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Reply on RC1

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Author comment on "Public acceptance of wind energy – concepts, empirical drivers and some open questions" by Michael Ruddat, Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2021-118-AC1, 2022

Thank you very much for the detailed response. It will definitely help to improve the quality of the article. First of all, there is a lot of critics about chapter 2 concerning theoretical concepts of acceptance. Actually, this chapter wasn't intended to tell something about the "true nature" of acceptance. It was simply intended to give a very short overview about known concepts that are used in the context I do research in the field. I tried to connect these concepts analytically, but according to revier#1 I failed doing so. For example, socio-political acceptance definitely encompasses more than just public attitudes towards a technology and is not an object. Institutional elements (see below) play an important role, too. Fortunately, these connections are not really necessary here and I will now simply list the concepts as an overview before my own definition used in the article (and omit the "theoretical", which has perhaps invoked too high expectations).

Indeed, I have a mainly sociopsychological perspective and my focus is on public (or individual) acceptance of wind energy, wind turbines and wind farms. There are many more perspectives as reviewer#1 rightly points out. But this would be on the one hand too much material for the article and on the other hand isn't really in my expertise. So, public acceptance is indeed only a part of the puzzle and I will certainly acknowledge that in the second version of the article. The concepts I briefly described fit to the empirical data used. Of course, this data is also limited. So, a suggestion could be to do more studies in a comprehensive framework consisting of institutional context (financial system, support programs, legislation etc.) and culture. This would be a promising approach for the future and I will add this to the discussion section. I certainly make no normative judgement about the role of public/individual acceptance on the successful realization of wind park projects that goes beyond the statement that it has some effect. I do not state how big this effect is (and I will revise the criticized sentence in line 30 to make it more suitable to that position). Certainly, there are many impact factors and public/individual acceptance is just one of them. I wonder about the critics on the term "social acceptance" since I do not use it in the article. I just speak of acceptance in general and then of acceptance by the public or parts of it (e. g. residents of wind parks, stakeholders, so individuals, see endnote I).

To make it clear, in this article, acceptance refers to the objects of wind energy in general and especially to wind energy projects (wind parks, wind farms, wind turbines) and the subjects of the public or parts of it (e. g. individuals like residents of wind parks,

stakeholders). These are elements used in the empirical data available to me at the point of writing the article. Maybe I made a mistake at the beginning of the final paragraph on chapter 2 since this could imply some sort of "true nature" of acceptance. This is certainly not the case. The definition proposed at the end of the chapter is just the one suitable in my view for the following analyses. Since a lot of studies build on independent and dependant variables using multivariate modelling approaches the focus on individual attitudes and (self-reported) behaviour in the definition of acceptance lies near. Because reviewer#2 also had some problems with the definition I now expanded it to encompass possibly all types of attitudes and non-attitude:

"Acceptance means a positive evaluation of a topic (like wind energy, wind turbines or wind parks) by individuals under certain circumstances (e. g. cultural or institutional context) that can have consequences for individual behaviour. Correspondingly, non-acceptance means a negative evaluation of a topic by individuals under certain circumstances that can have consequences for individual behaviour. If there is no clear positive or negative attitude towards the topic (e.g. ambivalence, non-attitude), we speak of tolerance that can have consequences for individual behaviour (but perhaps not so much as the endpoints of the continuum)."

Although I do not investigate the cultural or institutional context in much detail (because it is not the focus of the article), it is of course present (at least in a theoretical perspective) and should be part of the definition. Additionally, the empirical studies come from many different countries and so the institutional and cultural context is indeed varying. This definition implies two dimensions (attitudes and behaviour) and because of that, different forms of measurement. This is meant with the quote in line 338 ("The theoretical concept of acceptance is complex as well as multidimensional. It encompasses attitudinal and behavioural elements and can be measured in many different ways"). These elements are also part of the definitions of Upham et al. and Schweizer-Ries et al. and makes them at least two-dimensional in my view. In addition, Upham et al. incorporate "socio-technical system" as a possible acceptance object and in this way acknowledge the social dimension of technology.

I absolutely agree with the reviewer that acceptance is dynamic. Actually, this dynamic was shown in my presentation on the WES 2021 with respect to the different concepts of acceptance (analyses, feelings, behaviour). Of course, the weighting of benefits and risk is a process as is the formation of feelings towards a new technology or the way to the decision about protest or support of a local project, respectively. But at the end, there stands a judgement and this judgement can be measured by survey techniques (with all the methodological advantages and disadvantages, of course). So, it is advisable to look at acceptance at different points in time and this is done in the article by referring to data from a longer time span. Important to say, longitudinal research is of high value with respect to the dynamic nature of acceptance. Because of that, the case of the U-shaped form of acceptance (see line 180ff.) is of special interest and mentioned in the discussion. By the way, at least in the German case, public acceptance of wind energy is relatively stable over the past years, so although acceptance can be dynamic this doesn't mean it actually has to be. It is an empirical question.

The sentence "The further development of wind energy is of major importance for the success of the energy system transformation in Germany and elsewhere" (line 21-22) in the introduction induced some critics by both reviewer #1 and reviewer #2. What is meant is that wind energy is from a technical perspective the most promising and cost-efficient form of green energy today (at least this is what I have learned in interdisciplinary projects about the German energy transformation). Other technologies like solar power or biomass have more disadvantages. Of course, I could outline this using the respective literature but it would take some time to do it properly and (even more problematic) it would take too much space in the article. The focus is on the empirical

drivers and I do not want make shortages there. So, I would prefer simply to add the technical perspective (since the social one is represented by the public acceptance) to the sentence: "From a technical perspective, the further development of wind energy is of major importance for the success of the energy system transformation in Germany and elsewhere".

There seems to be a problem with the term "Utility-scale wind" in line 267. This is a quota from the Boudet-Article. It does not mean introducing a new concept or something like that. I understood it as representing great wind parks and their benefits and risks. And this is what chapter 3.4 is all about.

The question of ownership/initiator/operator is certainly of high relevance. I incorporated it in the chapter about trust as well as participation because there are strong empirical relationships (see the cited literature in lines 251f and 316ff) and also for brevity. I am not sure if it really needs an own chapter because there would be some redundancy and it would make the article longer (and by the way it is long enough in my view thinking of the reader). Please let me know how you think about that.

Concerning the remark on noise influence I will try to incorporate the findings of Eja Pedersen. I have a question here: If I understand you right, the visual assessment has some effect on noise annoyance. So, noise is only important if the object can be seen. Is that right? If so, I think it would be in accordance with the results of Pedersen and Persson Waye (2004). Concerning the importance of the subjective assessment of the visual impact of the landscape change, that's what I mean in line 126.