Black text: original comments of the associate editor and reviewers

Green: comments by SK in after the manuscript was published elsewhere

Summary

This document highlights the difficulties in competent in-depth review of the considered manuscript. It shows that referee1 and referee2 in a reputable journal (JFM) did not read the manuscript carefully, and made little effort to understand it.

According to JFM experts this study is

- overdocumented,
- contains no new physics (btw, what physics is considered as “new” or “old”?),
- nothing new “…in modeling or even in new experimental technique or design) on the subject (rather, the points of discussion are technical)…”

In other words, this work is trivial, and therefore its conclusions are trivial, too.

Summarizing the points unvelivable1, unvelivable2, unvelivable3, unvelivable4, what is the role of an associate editor here?

14-Jul-2022

Dear Dr. Khirevich,

JFM-22-0208.R1

Correct estimation of permeability using experiment and simulation

Khirevich, Siarhei; Yutkin, Maxim; Patzek, Tadeusz

I am sorry to have to inform you that your paper is not suitable for publication in the Journal of Fluid Mechanics. Rather than repeat the rationale described in my letter following the first submission for providing you and your colleagues with the benefit of the doubt, I simply refer to that correspondence and the comments from the reviewers that are given at the bottom of this letter.

For the avoidance of doubt, this assessment is made on behalf of the whole Journal. A resubmission of substantially the same paper to a different editor will not be considered. Any subsequent submission to JFM containing original material from this manuscript must mention this manuscript's JFM number and should be assigned initially to the associate editor who handled this rejected submission, even if the subsequent submission is of a different category (for example if a rejected Rapids submission is extended to the length of a Standard submission).

Yours sincerely,
Prof. _____
Journal of Fluid Mechanics

Reviewer(s)' Comments to Author:

Referee: 1

Comments to the Author

This is my second review of the manuscript and I appreciate the authors' efforts to improve the manuscript especially regarding the new literature added to the introduction. I also understand the difficulty to turn this massive campaign into a coherent, comprehensive article. I was happy to see that the authors have followed most of my advice to improve the manuscript but three major concerns still stand:

1) I think there is a strong discrepancy in the way the authors try to sell their key take home message and the actual problem that was under investigation. The authors claim that most of the measurements of permeability are flawed. I still think that such a message is not backed up by the analysis as it may lead the readership to conclude that this addresses all types of porosity measurements. Instead, the authors focused on a specific type of flow cell with certain dimensions in terms of tube diameters and length scales and a granular medium comprised of spherical beads. It is only for this specific flow setup that reliable conclusions can be drawn. The rest is subject to speculation. The authors should make this focus of their work absolutely clear in their title, abstract, literature review and conclusion. <- The simulation setup is based on the Stokes equation. Period. Creeping, viscous, slow, etc. flow of a fluid between static obstacles. The study was *designed* in a way that the positioning and shape of the obstacles is of minimal importance. We used packed beads as a sample in such a way that conclusions are not limited to that particular sample type. *As a proof*, we took some random sandstone sample and observed similar picture, as shown in Figure 7. **Unbelievable1.**

