Response to Reviewer #5

Received and published: 10 January 2013

General comments Besson et al. analyze wide-band radar data from the South Pole in order to characterize the ice in terms of birefringence. Basically they use a radar with a single linear polarization, but since the radar is stationary they can acquire polarimetric data by rotating the antenna. They observe the propagation delay and amplitude as a function of the polarization at five internal reflectors in the upper half of the ice sheet. The propagation delays of the reflectors do not vary with the polarization unlike the amplitudes. Also an oblique propagation experiment is described. The paper is interesting, but it lacks focus in the sense that some sections are not related to birefringence i.e. to the title. Observations are not always (attempted) explained or commented. Often details are provided where the relevance is not obvious (to me), and in some cases other details would have been valuable. Many symbols are not defined.

We apologize for the sloppiness here and have tried to address this in the updated draft. The title has been shortened to simply: ‘‘Radio-Frequency Probes of Antarctic Ice at South Pole’’ in an effort to improve consistency with the general scope of the radio sounding data.

Specific comments Page 4695: The title indicates that the comparison of the South Pole and East Antarctica is a key issue, but it is not addressed until the conclusion on page 4707.

In response to other reviewers, the comparison to East Antarctica has been stricken from the title, and soft-pedaled in the remainder of the text.

Page 4696, line 3: The precision is claimed to be 0.5 ns, but the band 0.2 GHz to 1 GHz mentioned page 4699 line 18 corresponds to 1.25 ns.

Perhaps we are not fully understanding, but there are two different issues here - one is the inverse of the bandwidth of the system, which, as the referee realizes, gives the binning of the inverse fourier transform. The other is the sampling frequency; we refer here to the latter. To minimize confusion, we have changed ‘‘precision’’ to ‘‘sampling’’.

Page 4696, line 10: The correlation applies to the amplitude, not the birefringence just mentioned.

Correct; we have made that more explicit.

Page 4696, line 24: In practice the reflection types are not easily separated on the basis of the magnitude.

The revised text reads: ‘‘The three scattering types differ in the magnitude of the radar echoes they produce, as well as the frequency dependence of those radar echoes.’’

Page 4697, line 3: publicatons ¿ publications.

Thank you for catching that.

Page 4697, line 11: Acid vs. density: In practice there are shallow acidity contrasts too, i.e. the overlap of the two mechanisms complicates the discrimination.

Thank you for point that out. The new text now reads: ‘‘Whereas acid scattering may occur at any depth within the ice sheet, density contrasts tend to diminish with over-pressure, and should be largely unobservable below $r \sim$1 km depth’’.

Page 4697, line 18: A COF change over tens of meters hardly causes a strong reflection when the pulse length is 0.5 ns.

Yes, we fully agree here. Although not explicitly highlighted in the paper, the fact that we see temporally well-resolved reflections further argues against the changing-COF-model that was invoked to explain reflections in East Antarctica.

Page 4697, line 23: The baseline amplitude is not proportional to $1/r$. It depends much more on the exponential attenuation caused by absorption and scattering than on the Friis dependency (except maybe at short ranges).

Yes, the reviewer is correct - the 1/r baseline requires infinite attenuation length. We have changed the text to read: ''This volume scattering sets the baseline above which stronger returns from planar reflections ($A \propto 1/r$, for a radio transparent medium) may be visible.''

Page 4698, line 12: Fujita et al. 1996 is not found in the list of references. Our apologies - the reference: ''Fujita, S., T. Matsuoka, S.Morishima, S. Mae, ''The measurement on the dielectric properties of ice at HF, VHF and microwave frequencies.'', Geoscience and Remote Sensing Symposium, IGARSS '93, 1993. Abstract #48, 1258 - 1260 vol. 3'' Should have a date of 1996, rather than 1993.

Page 4698, line 19: The c-axis orient towards the direction of convergence (cf. Y. Wang et al., Annals of Glaciology, Vol. 35, pp. 515-520, 2002). Hence, the convergence/ divergence determines the anisotropy not directly the flow direction, but in practice they might be interrelated to some extent.

We thank the reviewer for pointing out this reference, of which we were unaware, and which has now been added to the bibliography. Yes, the two are clearly, and strongly coupled (as Reviewer #2 has also observed).

