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Interactive comment on "Characterisation of subglacial water using a constrained transdimensional Bayesian Time Domain Electromagnetic Inversion" *by* Siobhan F. Killingbeck et al.

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This is a generally well written paper by Killingbeck et al, which I recommend publication of on the condition that the following minor points be clarified:

Page 2 Line 8: It is better to expand TEM as Transient EM instead of Time Domain EM. If the authors wish to say Time Domain EM it is more appropriate to say TDEM.

Page 2 Line 13: "electromagnetic fields to investigate subsurface resistivity structure INDIRECTLY by measuring TRANSIENT eddy currents"

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Page 2 Line 22: Please change eddy currents to “transient decay”

Page 3 Line 6: TEM / TDEM confusion

Page 3 Line 15: More time domain / transient confusion. For example, GPR can also be considered a time domain method, when analysis is done in the time domain. However, quasi-static diffusive EM geophysical methods, when analysed in the time domain, typically involve transients, hence the more appropriate “Transient EM” or TEM.

Page 3 Line 20: Change “time dependence” to “switch off”

Page 3 Line 21: The eddy currents PRODUCE a secondary EM field

Page 3 Line 22: The receiver TYPICALLY measures the induced . . . in the off periods. We can measure during on-periods too, it is harder to model.

Page 3 Line 25: The authors could mention somewhere around here that conductive material implies slower transient decay (e.g., Figure A4 of the manuscript) and sustenance of the induced subsurface eddy currents

Page 3 Line 3): Equation 1 is an approximation I believe, should be mentioned.

Page 4 Equations 2: t is not necessary in the data vector as time is not an observable. Just to be clear here, the authors should mention here that the mean recording in a stack window is used as data, and the variance of the mean (i.e., variance of the measurements divided by the number of measurements in the window) is the variance of the data. Population variance is not the variance of the mean. Also, the stack, through central limiting admits the use of a Gaussian likelihood.

Equations 3) We do not use the evidence constant for trans-D. Better to leave out p(d) and say p(m|d) \propto p(d|m) p(m)

Page 5 Line 8: Using depth dependent priors in a trans-D formulation is not strictly allowed (see Bodin and Sambridge 2009 for why this is so in the development of the prior). One can however use trans-D with depth independent priors and transform to

depth dependent values before modelling.

Page 11 Lines 20 onwards : As the authors point out on the following page, it is not surprising that better resolution is available when conductivity thickness tradeoffs are restricted. The authors could look at another approach of conditioning the posterior after inversion (Ray and Key 2012). However, I would recommend mentioning that the fixed interface depths may be allowed to vary, as the uncertainties on GPR interfaces are not as low as purported in the manuscript (see Ray et al 2016 for uncertainties on seismic reflectors and analogous wave physics, for example).

Also, the authors may try proposing from the prior for birth for better convergence (Dosso et al 2014).

Page 14 Section 5.2 and Figure 8: Marginal uncertainties along a 2D line can also be displayed instead of showing modal models, as shown by Ray et al 2014, Figure 11

In conclusion, since the authors have carried out probabilistic inversions of EM and shear wave dispersion data, I would recommend they try and present their conclusions on the facies classifications of the geology also in a probabilistic manner (or at least mention that this can be done). This requires some thought but will provide a much more informative set of displays than Figure 10.

All the best in your revisions, Anandaroop Ray

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