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## **Comment on wes-2021-129**

Tomas Sabaliauskas (Referee)

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Referee comment on "A model test study on the parameters affecting the cyclic lateral response of monopile foundations for offshore wind turbines embedded in non-cohesive soils" by Dennis Frick and Martin Achmus, Wind Energ. Sci. Discuss., <https://doi.org/10.5194/wes-2021-129-RC1>, 2022

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A very exciting topic, touching very important questions. At the same time not digging deep, not raising questions that would be radical. Instead, staying within a small box, a small niche of a small paradigm - discussing importance of one parameter between a few very similar, simple "models" that plot curves on top of data. Nothing extremely significant stated, but the careful points made are very well written, presented. Easy to follow. Yet... nothing "shocking" proposed or discovered.

The discussion shows various models compared. All the models are built using almost identical parameter names, assuming nearly identical theory / explanation. They all assume knowing the load and initial density - is enough, and nothing more will ever be needed. Completely ignoring the elephant in the room - non of these "solutions" truly solve irregular loaded structure behavior.

None of them quantify disturbed soil states. None of them show how to "disturb a foundation back to initial state". Real structures go through millions of irregular loading cycles, and we have to start modelling this with good precision some day (sooner preferably). Instead, literature tends to be stuck discussing "one parameter" in some few methods none of which come even close to the "real" solution. Topic limited to "which method is the least bad - out of all the not good enough".

This topic is ripe for radical, provocative, alternative / opposite, new and unseen, different tests, findings, models and topics... it is simply screaming for it. But students are taught to be nice, polite and not question absurd limitations imposed by convention. Not to take risks. Be calm, polite, simple.. be nice, agree and stay silent - remain not noticed. Confirming, submissive... Do not question superiors. Do not provoke.

Repeat the same tests. Even if same curve is hard to produce twice - fake confidence in the model, say it is good and predicting well. Never do tests - others had not done before you. Copy others. Follow others... stay behind - follow. This paper show that quite strongly. It is too gentle. Proud and protective of methods that do not work well (even if no one has better ones yet). I'd like a mention of the big problem - none of these are "enough". Something "big" is not there - in them all.

I wish the elephant in the room to be mentioned, exposed and attacked very aggressively. Disturbed soil, irregular loading cycles - are neither tested or discussed in the paradigm. All "research" is stuck repeating the same "constant amplitude" loads - and then arguing how to fit such basic case. With each case either re-calibrating the model parameters, or changing position of some parameter. Each test - new model, new calibration. Rather than looking for "common pattern" in the "big picture". Rather than raising the question of "how does the physical system work?", "what govern the physical properties, fundamentally?", "how to control the shape, size and position of stiffness hysteresis loop"... instead, the discussion is "how to compliment experts", "who to like", "who to side with". I see no sides that are "right" yet. The "world best" is not good enough - yet.

Paper does mention conflict. Density having lower influence than expected. Some cyclic accumulation coefficient not having influence in some cases. But that does not address the big problem - irregular cycles. Controlling / predicting shape, size, position of individual hysteresis loop...

I was expecting more, after reading the title. I was expecting either GPU (parallel computing) compatible models or test results bridging across various testing cases. Tests that explore irregular cyclic loading - combining drained and undrained sequences. Examples disturbing a foundation back to initial state, controlling the shape size and position of individual stiffness hysteresis loops - this had been achieved, had been published. It is plausible to demonstrate and observe very easily. Although, not very popular. Not done by famous, high cited "experts".

The experts tend to build larger piles and repeat the same test to reach the same conclusions - by calibrating the same models with the same coefficients... then publish the same books, with the same text but a few extra pictures of a larger pile. Despite it's apparent aim to expose contradictions - this paper still fits well in that path.

My main criticism for this paper is - it takes no risks. It says nothing new. It talks about other people and others work. Trying to compliment and carefully "fit in with the others". Too careful for my taste. There is nothing provocative or exciting discovered, observed, assumed, tested, etc... no one insulted. No theory "crushed" by the "extreme new empirical observations". Above all it shows a lot of books had been read, and the "important names" are known. No grammar errors made - due to author being excited about the topic "too much". Good for a luxury diner with a stale conversation - less good for real / meaningful scientific progress.

Thus, I strongly recommend to publish this work. It is very good. It is just like the rest of the publications. Does not stand out in any significant way. Does not excite. Feels dry. Simple. Perfect for a safe "yes man" career within academia / teaching / repeating. Fit for a young pensionist - saving for pension in 20's. It contradicts nothing and insults no one, by proposing no "really new" ideas. With perfect grammar, and fluent writing. Totally by the book, following all rules precisely. It is a perfect paper by academic standards. Good for teaching, bad for science.