Page 4698, line 24: No information is provided on the signal generator. How much power? Is it pulsed and if so does it generate a short pulse or a modulated pulse for subsequent pulse compression?

We apologize for omitting this detail, which another reviewer also noted. The signal at the output of the signal generator is reproduced below, and now included as a separate figure in the source .tex file. As can hopefully be clearly seen from the Figure, the signal is not modulated.

During typical data-taking, the pulser is run at a repitition rate of 100 Hz, although data accumulation is limited by the throughput of our digital oscilloscope. This comment is now included in the main text.

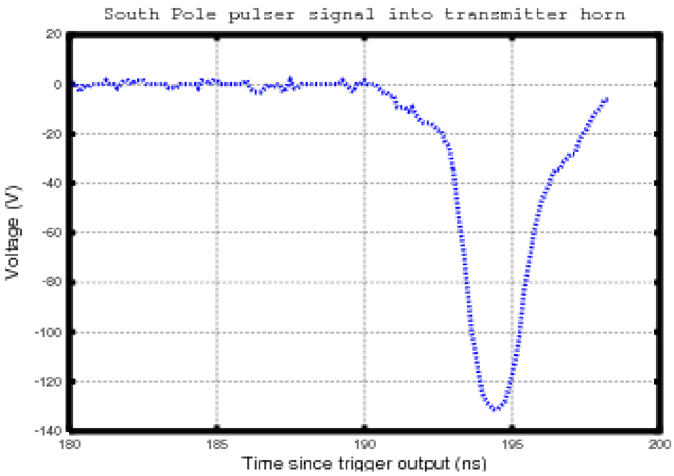

FIG. 1: Pulse generator output signal used for the primary measurements described herein.

Page 4699, line 2: Each cable is connected to a horn. Please make it clear from the beginning that the two horns are for transmission and reception, and specify the distance between the two.

The revised text reads: ``Each cable was then connected to a Transverse ElectroMagnetic (TEM) horn antenna on the snow surface (one for transmission and one for reception); these horn antennas are capable of transmitting or receiving linearly polarized signals.'', and later: ``At a distance of approximately 25 meters from the transmitter horn, a separate coaxial cable connects the receiver horn antenna to the data acquisition electronics.'' Hopefully, this is in the spirit of the reviewer's comments.

Page 4699, line 3: Probably many readers do not know what a TEM horn is. A typical reader would reader appreciate knowing that a linear polarization is transmitted.

Our apologies for this omission. As indicated above, the relevant revised text now reads: ``Each cable was then connected to a Transverse ElectroMagnetic (TEM) horn antenna on the snow surface (one for transmission and one for reception); these horn antennas are capable of transmitting or receiving linearly polarized signals.''

Page 4696, line 3: Likewise, many readers are not familiar with the VSWR. If a plot of the antenna gain as a function of the frequency cannot be shown, please briefly explain why the VSWR tells about the antenna bandwidth, or simply omit the figure.

The revised text now reads: ``These antennas have reasonably good transmission characteristics, from 60 MHz up to 1300 MHz, as indicated by their Voltage Standing Wave Ratio (VSWR) specifications. The VSWR represents a measure of the fraction of signal delivered at the input port of an antenna which is broadcast into the environment, with a value of 1.0 representing 100% power transmission efficiency, and a value of 3.0 corresponding to 75% power transmission efficiency. The VSWR data for the two horn antennas used in this measurement are displayed in Fig. ??.''

Page 4699, line 20: The receiver gain is not very interesting. The noise figure might be.

The noise figure of the amplifiers used for this measurement is 1.8; the revised text now reads: ``and finally amplified by +52 dB prior to data acquisition and storage. For the low-noise amplifiers used in this measurement, the noise figure was reasonably low (1.8).'' One additional comment about the importance of the receiver gain, of which the reviewer is likely already aware. If the receiver gain is too low, then the weak signals will not be observable above the internal noise generated by the LeCroy scope used for our DAQ. On the other hand, if the receiver gain is too high, then the through-air signal observed at very early times will saturate the amplifiers, rendering amplitude measurements for times beyond that compromised. So, although it is a detail which perhaps may not seem worthy of inclusion, it is, in fact, a very important experimental issue.

Page 4700, lines 4-9: What is the implication of the three differences?