2) What struck me as somewhat frustrating is that the authors investigated their specific flow setup in full detail but they do not provide quantitative measures for optimal flow cell design. For example, the newly added section "The sharp pressure drop…" on page 10 states that there is significant energy loss if D_tube is small compared to D_smpl, what do the authors deem to be significant and how small is small when it comes to the ratio of D_tube to D_smpl? Same holds for "Introducing gaps…" on page 12: no quantitative analysis is presented where readers can learn on optimal flow cell design. <- Figure 6b. How big does the cell have to be in terms of bead size to sample width? Can the authors quantify the effect of the walls? <- Figure 6a… diamonds vs. squares vs. circles vs. dots… **Unbelievable2.** How big does the flow cell have to be to provide a representative measurement of the permeability for just the porous medium? <- Figure 6b. In this regard, I do not understand what the authors mean by REV-related ambiguity in their response to my concern 1.3.1? <- Typically, experiments are performed on a larger sample compared to the simulation domain used for pore-scale simulations. In this case one can ask about representativeness of the chosen simulation domain size. But this is not the case in our work: simulations and experiments were *designed* on the same geometry, and therefore REV question simply does not exist here. Moreover, the considered geometry already includes representative volume (about 3–5 diameters^3), as the agreement with other studies in Fig. 3a shows (we have no idea about dimensions of packed bead samples of others, but it appears that all of them include representative volume, subject to a possible impact of confining wall). Should it not be the prime goal of every permeability measurement to provide a macroscopic parameter that describes the granular medium on a continuum scale? <- Yes, it is the goal: in the case of a large homogeneous isotropic sample it is sufficient to estimate (e.g., simulate or measure) permeability of its smaller representative volume, REV, and this REV permeability will quantify the flow through this whole large sample. This is the key idea in permeability measurements in many fields. If the REV permeability is flawed, the quantitative conclusions about permeability of the large sample are flawed too. Actually, it is regrettably to find out that discussion occurs on such a level.

3) Finally, it seems that I was not the only one among the referees and the editors who got lost in the extensive description of the measurements. <- very "wise" assoc. editor's decision to share reviews among referees, making their opinions even more independent. To be honest, there were a few cross-references between replies to each referee, but in this case the assoc. editor

could take some effort and provide only the corresponding part. I understand the authors' reasoning to have one comprehensive article, but sometimes, splitting the manuscript into two companion papers is a better decision. Here, this clearly seems to be the case for me. There is one huge part on experimental techniques that can be best published in a journal on experiments in fluids, and another part, where measurement results and simulations are combined to criticize previous efforts to determine permeability in flow cells with limited spatial extensions. Both parts could stand on its own and make cross-references to one another when needed. The authors should definitely consider this option as a valid alternative option to their current publication strategy.

As a minor remark, the authors explain the validation of their numerical code adequately in their response1.5, but I think it should also be mentioned in the manuscript to establish trust in the simulation results.

Referee: 2

Comments to the Author

First, I would like to thanks the authors for their effort to answer my questions and giving me many references. Unfortunaletly, I must say that my opinion has not really changed.

The major problem is that there is no new physical phenomenon in this work. <- This is the main strategy of this study: to simplify the considered system as much as possible (in terms of "physics" whatever this means) without removing essential details; what details are essential is the major question. This strategy makes it possible to concentrate solely on the quantitative aspects and to reveal the full power of computer models. By the way, this is the only way to go if one wants to *predict* reality with the models but not endlessly *speculate* about it with the same models.

Pressure loss in the tubes is something that is far from unexpected. This effect is, for example, "mistake number 4" of Chapuis (2012). <- A clever strategy: to counter-using the citation I found and provided without any wish for critical analysis :), also simply ignoring another provided and discussed links. And, by the way, pressure loss not in tubes but at the tube–sample contact (see below).

This work essentially demonstrates that the solution of Stokes equation in the full geometry is in agreement with the experimental measurement. It is nice to have one more confirmation, but is it so surprising? <- just one more among many others :) I provide citations for my statements (including "flaw affecting many experimental measurements", "ten-fold differences" below), and it would be nice to see similar citations for such reviewer's statements; they should be easy to find if there are *so many* confirmations where Stokes simulation in porous media, including sharp discontinuity, works exactly as experiment.

The other problem is that I am still very uncomfortable with the main statement in this manuscript. I understand that measuring permeability is difficult and can lead to many uncertainties and scattered data. <- I would say not "can lead" but, according to the literature, "always leads for some reason(s)" :) But this is not my problem.

The problem is the claim that so many experimenters have sparse data and errors essentially because they forget the pressure drop in the tubes ("a major flaw affecting many experimental measurements"). <- Figure 5C… not in tubes, but at the tube-sample contact; this is repeated so many times in the manuscript, and is already stated in the abstract. **Unbelievable3** Of course, one can find some mistakes in some work, we all do. But the claim that so many "mature" researchers have made this mistake is a very strong statement. I find also this claim rather condescending.

