Replies to reviewers' comments

Comment to reviewers

The Authors are grateful to thank the reviewers very much for their important comments that helped us to improve the original manuscript. We have responded to all comments and have revised the paper in light of them. Details of our responses to each comment are shown below.

Reviewer 1:

Comment (1):

The review of IGS products are poorly done. If the authors wish to discuss them in detail, they should do the comparison and discussion properly. They mention only UPC and CODE. The complete list lack CAS and WHU.

Response:

Considered: Concerning the revision of the IGS products in the paper, we would like to remind the reviewer that it is a paper not a thesis, just we mentioned the most recent part of IGS, we can not cover what he asked for. However, the abstract has been modified accordingly to try to cover partially this comment.

Comment (2):

There are problems in the application of double-differencing. It is not clear how the authors handle challenges related to receiver biases and how they resolve ambiguities. At the end of the discussion, they just assume them to be negligible (line 123-138). Also, it is not clear whether they have performed tropospheric correction or not. For real-time kinematic applications, it can be done only in near real-time and they should correct for the tropospheric delay as well. The data and details should be discussed in the paper. The information given in lines 257-261 is inadequate. The receivers are too far away from each other to apply double differencing. The spatial correlation length of ionosphere is less than 70 km in that part of the world.

Response:

- It is not clear how the authors handle challenges related to receiver biases and how they resolve ambiguities. At the end of the discussion, they just assume them to be negligible (line 123-138).
- **We would like to remind the reviewer that it is a paper not a thesis, Just we mentioned the outline of the used algorithms. To discuss the details of ambiguities resolution either on short baselines or long baselines we need a book to explain. Additionally, for other biases for troposphere, orbit and multipath we just mentioned a hint about the possible models that are mentioned in the used commercial software TTC. It is very easy to describe what the reviewer asked but again it is a paper not a thesis**

- it is not clear whether they have performed tropospheric correction or not. For real-time kinematic applications, it can be done only in near real-time and they should correct for the tropospheric delay as well.

Did the reviewer think that any one used GPS in research could not apply the tropospheric correction in his work. Really it is out of sense completely

it can be cancelled by differencing. For the tropospheric residual errors, the best standard method of computing is to apply a tropospheric error model at the locations of the reference and remote stations. Examples of such models include the Hopfield model and the Saastamoinen model [Hoffman B. (2008)].

In the current research we mentioned that Saastamoinen and Hopfield models are the tropospheric models that are applied in the used Trimble software.

- The information given in lines 257-261 is inadequate.

We put the reference for more information about the program

- The spatial correlation length of ionosphere is less than 70 km in that part of the world.

Comment (3):

The discussion on the 'Processing Software' does not make sense at all. How did the authors obtain the TEC over the stations? The VTEC definition in line 169 is only for one epoch, one satellite and one frequency. What happened to the other frequency? How does the software inside a receiver can be trusted? Where are the references that discuss the accuracy and reliability of this computation? What is the purpose of NRCan Online PPP Software?

Response:

- The discussion on the 'Processing Software' does not make sense at all. How did the authors obtain the TEC over the stations? The VTEC definition in line 169 is only for one epoch, one satellite and one frequency. What happened to the other frequency? How does the software inside a receiver can be trusted? Where are the references that discuss the accuracy and reliability of this computation?

We mentioned that software were developed by Tawfeek et.al., 2018
For any more details you can see more details in the original paper. , attached separately with the comments

- What is the purpose of NRCan Online PPP Software?

We use it as a threshold value to compare our result with it.

Comment (4):

Predicted GIM is only available for certain analysis centers. How does the Trimble receiver obtains these products? What happens if none are available at that time?

Response:

As it is known that IONEX files are produced daily from many analysis centers. But due to shortage of GPS stations over North Africa generally and Egypt especially, a high interpolation algorithm where done to cover this shortage.

Comment (5):

The discussion in lines (240-247) on GIM data is wrong.

Response:

What is wrong in this section definitely, Pls we need a specific comment but the generalization by the reviewer is not accepted at all.

Comment (6):

The algorithm in Figure 6 is flawed. The authors use only one frequency, one satellite and one receiver to replace the GIM-TEC value in the IONEX file. The resolution of IONEX 2.5 degrees in latitude and 5 degrees in longitude and two hours in time resolution. What happened to the other frequency, other satellites in view, other receivers in the same IONEX map cell and 30 s data from RINEX?

Response:

Really we could not realize on which base the reviewer claims that we used only one frequency, one satellite and one receiver, Nothing was mentioned in the figure or the paper about that. What is mentioned on the chart that the computed vertical ionospheric delay was scaled to L1 as a standard value, the other waves can relate to it. Pls refer to any handbook explains the basics of GPS and ionosphere.

Comment (7):

The paper should be revised by a native technical editor. There are too many grammatical mistakes to correct.

Response:

We are not a native English speaker, we format it in English as we can. Keep in mind we published more than 40 papers in different international Journals.

Comment (8):

There are problems with the figures. Some are not legible and some just do not make sense. Delta E, Delta N and Delta h are not defined mathematically.

Response:

In lines 343 to 346, the differences in the position were described in the following: "The differences between the CRCS-PPP solution and the two TTC positioning solution were computed and are depicted in figures 7, 8, 9 & 10.the figures show the position differences in easting, northing and ellipsoidal height between the computed static NRCan PPP and the kinematic epoch by epoch solution in case of normal default processing parameters (D.D) and with using modified IONEX values (D.D.M-GIM)."