We have added additional commentary, so that the expanded text now reads:

1. We use a nanosecond-scale transmitted pulse, vs. ``tone'' signals of frequency ∼100--200 MHz, having duration of order microseconds. Doing so, in principle, improves our ability to resolve fine details of internal structure.

2. Our receiver data acquisition samples at between 1 and 2 GSa/sec, vs. sampling rates which are comparable to the CW signal being broadcast. This also results in improved range discrimination.

3. Reflections are reconstructed directly by averaging, rather than synthesizing the reflected echogram image using SAR techniques. In principle, such averaging directly improves the signal-to-noise by a factor of $\sqrt(N)$, with N the number of samples taken.

Page 4700, line 15: How is the synchronization measured? Cross-correlation like on page 4705 line 4?

Unfortunately, nothing so sophisticated - one can inspect the overlaid signals visually to observe the synchronization. For instance, one such overlay is presented below, showing the received signals around 9.6 microsecond return time:

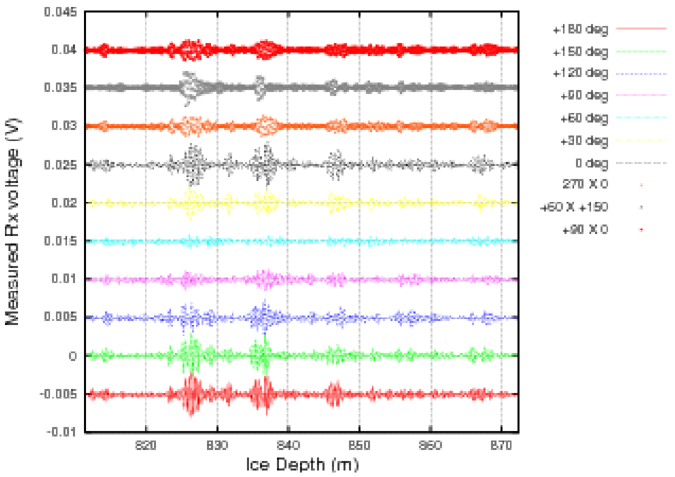

FIG. 2: Zoom of signals observed in the vicinity of 9.6 microseconds return time, converted to depth.

The text now includes zooms of the 6, 9.6 and 13 microsecond reflections to illustrate that synchronicity. We have, by the way, for those zooms, also included estimated range information, anticipating that (and partially in response to the spirit of reviewer #2) some readers may find that more useful.

Page 4700, line 16: Please compare the one nanosecond with the difference in propagation that would be expected in case of a significant birefringence (e.g. as measured in East Antarctica).

Good point. Since the authors of the East Antarctica birefringence claim do not give a numerical value for the magnitude of the birefringent asymmetry, but only claim observation of the effect, it is a bit difficult to quantify. Nevertheless, based on the fact that they observe a full 2π phase variation on the scale of about 500 meters, at a frequency of order 100 MHz (they use 60 or 190 MHz, I believe), corresponding to an in-ice wavelength of about 2 meters, their implied asymmetry is something like 0.2%, corresponding to a temporal asymmetry of order 20 ns for each 10 microseconds of two-way propagation time. The revised text now reads: ``Although the birefringent asymmetry in East Antarctica, as measured interferometrically, was not entirely quantified, the information provided in those papers implies an expectation of, at least, a 10 ns temporal variation in observed return time as the transmitter and receiver sweep through the entire azimuth.''

Page 4700, line 21: Please write explicitly that a missing x means copol.

Okay. The revised text now reads: ``In these Figures, for cross-polarized orientations, the notation ``A x B'' designates the azimuthal polarization orientation of transmitter (`A') and receiver (`B'), respectively. ``60 x 150'' correspondingly indicates a cross-polarization orientation with transmitter at 60 degrees and receiver at 150 degrees; co-polarization orientations are denoted by exclusion of the ``x''.''

Page 4700, line 24: In all plots the yellow peaks is barely visible.

We apologize; an alternative color scheme has been adopted which is hopefully an improvement.

Page 4701, line 1: Why does the power of the cross-pol signal exceed that of the copol signal? Also on page 4707 line 24 this issue is mentioned without any attempt to explain it.

This is, indeed, puzzling. The only possible explanation which occurs offhand is that there is a 'multiple grating' effect which is somehow favoring the cross-polarization orientation. Such speculation is now offered in the text, although, in fairness, and, as the reviewer notes, this is quite unexpected.

