

Response letter to referees' comments

Prof. Magnus Karlsson

1) Eq 2.5 contains a typo - the square on responsivity should be removed.

The square has been removed.

2) I am not sure that the dependence on M in the erfc argument in (2.6) is correct. I think it applies to symmetric (centered PAM, not PAM that includes the 0-level. A reference to motivate (2.6) should be needed anyway.

The referee is correct. That expression evaluates the Symbol Error Rate of a symmetric antipodal PAM transmission, and it does not apply to the considered scenario. In fact, throughout the thesis, this expression was never used to evaluate the SER and/or the BER. The purpose of (2.6) is only to compare the correct expression (2.5) with the expression that is commonly found in textbooks (2.5). Nevertheless, to avoid further confusion by the reader, equation (2.6) was removed.

3) DP should think about whether the factor of 8 difference between IM/DD PAM and coherent M-QAM mentioned in ch 2.3.2 is correct. I would have expected 4. If it remains it will be an interesting topic for discussion!

The factor 8 comes from the combination of three effects:

- Polarization multiplexing (factor of 2)
- Use of two quadratures instead of one (factor of 2)
- Root-raised-cosine pulse shaping (factor between 1 and 2, depending on the roll-off)

Assuming Nyquist pulse shaping (0% roll-off), the factor is 8. To address the referee's concern, a short sentence was added to further justify this comparison.

4) Some strong statements about SSB in ch 4 and the concluding Ch 8 is not well supported and should be softened in my opinion. For example ch 4.4: "SSB/VSB signal is immune to chromatic dispersion" is incorrect, rather ref 179 shows that it can be compensated in the electric domain. It is certainly not "immune". And ch 8: "SSB significantly outperforms IM/DD systems in dispersion-uncompensated links longer than a few kilometers..." this is not convincingly shown for these systems; in particular BER has not been evaluated; only SNR and selected it could be questioned if these noise sources are sufficient model of reality for these links. Does it hold for all baudrates, does it assume a specific DSP, does it depend on nonlinearities, dispersion maps and degree of WDM loading - all these may be crucial caveats. Without exp support I would in fact not believe in these conclusions.

The statements about the immunity of SSB over chromatic dispersion were softened. Moreover, an additional reference (<https://dx.doi.org/10.1364/OFC.2017.Th2A.29>) was added, which evaluates the penalty caused by the interaction between dispersion and OFDM-SSB transmission.

5) Eq 5.1 is incorrect (not significantly, but still), and if the author believes it is correct I challenge him to provide a reference in support. In fact such a ref would be valuable anyway, because people make various approximations when defining and using the noise figure, so it could be interesting to see what the opinion of the author is.

The referee is right that that equation is not strictly correct. The equation assumes that the spontaneous-emission factor of the EDFA is half of the noise factor. However, this is only an approximation for large gains. Therefore, Eq. (5.1) was rewritten as a function of the spontaneous-emission factor, and a new equation (5.2) was added, which approximates the noise figure. As suggested by the referee, a reference (10.1364/OL.15.001064) was added, which contains the equations that are reported in the thesis.

6) The index n is not defined in (5.3)

The index n represents the discrete-time index, T-spaced. A short description was added in the text.

7) The probabilistic shaping stuff was, as far as I can see, evaluated in terms of measured or numerically estimated MI, but there is no description whatsoever on how this MI is computed/estimated. Nor it is in the JLT paper - or at least I could not find it. This is not trivial to do from measured histograms, and will always be accompanied with approximations and assumptions. Therefore I would require a description (short would be enough) of how the MI was calculated from the measured data, and some estimations of its accuracy as it underlying all conclusions in that paper.

The AIRs (MI/GMI) were evaluated using a Monte-Carlo integration on the received symbols, assuming that the channel is AWGN (Chapter 6 and 7) or AWGN with Tikhonov phase noise (Chapter 7). This gives an AIR that is lower than the AIR calculated with a channel model that is estimated (e.g. using histograms) from the received samples. However, the result is still achievable. Moreover, by assuming an analytical channel model, calculation of the LLR is very fast.

To address the referee's concern, a short paragraph was added in Sec. 6.2.2.

8) And while fixing 7), please argue why MI and not GMI is the chosen metric.

Actually, in the thesis, both MI and GMI were used. The use of two different metrics lays in the fact that they refer to two different experimental investigations, done in different times.

Using the MI metric allows to infer the maximum AIR, without making any assumption on the receiver structure. On the other end, by using the GMI one assumes a receiver with a bit-wise decoder. Both of the AIRs are formally correct,

even if the GMI is more “practical”, since it specifically targets a widely used coded modulation scheme (BICM).

Nevertheless, to address the referee’s concern, a short sentence was added at the end of Section 6.2.5 to highlight the limitations of the MI metric.

9) Don't you have any nice constellation diagrams to show from the 16-QAM /shaped 64-QAM experiments? Would love to see them!

Unfortunately, we did not capture constellation diagrams for these experiments.

