Review of "Correlation between energetic charged precipitation over the South Atlantic Anomaly and L-band Ionospheric Scintillation over South Africa: Investigation of SA's SKA Bid" by B. Opperman and P. Cilliers

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Overall, I believe that this report's primary conclusion, that there is no evidence of increased scintillation <u>at L-band</u> in the general vicinity of the proposed South African SKA site caused by energetic particle precipitation in the South Atlantic Magnetic Anomaly (SAMA), is correct based on the evidence provided in the study and on my own expectations. Despite accepting the study's conclusions, I do have reservations about the approach taken and on how this result might be interpreted for SKA operations, which will be at frequencies as low as 70 MHz (as compared to GPS frequencies in the 1200-1500 MHz range). These reservations fall into three general areas: (1) Data Processing Methodology, (2) Correlation Analysis Methodology, and (3) Application to SKA.

A separate problem at the onset of this study is the lack of a description of the processes by which the authors were expecting the energetic particles precipitating out of the radiation belts and into the upper atmosphere to cause density irregularities in the ionospheric plasma severe enough (in terms of ΔTEC) to cause scintillation at L-band frequencies. At high latitudes, severe scintillation is caused not by the precipitating auroral particles *per se*, but by processes which generate strong irregularities on plasma-density gradients, gradients that are, by-and-large, caused by advection of larger-scale plasma-density "clumps" in the high-latitude convection pattern. There are no such processes at work in the SAMA, so the only possible source of irregularities would be structure imposed on the ionosphere within the SAMA by the precipitating particles. A discussion of the intensity of the precipitation that would be required to produce L-band scintillation, and of the spectrum of the particle population, would be a useful lead-in. This would also likely support an argument that one would not expect to see L-band scintillation in the SAMA region due to particle-precipitation processes at all, based on know precipitation characteristics within the SAMA. There is a general discussion (not altogether correct) of scintillation-causing processes at other latitude regimes, but these are not necessarily germane to the problem under study.

1. Data Processing Methodology

The authors do not make it clear why they chose to use a proxy for the amplitude scintillation index, S_4, when the SKA is going to be sensitive to rapid changes in the Total Electron Content (TEC). What one would want for the study is the statistics of rapid TEC variations (changes on the order of a few seconds), with a correlation of this parameter to indicators of precipitation within the SAMA. It is inferred that the reason for this might be due to the limited sampling rates available, which are typically (for RINEX data obtained from the International GNSS Server, IGS) one sample every 30 seconds at best. While not explicitly stated, it may be that the authors were looking for a surrogate, or proxy, measure of scintillation that could be calculated from the available data. If this is the case, they should so state. This makes the results of the study more qualitative in nature (is there any correlation between SAMA precipitation and potentially-strong L-band scintillation) than quantitative (how does the level of L-band scintillation vary as a function of precipitation intensity within the SAMA).

I also have reservations about the relatively large (>5 TECU/second) ROTI values shown in this report for some of the Gough Island results (Figure 8 and Figure 21, for example). This infers much larger TEC variations than one would expect, even if the data were from the equatorial region. I suspect that there are still some artifacts in the results such as those discussed in their Section 9.2.

There is also inconsistency in what parameter is being used in the correlation comparisons with the particle data. Text refers to the proxy-S_4 parameter, but the graphs refer to ROTI. From the axis ranges on the plots, I'd guess that the parameter plotted was the ROIT and not proxy-S_4. This latter parameter should, for mid-latitude L-band scintillation, rarely exceed 0.3.

Finally, please note that there are a number of errors in Section 5. For example, it is stated that the S_4 index is proportional to the variance $<\delta N^2>$ and that it varies as $1/f^{2.5}$, when in fact it is proportional to the square-root of the variance and varies as $1/f^{1.5}$. There are also statements made about the phenomenology of scintillation that are incorrect, and others that are not really germane to the issue at hand. These do not change the validity of the conclusions drawn in the study, but I would strongly urge that this section be reviewed and edited carefully by someone more familiar with ionospheric scintillation before it is formally published in any fashion.

2. Correlation Analysis Methodology

The authors should have used some other approach to these data than a bulk analysis of averages and cross-correlation. These tools are predicated on Gaussian distributions, and that does not appear to be the case with these data. A more appropriate approach might have been to ignore the bulk of the data (which is arguable either at or below a non-established noise floor) and apply statistical tools that are designed to deal with outlier events rather than Gaussian-distributed events. From a cursory review of the ROTI correlation plots, I'd say that it would be possible to establish a threshold ROTI value, and then focus on those cases where the ROTI exceeds this threshold. I would then work from this much smaller set of cases. This would address another issue I have, which is that the study does not specifically address where the noise floor is in the GPS data, and this leads to inclusion of noise in the various correlations. As it stands, the statistical correlations shown in this report are largely between noise in one measurement as a function of noise in the other.

3. Application to SKA

Applying this study to SKA only answers the question of potential impacts of SAMA-related scintillation on frequencies above about 800 MHz. Since the authors do not present an analysis of the noise floor in their measurements, it is difficult to assess accurately to what lower frequencies the result can be extrapolated. However, I would be uncomfortable extrapolating much below 800 MHz. The unfortunate truth is that there are no long-term scintillation observation data sets at lower frequencies in this particular part of the world (from many parts of the world, for that matter). This is a weakness that cannot be fixed, so the SKA team should not be surprised if scintillation effects show up in SKA observations at the lower end of the SKA frequency range.

Summary

While there are some flaws in the study, the data presented support the conclusion that there appears to be no compelling relationship between particle precipitation within the SAMA and L-band scintillation at stations near the SA SKA site. However, there does appear to be some evidence of L-band scintillation at the stations near the SA site as well as the stations in western Australia, possibly correlated with geomagnetic activity. The statistics-of-occurrence appear to be low, and given some of the identified uncertainties in the data processing and analysis employed in the study these may be artifacts. However, the presence of L-band scintillation at these locations, coupled with the plans for SKA to operate down to 70 MHz, indicates that ionospheric scintillation may impact both sites during certain geophysical conditions (night during large geomagnetic storms) with certain operating modes (low frequencies, viewing south at low elevation angles). SKA researchers should keep this in mind when trying to understand anomalous results obtained when operating under these conditions, independent of the final site location (South Africa or Australia).