We actually considered this in some detail. For the shallowest reflection at 6μs, the cross-polarized reflected power exceeds the power measured in the co-polarized orientation. In the simplest model, a uniform, flat reflecting layer due to simple density contrast is expected to produce an azimuthally uniform return. However, a sloped reflecting layer can produce an azimuthal variation in echo strength. The Fresnel zone has a radial extent R roughly given by $R \sim \sqrt{2\lambda d_{Tx} d_{Rx}/(d_{Tx}+d_{Rx})}/2$; using $\lambda \sim$1 m, and $d_{Tx} \sim d_{Rx} \sim$1000 m, we find $R \sim$30 m. Nanosecond scale interference effects (the maximum allowed by Figure ??) would therefore imply sloping of a reflecting layer by approximately 20 cm/30 m, or less than a degree. Such a gradual slope is not excluded by extant data. In principle, a lateral variation in the wavespeed over a Fresnel zone could also result in such interference. Such a variation might ostensibly be caused by slight variations in the overpressure above the layer. Finally, a previously-undetected circular-polarizing capacity in ice could also contribute to the cross-polarized signal amplitude.

Such an amplitude variation might also be caused by, e.g., a conductive layer with a preferred alignment that results in a `grating-like' behavior. Such a grating would produce cross-polarized reflected power ($\propto \sin\theta\cos\theta$, where $\sin\theta$ is the projection of the transmitter axis onto the grating axis, and $\cos\theta$ is the projection of the receiver axis onto the grating axis) greater than the co-polarized power for some orientations ($\sin\theta\cos\theta > \cos^2\theta$ for $\theta < \pi/4$, e.g.). However, the fact that the observed cross-polarized power for the 6μs reflecting plane exceeds that of the co-polarized power, for all azimuthal angles, is impossible to reconcile in a single grating model, and suggests the presence of an interference enhancement, perhaps of the sort enumerated above.

The text has been accordingly amended to offer these hypotheses.

If the reviewer is interested, we mention additionally that a data sample is currently being collected at South Pole, with a higher-power transmitter, to investigate this phenomenon further.

Page 4701, Section 2.3: A spectral analysis of time windows centered at the reflections would probably be more illustrative than this indirect time domain analysis.

We did, in fact, investigate this, although the frequency-domain plots were less informative than we had hoped (see the response to Reviewer #3).

We note that a previous draft of this paper included the section reproduced below. However, since the results of that section were, we felt, inconclusive, we chose to delete that section from the final, submitted draft:

1. Frequency Domain

Figure 3 shows the power spectrum for the indicated echoes, summed for all co-polarization orientations shown in Figure ??. The ''off-peak'' graph shows the power spectrum for data outside the peak regions, and is primarily the result of the high-pass filter, combined with the frequency characteristics of the transmitter pulser itself. In general, the power spectrum of the reflections is peaked at slightly lower frequencies than the off-peak data, consistent with a power spectrum falling with frequency.

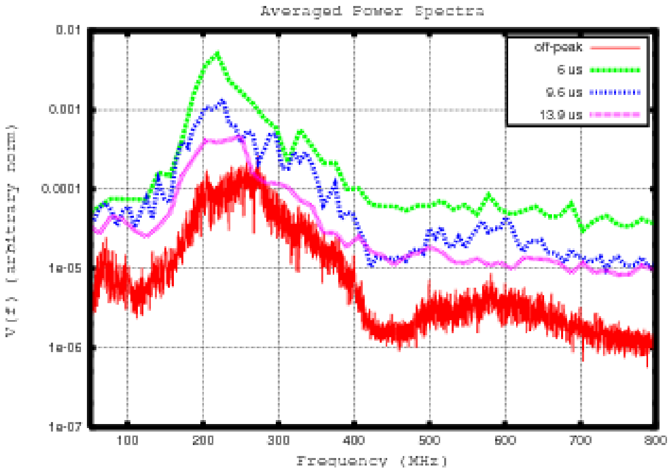

FIG. 3: Power spectra, isolated for indicated echo returns, compared to samples taken outside peak regions.

To absolutely determine whether the observed signal strength of the reflections are consistent with the expected 1/f dependence characteristic of acid scattering, Figure 4 shows the frequency spectrum of the signal generator output compared with a typical reflecting layer (13.9μs at an azimuth of 150 degrees). We observe that the measured frequency spectrum is entirely consistent with the measured signal generator output, modulo the effect of the NHP-250 MHz high pass filter, and some additional cable attenuation effects.

