-based perspective can provide more plausible explanation. In all examples he showed, i.e., duct cross-sections, open channel cross-sections, tree-shaped flows, and turbulent flow structure, it is believed that physical processes occur at local scale and the pattern emerged at the greater scale is the result of such self-organization procedure. For example, evolution of duct cross-sections may result in the state of less resistance, but 'low resistance' is the consequence of local physical processes instead of the cause of the process.

In summary, unlike the author's argument, proposed theory is insufficient to predict the occurrence of flow patterns in nature. The difference between optimal shape and natural pattern is too significant to be treated as the 'diversity.' The proposed theory is too general and the manuscript does not include convincing proof for the proposed theory to be the third law of physics, which is a very ambitious task. Therefore, with current format, this manuscript is likely to raise only confusion among HESS readers.

If the author decides to describe the proposed theory in a more focused manner, the author should cite all relevant earlier extremal theories, along with critiques, and address that how this theory is different from earlier ones and how the author overcomes the critiques. This is especially required since the author intends to write a review paper.

Finally, I am also not convinced with the necessity of this review paper, which reviews mostly the author's own works. All applications presented here are believed to be found in the author's other publications (including a book). Minor comments are enclosed below.

Small comments:

I cannot understand what the author means by the proportionality between width and depth in rivers (p8). Evidences show that they are power functions of discharge [Leopold and Maddock (1953). U. S. Geological Survey Professional Paper, 252], but

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their exponents are different. Mostly increase of depth hits limits (depth of very deep rivers is still the order of less than 10 m) while river width may increase up to the order of a few kilometres.

In the example of open channel cross-sections, note that semicircular shape is rarely found in nature. Semicircular shape is often preferred as an engineering solution (can be found in basic hydraulics text books) but this is far from natural setting. In terms of ecological perspective, semicircular shape is not friendly. Note that the natural channel is not that simple corridor, but is a system where ecological, hydrological, geological, geographical, and climate conditions all play roles in shaping the structure.

Drainage basin problem: I am not sure whether the author is dealing with river drainage network (surface water) or subsurface flow network. If this is the former, the formulation based on Darcy's law may not be the best way to describe the process. The author should provide more reasoning of Darcy-based formulation for surface water process. As the author agrees, Fig. 6 & 7 do not look like natural drainage basins. However, contrary to the author's opinion, Fig. 8 is also very different from natural drainage patterns in my view. To claim his opinion, the author is recommended to show quantitative network characteristics (such as size distribution, length distribution, etc.) of his results and compare those with values of natural drainage basins.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 1773, 2006.