HERA beam paper referee comments and responses

Referee comments received February 23, 2016

Reviewer's comments are in bold, and our responses follow each bold section.

The paper presents a characterization of the power pattern of a prototype HERA antenna, and explores some implications of that pattern on planned EoR measurements with a full-scale HERA array. It is a useful analysis describing meaningful experimental and simulation results, and as such warrants publication in the Astrophysical Journal.

It would have been helpful to compare HFSS and CST simulations with antenna models that are as nearly identical as the software allows, in order to separate the effects of slightly different assumptions from the effects of different simulation codes. Alternatively, changing the assumptions within a single package would give a better handle on the effects of those assumptions. It is stated in the Fig 7 caption that the measured patterns differ from the simulations by substantially more than the simulations differ from each other, and this appears to be mostly the case. The conclusion that "real world effects" dominate the simulation effects would be firmer with a better characterization of the simulations. Note that the text near the bottom of page 5 on this point ("Neither model looks consistently better ...") is confused, and mixes up the issue of which model is better, with the size of deviations from each other and from the data. This should be tightened up.

We have revised our discussion in Sections 2 and 6 to clarify that the choice of numerical EM modeling software is incidental and does not affect our conclusions. The models should be thought of as (1) full-faceted; and (2) perfect paraboloid; and we have adjusted all references and labels in the manuscript. The models are quite similar (agreement better than (~1dB) down to -25dB below zenith; below that, any numerical EM solves becomes quite sensitive to modeling details. In practice, real world antenna-to-antenna variation dominates at that level.

In section 4.1, there is a discussion of the sources of systematic error in the orbcomm measurements of the HERA antenna system, and it is concluded that feed centering is a dominant effect, leading to off-center patterns in figures 6 and 7. While these offsets are obvious, the explanation is less so. It is asserted that adjustments of the feed height can affect the centering, but not the tilt or rotation, however the description of the suspension and positioning system is insufficient to support this conclusion. In fact on page 11 it is stated that the tie-down system for the full array will allow leveling of the feeds, which seems to conflict with the notion that tilt is not affected in the current arrangement without the tie-downs. The assertion as stated may be justified, but the paper should include a better description of the mechanics so that this is apparent to the reader.

We have clarified in the last paragraph of Sec. 4.1 and the third paragraph of Sec 5. The feed is suspended from three ropes, each attached to the center of the feed back plane to a telephone pole beside the dish. Changing the rope lengths does not change the feed rotation or tilt (because all three ropes attach to the same point on the feed), but of course changing rope lengths clearly changes the feed position. Thus because the observed beam deviations from the model vary from feed height to feed height, the most likely systematic is beam centering. The feed tilt and rotation could also be slightly wrong, but they would not change as the feed height is changed. In the full HERA array, each feed will be tied down to the dish surface to mitigate all these errors.

The discussion of collecting area on page 8 leads to estimated aperture efficiencies in the 50-70% range, which is plausible. It is not clear why or how the reduction below 100%, caused by blockage, imperfect illumination of the primary by the feed and structural imperfections, is a choice and what aspect of this choice relates to azimuthal beam asymmetry. For example, is the dish being deliberately under-illuminated to reduce the impact of asymmetric structure in the outer parts of the main reflector? Clarification is in order.

We have clarified in Sections 2.2, 4.2, and 6 that the feed is designed to taper the dipole beam towards the edge of the dish, and also to make the X and Y dipole beams more similar to each other through use of the mesh cylinder draped from the feed back plane around the feed. This feed design has a reduced aperture efficiency over that of a naked dipole, though feed optimization is ongoing (DeBoer 2015).

In section 6, the sensitivity of the delay spectra to the beam models is highlighted. It is unclear what value this has for probing the beam response, as opposed to merely illustrating that imperfect knowledge of that response will complicate interpretation of the spectra. It seems like a negative, not a positive, and places a premium on devising methods for measuring low elevation beam patterns.

We have added to the discussion in paragraph 6 of Sec. 6 to note that direct measurements using drones (Virone et al. 2014; Pupillo et al. 2015) would be ideal, but valuable information on the beam model can nonetheless be gleaned from delay spectra. By forward modeling foreground delay spectra using different MWA primary beam models, for instance, it was observed that the MWA bowtie dipoles are better modeled as isotropic radiators than hertzian dipoles at these low elevations (N. Thyagarajan, private communication).

More generally, the impact of the horizon brightening effect on EoR power spectrum measurements is discussed only qualitatively, noting that this emission is "most at risk" for leaking into the EoR window as a result of calibration inaccuracies and other effects. The computed sensitivities of the HERA array are based only on effective collecting area and noise considerations. Perhaps the leakage of chromatic horizon response is discussed in more detail in the other papers of the series, but it would still be appropriate to comment here on the magnitude of the problem, and the requirements on instrumental specifications that are implied by the sidelobe levels and horizon brightening magnitudes estimated in this paper. Some indication of whether the problem is so hopeless as to render the SNR calculations irrelevant, or whether the

simulated levels can already be handled by established techniques (or something in between) would be appropriate.

Foreground leakage due to beam frequency dependence is exactly what the wedge "buffer", used in the foreground avoidance SNR calculation, is intended to mitigate. We used a buffer of 0.1 h/Mpc (Pober et al., 2014) in our submitted manuscript, resulting in an SNR of 12.7 using foreground avoidance and the best beam configuration. Since our manuscript was submitted, Ewall-Wice et al. (submitted) has finished analyzing the beam frequency dependence, finding that a buffer of 0.15 h/Mpc is needed to avoid wedge leakage over the majority of the band. Using this very conservative assumption, our SNRs shrink slightly from 15-20 for the there beam heights to 10-13. The SNRs for foreground subtraction are unchanged, as they assume sufficiently good calibration and foreground modeling to access all modes outside the main lobe wedge. We have clarified this logic and the SNR values in Sec. 4.2, Sec. 6, and the abstract.