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Geoscientific Model Development Discussions

Interactive comment on "TITAM (v1.0): Time Independent Tracking Algorithm for Medicanes" *by* Enrique Pravia-Sarabia et al.

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TITAM (v1.0): Time Independent Tracking Algorithm for Medicanes. Reply to RC2.

September 15, 2020

Referee comments: in blue.

Author responses: in black.

We welcome the feedback and appreciate all the referee suggestions and comments. While we do not agree with his/her general message , i.e. that he/she does not favour the publication of a purely algorithmic contribution, we agree that there is room for an improvement in the literature review regarding tropical cyclone tracking algorithms, and thus we have accordingly tried to adapt the manuscript to improve this caveat.

Related to the general comments:

The greatest contribution of this article are the criteria it uses to distinguish medicanes from typical extratropical cyclones. The use of the "cyclonic potential," and its motivation via connection to quasi-geostrophic theory is (to my knowledge) novel and a strength of the article.

We thank the reviewer for this comment.

These criteria are actually independent of the algorithm and software employed by the

authors which, as the title suggests, is a main focus of the article.

Even though the criteria are also valid for time-dependent methodologies, the main advantage is gained when used for time-independent, since it rapidly isolates the location of the medicane and, thus, it is not necessary to develop a method for searching the next center in the surroundings of the previous one.

Unfortunately, the software and algorithms advocated by the article are not new. Indeed, the algorithm is the same as the commonly used 2-step search that is described in greater detail by Bosler et.al. (2016), Ullrich Zarzycki (2017), Wernli Schweirtz (2006), and Zhao et. al. (2009), and each of these references succeed previous implementations of the same basic algorithm (e.g., Blender et.al. (1997), Hodges (1994), and Vitart et.al (1997) that are themselves successors of even earlier work. Even this list of references is, therefore, far from complete, and more specialized studies using this algorithm that are also relevant to this work include Hanley Caballero (2012), which (like the present work) addresses multiple circulations within the same larger system. None of the references in this review are cited by the current article, which is a significant omission, as it is not clear how (if at all) the present work distinguishes itself from them.

Perhaps this is a misunderstanding. Our algorithm is not exactly as those reviewed by the referee. Although undoubtedly our method takes ideas from previous ones, as we acknowledge in the literature review we provide (which of course can be and it is indeed improved in the new version), it tries to be more general and flexible. First, it is still based on an optimization problem for the search of the cyclone centre, but we introduce an additional level of complexity by allowing the possibility that the cyclone core is not necessarily the solution to the mathematical optimization point, but another one close to it that satisfies other physical conditions: optimizes a new function covering cyclonic potential, symmetry or warm core character. Thus, although it is not completely new in the fundamentals, we still claim its benefits when applied to the special problem of detecting and tracking medicanes in an automated way. Second, we have cited

all the works on which our method is based. Still, we greatly appreciate the list of additional references provided by the referee and have accordingly tried to improve the manuscript, more specifically the introduction section. Please note the changes associated with these comments in lines 64-94 of the changes version generated with the latexdiff tool.

As a consequence, I cannot recommend this article for publication.

This view is based on the two major concerns expressed below. Of course we do not agree with this recommendation, and we argue why in the response we give below to each detailed comment.

However, I would like to encourage the authors to reexamine their work from the context of their specific search criteria, which appear to be very successful at identifying medicanes. These methods could be applied to a larger dataset to examine the climatology of such storms, as in Zhao et. al (2009); the sensitivity of these climatologies could be examined with respect to different threshold values or criteria choices, as in Horn et. al. (2014). I also commend the authors for employing the cyclone “phase space” model as an analysis tool, and encourage them to continue to use it as this work matures.

Producing and analysing a medicanes climatology is, in fact, an interesting followup of our method, but we believe it is out of scope for this article. Clearly, an algorithm as the one presented here is a powerful tool for different studies. And this is precisely the reason why this method has been prepared for its publication separately. While we consider that the presented examples are enough to show its validity for medicanes, there are still some open questions about its implementation for tracking other types of cyclones. The namelist and the possibility of changing the different parameters provide a useful tool and opens a new discussion about the possibility of developing a single variable method for detection and tracking of different cyclones. Provided the great differences in scale and intensity between the tropical cyclones and the medicanes,

and given that the latter develop in baroclinic areas, the different tropical cyclones algorithms cited by the referee are worth a mention but still not valid for medicanes. Even though the time-independent methodology is not new in the tropical cyclones field, it is not so common in the medicanes tracking, as can be seen in the Table 1 (included upon request of referee 1). Apart from that, we have noticed that, if we are not wrong, no tropical cyclone tracking algorithm considers the center adjacent points as center candidates. This may lead to a misleading track if the found relative minimum does not fulfill the imposed conditions in multiple time steps. It is our understanding that provided the novel approach of the cyclonic potential measure (as the referee points out in the comments), along with the namelist-oriented implementation and the fact that some physical keys are thoroughly studied, such as the existence of a tilting and its relation with the slp minimum-warm core center deviation, or the consequences of using the slp minimum as the medicane center by using the Hart phase space, the manuscript contains sufficient information to be complete and valid on its own. Thus, the application to a larger dataset is beyond the scope of this contribution, although certainly it deserves being the main part of follow up studies. We are indeed working on applications aimed at improving medicanes process-understanding that we hope to send to revision within the next months. The objective of this contribution however is to follow the logic behind the development of the algorithm, while presenting, discussing and validating it, which in our view is very well aligned with the scope of a journal like GMD.

