

Editorial

Editorial Note from the EiC

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The material published in this “Discussion”—and the very reason for which we decided to publish it—requires a clarification on the part of the journal Management; therefore, I advise readers to peruse this foreword before embarking in the task of studying the often polemical statements and counter-statements contained in the Seibert and Rees paper, in the Diesendorf and Fthenakis et al. critique, and in the replies by Seibert and Rees.

Let me first reiterate that at *Energies*, in the over 12 years of my tenure as EiC, we have consistently made every effort to adopt a completely “unbiased publishing policy”. This means that any scientific opinion—controversial as it may be—on any topic falling within our journal’s scope is peer-reviewed with the utmost attention to its interest for the energy-conversion-systems community, to its scientific merit, to the methods of the research and to the appropriateness of the citations, conclusions, ethics, and academic style. Our record in this matter is immaculate and a source of great pride for us.

For a series of reasons, the original Seibert and Rees manuscript (S&R in the following) slipped through our system in spite of the warning signals given by two of our reviewers: it would be useless to explain the technical reasons of such a mistake here, but as the Editor in Chief, in the end, it is my own responsibility to enforce our publication standards; therefore, I must begin this foreword by asking our readers and our constituency to forgive me for accepting the original manuscript without requiring the authors to make some obvious corrections (that, in light of their response reported below, I believe they would not have accepted).

First of all, the original S&R paper is not a “review paper” but clearly an “opinion paper” (see, for instance, Section 4.3 in the original S&R paper and the last sentence in their response to Diesendorf). We removed the attribute “review paper” from our records as soon as some of our EB members signaled this mistake.

Second, the original Seibert and Rees paper is not only clearly an opinion paper but also a strongly biased one. This emerges from a careful analysis of its original text and of the authors’ responses to Diesendorf and Fthenakis et al. I have made a personal list of the inconsistent “technical” statements in their writing but chose not to report them here because this is obviously not—nor should it become—a personal “technical bullfight”. One point is, however, noteworthy: the fundamental idea that the overshoot is the only measure of ecological impact is an opinion not substantiated by facts, and presenting it in such a fideistic fashion constitutes a profound lack of respect for the large community of scholars, researchers, and experts that hold a different opinion and propose different environmental indicators. I would also like to signal that S&R’s contention that theirs is the only promising approach to the much-needed transition to (pseudo) sustainability is just plain wrong. As for their quoting Kuhn (why not Popper and Lakatos?) in their final statement, the only rational remark that can be formulated is that, yes, some “minority opinions” have often shaped the progress (in the sense of “advancement”) of science, but history also teaches us that a much larger number of such opinions have been proven wrong in the past and have rapidly been superseded by the factual reality. Scientific consensus is—thankfully—much more difficult to “buy” than political consensus.

Please note that I am not taking sides with the Fthenakis et al. counter-paper, which also contains statements with which I disagree, but readers will easily convince themselves



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that while the latter counter-paper is credible from a methodological point of view (for example, it presents arguments that can be “falsified” if one finds solid and accepted counterexamples), the S&R paper is not. Thus the whole issue boils down to a problem—yes—of scientific method. You may like the idea that there is sufficient eXergy (not eNergy! But that is another story . . .) influx into our planet to ensure our near-sustainable survival as a species for millennia, or you may think that we are on the verge of a novel apocalypse . . . but it is one thing “to believe” and another to “prove one’s thesis”.

In the merit, S&R have one single concept they repeatedly cling to: “overshoot”. The senior author (WER) is well known for introducing the concept of an “Ecological Footprint” in the 1990s (EF in the following) [Rees and Wackernagel 1994], which is a quantitative measure of the “environmental services” required of the Earth by our human society. Since the EF is expressed in units of “equivalent km²”, it is apparent that as the global EF exceeds some properly averaged measure of the planet’s surface, we are bound to consume fossil resources to make up for the difference. “Overshoot” means, therefore, that if our level of material and energy use exceeds the Earth’s capacity and we continue to survive on non-renewables, sooner or later we are going to run out of primary resources. By the way, this is not a new idea, and the qualitative Hubbert’s curve displayed in Figure 1 in the S&R rebuttal to Fthenakis et al. can be found in several other previous sources, in some of which it is also quantified (and criticized).

The simple truth is that the EF is just another environmental indicator, and it has been, as many others, often used to measure our “degree of sustainability”. It has come under some critique, and to the best of my knowledge, the number of publications referring to it is decreasing. However, it IS INDEED a legitimate indicator, and if scientists wish to adopt it and draw conclusions based on its application to certain scenarios, they are absolutely free to do so.

What is unacceptable in the S&R paper is not their insistence on the overshoot but the fact that they do not consider any other facet of the much more complex issue of enacting a transition to “sustainable” development: they simply insist on the need for a “prosperous way down” a’ la Odum, and even suggest to set a limit to the world population so as to avoid overshoot . . . an unfortunate echo of Malthusianism that is surely not even conceivable today.

In conclusion, while we again offer apologies to our readers for our original mistake in failing to require major changes to the first S&R manuscript, we also invite them to consult the attached documents and build their own opinion. While we shall not publish any other report on this matter, and consider the case closed, we urge the involved authors, our readers, and the energy community at large (including myself!) to learn from this case: opinions are OK, but if they are crystallized in an archival paper, they must be substantiated by “facts” (i.e., either well-documented experimental evidence or well-accepted scientific references). As a side note, this is, by the way, exactly what Popper, Kuhn, and Lakatos—each one in his own way—strived to structure and formalize in their critique of the “scientific method”. To the best of my knowledge, neither one of these great philosophers of science ever responded to critiques by saying “critics have not read my paper”.

Hope you will enjoy reading all the attached documents about this case!

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Conflicts of Interest: There is no conflict of interest.