10) As soon as you use GMI for characterization you would need to specify the bit-to-symbol mapping, which is almost never done, but the GMI dependence on SNR may differ significantly depending on the bit mapping.

For square QAM constellations, standard Gray code was assumed. With 32-QAM and GS-32-QAM, due to the absence of a Gray code, the bit mapping was different. An Appendix was added with Tables that provide the constellation points and the bit mapping for these two constellations.

Dr. Robert Killey

Page 11, in the sentence before equation 2.1, ‘from the equivalent-input noise power spectral density i_n should read ‘from the equivalent-input noise power spectral density i_n^2 .

The sentence has been modified.

Page 12, an explanation of the derivation of equation 2.5 would be helpful for the reader. Should the detector responsivity squared , be multiplied by the OMA_{out} , which is proportional to optical power?

There was a typo in the equation, and the responsivity was not supposed to be squared. The equation has been corrected.

Page 14, 16, 49, and 53, the text refers to Intra-DC, where it should be Inter-DC

This mistake has been fixed.

Section 3.2, the approach requires a well-controlled wavelength offset between the channels propagating in opposite directions, to avoid interference penalties due to back reflections. However, this has the disadvantage that more precise laser wavelength control is necessary, which increases the cost in this cost-sensitive application. This potential drawback should be mentioned.

A sentence at the end of Section 3.2 and in the conclusions of Chapter 3 was added to clarify this potential drawback.

Page 24, ‘ $R = P_{rifl} / P$ is called reflection’ -> ‘ $R = P_{rifl} / P$ is called reflectivity’

The parameter R has been renamed from “reflection” to “reflectivity” throughout the Chapter.

Page 25, explicitly state that a and b refer to the symbols’ optical powers

A short explanation was added, explicitly stating that a and b refer to optical powers and not amplitudes.

Page 31, ‘the total reflection R ’ -> ‘the total reflectivity R ’

See previous comment.

Page 38, when comparing the bandwidth requirements of SSB direct detection and coherent receivers, make it clear that the coherent system being considered is a homodyne coherent system (as opposed to a heterodyne one, which has the same bandwidth required as the SSB DD Rx).

The word “homodyne” was added to explicitly state that the comparison has been carried out with respect to a homodyne system.

Page 45, a more detailed explanation of the derivation of equation (4.17) would be helpful.

More details on the derivation of equation (4.17) were added.

Page 51, a drawback of applying the predistortion to the signal is the resulting spectral broadening, which will require a DAC and modulator with higher electrical bandwidth. This should be mentioned in the text.

A short sentence on this drawback has been added at the end of Sec. 4.4.1.

Section 4.4.2, state the net bit rate at which the results are obtained.

A short paragraph at the beginning of Section 4.4.2 was added to state the gross and net bit rate used in those simulations.

Page 61, ‘A single-polarization coherent transmitter is identical to an SSB transmitter’ – clarify this statement, as some SSB transmitters use a second modulator or laser, or additional RF electronics to generate the optical carrier, which is not used in the coherent Tx.

The statement was slightly rephrased to avoid confusion.

Page 75, in Fig. 6.6 it’s quite difficult to see from the 3D plots that the probability of the outer points of the PS-64QAM format with the Maxwell-Boltzmann distribution is lower to that of the exponential distributed constellation. It may be easier to see with a logarithmic axis for probability.

The Z-axis of Fig. 6.6 was changed to a logarithmic scale to ease the readability.

Pages 78-79, are the 0.9 dB and 1.05 dB penalties observable from the plots in Fig. 6.9?

The plot of Fig. 6.9 was changed to show the theoretical curves of the modulation formats, from which the penalties were calculated.

Page 78, the performance of the system, including maximum reach, are obtained using mutual information calculations. Can any conclusion be drawn for the case where GMI is used, or a practical FEC? Either this should be included, or the section should mention this as possible future work.

Since, at medium/high values of SNR, the MI and GMI for Gray-mapped square QAM constellations are very close, we do not expect big differences by changing the AIR. However, a proper analysis taking into account a specific (and practical) receiver structure should be carried out in future research. A short sentence at the end of that Section was added to address this limitation.

Page 81, the SNR gap is used to quantify performance of the 64-APSK signal. Can optimal, or near optimal, bit-to-symbol mapping be performed, and can the SNR gap be easily translated to BER increase or information spectral density reduction?

Performance of the APSK constellation, explained in Section 6.3, was measured using the GMI performance metric, i.e. the bit mapping was taken into account. In fact, the tested APSK constellation has a near-optimal bit-to-symbol mapping, which explain its good performance in terms of the GMI.

The text in Section 6.3 was slightly changed to avoid confusion.

Page 103, in Appendix B, the second term on the right-hand side of equation B.3, $\sqrt{x \int (t)/SIR}$, should not be square rooted, as the photocurrent is proportional to the optical power, not optical field.

This error has been corrected.

Additional minor typos and grammatical errors to be corrected:

I thank the referee for spotting those errors, which have been corrected.