The focus in this ongoing work should not be on software. While writing software is undoubtedly where the authors spend much of their time and effort, this is the nature of modern science. The algorithms and code accompanying this article is not novel; it is simply one of many software packages that have been developed for similar applications in recent (and not-so recent) years. Instead, the application the authors have chosen is an excellent topic for additional study, particularly as resolution (both model and data set) increases to the point that medicanes are well-resolved.

We feel that this point of view is what has influenced the referee overall recommendation of rejecting our article. And we clearly do not agree. We consider that the focus we have given to the paper, aimed at describing the algorithm in a practical manner by means of examples and with parameter explanations and discussions, is best suited for a publication in a journal such as GMD, which is “dedicated to the publication and public discussion of the description, development, and evaluation of numerical models of the Earth system and its components”. Our method, albeit not a completely new approach, is complex enough as to need a detailed description of its components, and that is why we have considered GMD as the appropriate journal to present it. We wish to re-emphasize here the fact that, contrary to the majority of former models, ours is open software, released and made easily accessible to the scientific community and prepared for an easy deployment, including libraries installation instructions. We think that accompanying the code with a GMD publication in which we describe the components and unravel the ideas that lie behind the algorithm is the best way to enable the development of new models, hence the perspective provided to the manuscript. Perhaps we have misunderstood the scope of this journal and its purpose, and hence we call for the Editor to help us to solve these two confronted viewpoints.

Now we proceed with the responses to the specific comments.

1. Figure 2(a): The legend label (SLP) is incorrect; the field shown is âĹĞ2(SLP).

Thanks for noticing.

2. Line 298 reports that detected storms are shown in Figure 2(c). These are not visible in my .pdf copy. Also, it appears that there are many (56?) such detections âĂŤ why? How does this number correspond to the number of time steps in the data set?

Thanks for pointing that out, they are in fact missing in the first version of the manuscript. Please find them in the new version. Also, provided the time-independent methodology, the number of points detected as candidates in each time step is not related in any way to the number of time steps. In fact, these 56 points are the ones in the

99.9 percentile (in a simulation with 200 x 280 grid points). This percentile, a namelist parameter, should be lowered in the presence of large cyclonic structures that could dominate the cyclonic potential field and eclipse other structures to be detected.

3. Line 305: What is gained by not using the SLP minimum as the location of the storm? In figure 4, the SLP minimum produces a clearly smoother track.

While in Figure 4 slp minimum track might seem smoother, note that the red points indicate that it does not fulfill the Hart conditions, and when analyzed in the Hart phase space, it could lead to wrong conclusions. Figures 3, 4 and 5 showcase the disadvantages of using the SLP minimum as the medicane center. Caption of Figure 4 has been rewritten for the sake of clarity.

4. Line 324: In what sense is the current method more “robust" than one that uses all the same criteria but chooses to define the location of the storm as the SLP minimum? This is one of many unquantifiable comments in the article that are better characterized as “sales" than scientific analysis.

Is more robust in the way that the SLP minimum is the point that is first checked (first candidate) but, as it does not always satisfies the conditions, alternative candidates are selected (please see Figure 4: green points are the timesteps in which the SLP minimum fulfills the criteria, red points the ones where another point must be selected as medicane center). In fact, SLP minimum is the one selected, but only if it is valid to be considered as medicane center. We do not intend to “sell” our approach, but to point out the advantages that can be easily demonstrated through these examples. In the light of the results in Figures 3, 4 and 5, it seems not pretentious to maintain that our method is more reliable than another using the SLP minimum (or any other relative maximum or minimum) alone, particularly if further studies in the Hart phase space are to be performed.

5. For a study intended to identify medicanes, the ability to distinguish a North Atlantic storm seems (as shown in Figure 7) seems irrelevant. A better example would be an

extratropical cyclone, associated with a digging trough, in one part of the Mediterannean basin and a separate system elsewhere in the region that contained a medicane, presuming such a situation can be found or simulated.

The Mediterranean situation the referee describes seems to be technically equivalent to the one shown with the North Atlantic storm. However, the one provided in the manuscript exhibits the additional ability of the proposed tracking method to isolate a medicane in the presence of a much larger and deeper low-pressure system, which was, a priori, prone to show large cyclonic potential values. Please note that in the presence of multiple large cyclonic systems, for example when using the method for global data in simultaneous presence of typhoons and hurricanes, the percentile parameter is probably something to take into account.