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Comment on wes-2022-70

Anonymous Referee #1

Referee comment on "Bayesian method for estimating Weibull parameters for wind resource assessment in the tropical region: a comparison between two-parameter and three-parameter Weibull distributions" by M. Golam Mostafa Khan and M. Rafiuddin Ahmed, Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2022-70-RC1, 2022

GENERAL COMMENTS

This paper examines a genuine issue in the science of wind energy and is thus in scope for this journal (Wind Energy Science). There are certainly places with significant periods of calm (wind speed < 1m/s) but in which wind power may nevertheless be an option worth considering. I know of some such sites in Fiji and elsewhere in the Pacific Islands.

There is something of a cottage industry in fitting [2-parameter] Weibull distributions to measured wind speed at particular sites around the world, with at least 400 papers published since 2006 with such a subject, according to the Scopus database. But, as the authors point out, a standard 2-parameter Weibull distribution of wind speed necessarily does not include any calm periods, whereas a 3-parameter distribution can do so.

A major aim of this paper is to examine the extent to which this more complex model can improve estimates of wind energy potential. Related to this aim is the most original part of the paper, namely to examine whether a new Bayesian method of estimating the parameters of a 2- or (particularly) a 3- parameter fitted distribution is good for this purpose.

The paper achieves both these aims, and therefore warrants publication in some form. Certainly sufficient detail is presented to enable a reader to make their own appraisal, including as to whether the improvements are worthwhile in terms of practical estimation of wind energy potential, though the authors notably fail to discuss this aspect. Nevertheless, in my opinion, the main text of the paper includes some excessive detail that would benefit from moving into its Supplement or otherwise condensed.

SPECIFIC COMMENTS

Discussion and conclusions.

A significant use of fitted Weibull distributions is to facilitate extrapolation from the measurement site to nearby sites using computer models like WAsP. It is not self-evident that this can also be done with a 3-parameter fitted distribution, though the authors claim in a throw-away line in their conclusions that it can be done. Some such discussion should be included in the Discussion section, including a little on why such fitting can be useful – the more so as it is bad practice to include a substantive point in a Conclusions section that is not raised earlier in the paper. .

Table 8 is a valuable summary of results for average power production. Like Table 7, it supports the authors' qualitative conclusion that Bayesian-3P yields the best results (or nearly so) at all sites examined. MLE-2P represents the fit most widely used in the literature. It is surely worth noting that the Bayesian-3P (the method tested here) yields substantially lower relative error than does MLE-2P only at sites 3 and 7, which are the only sites with >2% of calms. In my opinion , it is very likely that for the other sites the difference of <2% between fitted and actual AEP would be overshadowed by the year-to-year changes in actual AEP, so that the gain for engineering purposes of moving from MLE-2P to Bayesian-3P would barely be worthwhile. These fundamental points also warrant discussion, and perhaps an allusion in the Conclusions and the Abstract.

Literature cited or not cited.

Much of the literature on wind energy makes broad statements about global trends in renewable energy , but perversely supported only by a citation to another narrowly focused paper on wind energy potential at a particular site. The first paragraph of the Introduction to this paper is a typical example. The assertion that many countries are turning to renewable energy is supported only a reference to Mostafacipour (2014), which is primarily yet another Weibull analysis of the wind energy distribution at a particular place; although that paper contains a similar sentence to the one in this paper, it too is supported only by a few references to similar single-site wind energy papers, and is any case rather dated.

Surely a more appropriate basis for such general statements would be recent reports from one or more of the several institutions which specialize in energy statistics and their analysis, such as the International Energy Agency (IEA), the International Renewable Energy Agency (IRENA), the REN21 network with its annual Global Status Reports, The United Nations compilation of World Energy Statistics, or the BP Energy Statistical Review. Such references could also support the first sentence of the same paragraph.

Conversely, there are a few general statements in the Introduction, which, while widely accepted, in an academic paper would normally warrant a reference. For example, that the Pacific Islands are among the most vulnerable places to climate change (line 45) could be readily referenced to the IPCC Sixth Assessment Report (Working Group 2) or similar.

Site selection and measured data

For nearly a decade, the research group led by Professor Ahmed has been measuring the wind speed at sites across the Pacific Islands, using equipment like that described in sec. 2 of this paper. They have published directly measured data, usually for about 12 months, for sites in almost all the places listed in Table 1. The number of observations , helpfully given in Table 3, indicates that these measurements (for most of which no reference is made to the relevant paper) are usually the "actual' data used in this paper. . It is therefore surprising that the present paper draws on satellite data for sites 4 (Kadavu), 5 (Rarotonga), and 6 (Nuku'alofa). Why is this? What is the accuracy of that data, about which no methodological details are given. While the quality of satellite estimates of wind speed has no doubt improved, historically it tended to overestimate actual wind speeds (especially on hilly islands) by some 20% (Kumar & Prasad, Renewable Energy, 2010) .

Of course, the aim of the present paper is primarily mathematical , to examine how well various fitting methods match a set of "actual" wind speed data, rather than an engineering aim to examine the potential power available from the wind. Thus the accuracy of the underlying "actual" data does not matter for this mathematical purpose, particularly whether it systematically over- or under-estimates the wind speeds.

A test of the curve-fitting method for sites with a greater proportion of calms than any of the sites examined so far would be of mathematical interest, even if such sites are unlikely to be of interest for production of electrical power. The most obvious examples in the Pacific for which a reasonable amount of data is available would be airports, which are usually sited in relatively calm sites for safety reasons! . Nadi airport for example has some 50 years of publicly available wind speed data at 3-hour intervals , of which >20% is calms (periods of 10 minutes with average wind speed <0.5 m/s) [FMS (2008), Surface winds at Nadi airport, Information Sheet No.38, , Nadi :Fiji Meteorological Service.].

It is important for wider use that a tool for using the Bayesian 3-parameter method is available in the widely used statistical software package R , as pointed out in sec.4.2.1.

TECHNICAL COMMENTS

The paper often refers to its sites as being in the "equatorial" region, including in the Abstract and the Introduction. . A more accurate term here would be "tropical", as the term "equatorial" refers more specifically to the zone from about 5degN to 5 degS , which includes the Intertropical Convergence Zone, which is the climatological region of the Doldrums, a calm region historically notorious among operators of sailing ships.

The page length of the paper could be slightly condensed by merging the multiple similar figures into one or two appropriately labelled figures, each incorporating 4 or 6 of the charts. This would save repeating the information in the captions and legends. Incidentally, several of the charts appear to have only one curve plotted from among the 4 identified in the legend. If this is becaasue the curves co-incide (to visual accuracy) then the text should say so.

Among references cited, for which better sources are available include Mohanty (2012) [which could be more appropriately replaced by the SPC's Framework for Energy Security and Resilience in the Pacific (FESRIP) 2021-2030 and/or its issues papers, or even Weir ("Renewable Energy in the Pacific Islands: its role and status, Renewable and Sustainable Energy Reviews, 2018) or Michalena et al ("Challenges for Pacific small island states", Energy Policy 2018) , which are slightly more recent than Mohanty's paper]. Also Kidmo (2015) is focused on a far inland region of Cameroon and says nothing about developing country islands, notwithstanding line 53.

The citations for MOE (2017) and PPA (2015) are appropriate, but the corresponding entries in the List of References are inadequate, since neither spells out the institution concerned, namely (I guess) Ministry of Energy (Suva, Fiji) and Pacific Power Association , respectively