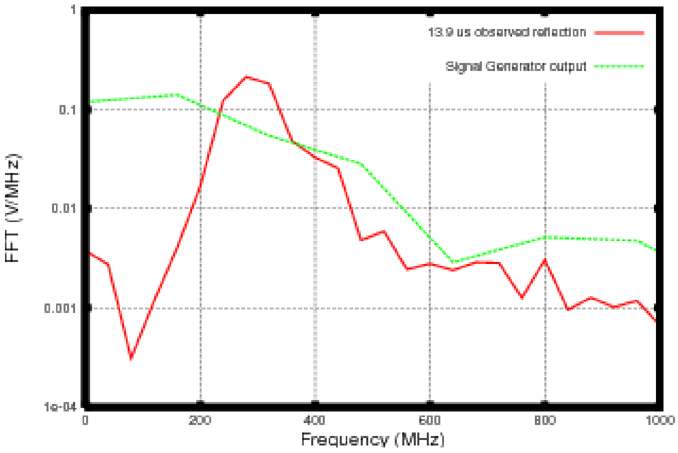

FIG. 4: Comparison of power spectrum of signal generator output (green) vs. FFT for a typical reflector (red).

Page 4701, line 20: Indeed, a longer reflection indicates a smaller bandwidth, which in turn could be a result of a frequency dependent reflection from a (deep) acidity contrast. Is the transmitted spectrum flat? If so please provide this information because otherwise the frequency response of the scattering could flatten the spectrum and have the opposite effect on the extent of the reflection.

Since this and the previous question are related, we will try to address them both, if that’s okay. The reviewer does indeed raise a good point - the temporal extent of the observed reflection simply varies as the bandwidth of that reflection, so a reflector with a 1/f dependence, convoluted with a signal with a rising frequency dependence will sharpen the observed reflection, whereas the same reflector convoluted with a signal with a falling frequency dependence will be extended in the time domain.

The text has been amended to read: ‘‘ Given the falling frequency spectrum of the transmitted signal (the Fourier transform of Figure ??), this indicates higher fractional signal content at lower frequencies.’’

Page 4701, line 23: ice attenuation increases with frequency: So far the frequency dependence of the reflection (not the attenuation) has been addressed.

Yes, perhaps the transition is too abrupt. The text has been amended to read: ‘‘One may ask if our observations of the time-domain reflection characteristics are a simple consequence of ice attenuation effects, which are expected to increase with frequency over this frequency range[?]. The waveform shapes, qualitatively, disfavor a model wherein ice attenuation increases with frequency, as this would tend to reduce the sharpness of the later, rather than earlier returns.’’

Page 4701, line 27: The choice of 500 MHz is unfortunate as it implies that the output of the highpass filter has a larger bandwidth than that of the lowpass filter (since according to page 4699 line 18 the center frequency is 600 MHz). Consequently, the resolution differs.

Perhaps ill-advisedly, but the selection of 500 MHz over 600 MHz was, in fact, purposeful, since the effective height typically varies as 1/frequency. A selection of 600 MHz would have resulted in equal bandwidths, but the S:N would have been disproportionately reduced for the higher-frequency interval.

To rationalize this, the amended text now reads:

‘‘(As 600 MHz represents the center frequency of our bandpass, the selection of 500 MHz results in intervals with unequal bandwidths, but, since the antenna voltage response varies as the inverse of frequency, comparable signal-to-noise).’’

Page 4702, Section 2.4: This section does not seem to address the issue of birefringence. If I am right, please delete it, and otherwise clarify how it relates to birefringence.

The reviewer is quite correct. However, unlike the later case of the depth measurement cited in the paper, which we have excised in accordance with the reviewer’s concerns, the variation in attenuation length with depth, to us, seemed to be a good cross-check of the uniformity of reflection strengths, given the inter-connectedness of ice attenuation and intrinsic layer reflectivity. Again, in keeping with the more general scope, beyond just birefringence, we have accordingly modified the title of the paper, although we have retained this section of the paper draft.

Page 4702, line 20: According to the Friis equation alpha equals 2 (in the far field). Please consider referring to the radar equation (both the (2r)-2 and r-4 versions) instead of the Friis equation. I suppose most readers are more familiar with the radar equation.

