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Editorial

What Makes an Excellent Journal Paper?

In the Volume 24, 2018 - Issue 7 (August 2018) issue of the journal, Associate Editor Jeffrey Spitler asked the question *why was my manuscript declined?* and listed some common reasons for these rejections. Excellent journal papers are world-leading in terms of their rigor, originality and significance. As Associate Editors, we lean heavily on these three terms when assessing the merits of a submitted manuscript. So, what *does* make an excellent journal paper?

Firstly, let's consider the 'world leading' aspiration in the definition above. 'World-leading' is distinct from work that is 'internationally excellent'. Manuscripts that are 'world-leading' are setting international agendas, changing the way we think about things and are in poll position to inform standards and policy. Of course, as Associate Editors, we also welcome work that is internationally excellent, that is, work that international reviewers would consider excellent in terms of rigor, originality and significance. Let's take a closer look at these terms and how we can use them to write high quality papers with the best possible chance of acceptance. Rather than doing this in a prescriptive way, which is difficult to do across different topic areas, it is more helpful to pose questions for authors to consider *before* and *during* the writing of a manuscript. Prior to submission, authors should also consider seeking an internal peer review by someone using the same three criteria.

***Rigor*:** Is there evidence that quality assurance has been applied that demonstrates to the reviewer that the results are reliable and methods are robust? For example, in a paper employing building simulation, is there evidence of an error analysis and validation? What are the limitations of the assumptions and the input data? You might even take this further and consider the sensitivity of the results to the model inputs? When undertaking experimental work, has the sensitivity and calibration of equipment been taken into consideration? Matters of rigor all contribute to increasing the confidence a reviewer has in the work being reported.

Originality: Contribution to knowledge has a broad interpretation. Ask yourselves, where does the originality in our manuscript lie? Have we developed a new design technique? Have we proved a new research method or experimental technique, or developed a new database that is going to be useful to others? Perhaps you have tackled a topic never considered before or tackled it in a multi-disciplinary way? Be overt and explicit by telling us in the abstract *how* your submission is original.

Significance: In determining how significant your work is, consider whether other researchers might be motivated to disprove or reproduce your results, or adapt their research methods as a consequence? Does your work influence or establish agendas nationally or internationally? For example, might your methods impact on recommended modeling, design or management processes currently employed in practice, or lead to changes in policy and regulations? Work that is significant (and visible!) has a very good chance of becoming a point of reference in its field: shouldn't this be the aim for all authors?

And finally, some portions of 'low hanging fruit' that I hope will help anyone who is seeking to publish high quality papers in our journal.

1. **The title**: beware of using titles such as, *'A case study of ... '*. This puts off the reader immediately suggesting the paper is little more than an interesting account of a short desk-top study, with no new knowledge. Ensure the title is distinct from your previous outputs and contains all the key words necessary to make it as visible as possible, thus increasing its chance of being read.
2. **The abstract:** tell us up front what makes your manuscript excellent. What is the contribution to knowledge and what makes it world leading? Which international agendas is it influencing (note the use of the word 'influencing' rather than 'addressing')? Does the abstract articulate clearly what is original about the work - please tell us - don't let us guess!
3. **Avoiding 'elephant traps':** there are certain features and omissions in manuscripts that immediately lower the reviewer's expectations and potentially the quality of the paper. Examples include: descriptive rather than analytical work; modelling work that is unvalidated or lacks details on essential input parameters and assumptions; and measurement work in which sensors are uncalibrated. Try also to avoid reporting partial work that claims to be a 'pilot study', 'initial study', or 'part 1, part 2, etc.' This instantly suggests you have chosen to publish a smaller, less rigorous piece of work.

We look forward to receiving your excellent manuscripts for review.

Many of the ideas in this editorial stem from discussions with colleagues at Loughborough University. In particular I would like to acknowledge the support and wisdom provided by Professor Elizabeth Stokoe, Professor Kevin Lomas and Dr Elizabeth Gadd.

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