In the end, I think the main merit of this work is that the authors have achieved a careful experimental and numerical measurement for this type of porous media. This is undeniable, but the fact remains that there is no new physical phenomenon. I therefore still think that this manuscript is more suitable for a journal more dedicated to experimental methods such as "Experiments in Fluids" or others.

Before doing so, I would also recommend to remove any value judgments towards other researchers and communities. For example:

"a major flaw affecting many experimental measurements" <- there is a direct reference to the permeability measurement standard in the manuscript **Unbelievable4**

" it is difficult to assess its value accurately, and ten-fold differences, real or imaginary, seem to be acceptable in some fields", etc. <- as stated, I provide links, also refer to the figure numbers in the first reply

Referee: 3

In the revised version of their manuscript, authors have tentatively answered my comments, most of the time opposing rebuttal. I strongly believe that this paper is over-documented even if authors deny using sometimes an (inappropriate) mocking tone to answer this issue (arguing that more details are requested through the questions raised while less details are required on the whole). <- there was strictly no mocking tone implied; moreover, review3 was head and shoulders above all other reviews from the point of depth of referee's questions. Especially, questions 3.9, 3.12, partially 3.10 and 3.7. While I was sure that concerns raised in, for example, 3.9 and 3.7, do not take place, I still need to find arguments to support my statement. This sparks a discussion that may lead to a possible manuscript improvement. I insist: some unimportant material could be advantageously discarded for a scientific paper. <- if this point is so critical, referee3 could specify exactly what parts have to be removed: beads 2d scanning, density estimation, viscosity measurements, stabilization of perm. measurements, scanning/image processing, flow simulations…? In order to keep the manuscript "suitable" for JFM. Moreover, it is ironic to see the following statements in an occasionally found JFM manuscript (doi:10.1017/S0022112010003113):

> **5. Conclusions**
>
> The canonical TBL under ZPG has been a research topic for a number of decades; still fundamental open questions exist related to, e.g. scaling and shape of the mean and fluctuation profiles. In the experimental community, it has been realized that the exact documentation of an experiment is crucial to provide accurate, reliable and reproducible results; both related to the physical set-up of the experiment (tunnel geometry, tripping, etc.) and the applied measurement technique (temporal and spatial resolution, correction methods for Pitot tubes, etc.).

I would have expected that authors could have considered this comments in a constructive manner, but it seems impossible for them to operate any kind of information contraction as they simply deny any suggestion in that direction. Rather, they provide convoluted and lengthy answers (making them sometimes very difficult to follow; e.g. answers to my questions 3.8 to 3.11). <- well, I was considering the answers as detailed; however, according to referee3 they are convoluted, difficult to follow, and tentative. Their answers spread over 21 pages!! (and 47 pages including all reviewers remarks!). Apart from absolutely unusual, a 12 pages long paper complemented by 37 pages of appendices seriously questions the pertinence of a publication in a scientific journal like the Journal of Fluid Mechanics. <- According to my knowledge, there is no space limitation in *of Fluid Mechanics, Journal*.

Again, I acknowledge the accurate investigation carried out in this work and the important and careful inspection of a lot of aspects that can be the source of errors in the evaluation of the pressure drop used to determine the permeability of a sample. I think it is a valuable work that undoubtedly deserves to be reported in a way or another. Nevertheless, taking into account the fact that i) authors insist that the "presented details are already a shortened version" and ii) there is nothing new in terms of the physics (nor in modeling or even in new experimental technique or design) on the subject (rather, the points of discussion are technical), I would suggest an alternative way of publishing this work. Maybe, a monograph would be more appropriate, in which all the extensive details that represent side information, could be provided, along with clear recommendations. Alternatively, a journal more specifically dedicated to technical aspects of porous media characterization (e.g., Transport in Porous Media or Journal of Porous Media), would be most probably more appropriate and the paper would certainly get more audience than in the Journal of Fluid Mechanics. <- This is just unbelievable: in this and my previous studies I refer to the extremely identical studied systems (and do not want to refer to them here to avoid involving other names into this story) published in JFM.