Hmmmm... this seems to be a matter of taste, we believe (although the reviewer likely has much more familarity with this than ourselves). The basic point here is to highlight a systematic uncertainty in our assessment of the variation in ice attenuation, which would be true, we think, whether the Friis or the radar equation were cited.

For what it’s worth, the full Friis equation is now explicitly included in the text.

Page 4702, line 15-19: Which temperature profile is used? How are the 6K computed?

We apologize for this omission - the temperature profile used was that of Price et al, from direct *in situ* measurements in conjunction with drilling and deployment of the IceCube experiment. The reference, now cited in the text, is:

Price, P.B. and 9 others, 2002. Temperature profile for glacial ice at the South Pole: Implications for life in a nearby subglacial lake. *Proc. Nat. Acad. Sci.*, **99**(12), 7844--7877.

Page 4703, line 9-28: According to ND Hargreaves (J. Phys. D, 10(9), 1285 1304, 1977) a 90 period results from birefringence, while a 180 period results from anisotropic reflection. I recommend including a reference to this paper rather than the lengthy (and not easily understandable) explanation why the South Pole observations differ from those in East Antarctica. In addition, an explanation of the 180 period is missing. At least Hargreaves has one potential explanation (anisotropic reflection).

We thank the reviewer for reminding us of this paper; the reference is now included. The revised text begins:

‘‘Mechanisms responsible for observed azimuthal dependences in radio echo soundings were outlined nearly 40 years ago[? ?]’’.

The remaining comments of the reviewer primarily address measurements that were made in 2011-12 at South Pole, in an attempt to observe birefringence projected onto the horizontal plane. Our hope was that, despite limitations of horizontal baseline, that the ‘‘second’’ signal, which should be present if there were birefringence for the horizontally propagating signal, might still be visible. As the reviewer realizes, this was not the case, and we are planning to deploy an additional rig in 2013-14, with a stronger transmitter and a ’smarter’ trigger system, that will hopefully allow such long-baseline signals to be observable. In retrospect, we realized that a precise depth measurement, nevertheless, could be extracted from those data.

Nonetheless, and in accordance with the reviewer’s wishes, this section has now been completely removed, and will, instead, form the basis of a separate paper.

Page 4704, line 5: The meaning of three-dimensional is not very clear to me. Is it the orientation of the preferred c-axis in 3D, not just in the horizontal plane?

Page 4705, line 5: The oblique scattering from the bedrock results in a long return waveform. Hence it makes sense to use the onset. Please explain that this is due to the rough surface and the geometry. Also, so far the signals polarizations have been quite similar (except for the amplitude and potentially the propagation delay). Now the polarizations are completely different. Please explain why.

Page 4705, line 12: Indeed the horizontal component of the V polarized return from the deep bedrock becomes very small when the RX-TX separation is small. In this case, is the experiment worth anything? If it is not, please delete this section.

Page 4705 Section 3.1: This section does not seem to address the issue of birefringence. If I am right, please delete it, and otherwise clarify how it relates to birefringence.

Page 4706, line 21: I understand that you compare with the BEDMAP grid points (not the radar sampling points on which the BEDMAP is based). If so, the comparison does not make sense in my view.

Page 4706 Section 3.2: This section does not seem to address the issue of birefringence. If I am right, please delete it, and otherwise clarify how it relates to birefringence.

Page 4707, line 14: In my view it does not make sense to compare radar measurements at Dome Fuji and South Pole without discussing the glaciological differences.

Yes, okay. We have added a paragraph to the text which represents the result of a non-exhaustive literature search. I suspect other persons will do a better job of interpreting these results, in conjunction with the East Antarctic results.

Page 4707, line 21: I am not sure monolithic is the correct word. Clearly the ice is polycrystalline. Maybe uniform is more correct?

We have substituted ‘‘uniform’’ for ‘‘monolithic’’.

Page 4708, line 5: Do you really mean that the results from the oblique propagation confirm the results obtained with the vertical propagation. The former addresses the entire ice thickness the latter only the upper half. Also, the former suggests birefrin-gence, while the latter does not.

As mentioned previously, we have excised reference to the oblique scattering measurements.

Page 4708, line 11: propagation perpendicular ¿ propagation parallel

Thank you; that was an egregious error.