Authors’ response to G. Caumon interactive comment on “Monte Carlo Simulations for Uncertainty Estimation in 3D Geological Modeling, A Guide for Disturbance Distribution Selection and Parameterization”.

This paper shows on a synthetic and real examples that using spherical orientation distributions to describe structural data uncertainty is important in 3D structural uncertainty quantification. This is important, as most work considering structural uncertainty (including some papers I co-authored) have neglected spherical distributions and used simpler and independent statistical models for plane strike and dip. The results show that a more careful consideration of spherical distributions can have an impact on Uncertainty Quantification results, and some interesting statistical insights are provided. The authors also provide their models as supplemental material, which I consider very useful, and give practical guidelines to use Fisher distributions in the Appendix, which is also useful for practioners. So, I think this paper deserves publication. However, I have a few problems and I am still unclear about some parts of the paper. Therefore, I am making several comments and recommendations below, which I hope will help the authors to make the paper easier to read and more precise.

Specific comments

1. I had some difficulties to understand the paper. There are locally some purely formal aspects to it, which a careful reading could easliy fix. More importantlty, part of the reason is that several of the statements appear as general truths in the paper, whereas they only hold in some cases or under some assumptions that are not explicitly described. I think these inappropriate generalizations should be addressed before publication. Another reason is that some of the ideas and principles are not always clearly expressed (in particular in Section 3.2). Overall, I find the beginning of the paper not very easy to read and to understand. I have higlighted several of these issues in the annotated pdf manuscript, I hope this will help the authors make the paper easier to follow.
2. I think the term MCUE does not precisely describe what the authors have in mind (by the way, "Monte Carlo simulation **for** uncertainty estimation" (as in the title) seems clearer to me than "Monte Carlo simulation uncertainty estimation" as in the abstract and main text). Indeed, the fact that this paper focuses on ** data ** perturbation is not clear from this wording. Indeed, MC perturbation of model parameters is another (and widely used) approach to sample uncertainty in structural modeling (see seminal work by Abrahamsen (Geostats 1993) for horizons; Lecour et al (Petrol. Geosci 2001) for faults and many other papers since then). MC simulation is also used to change how data can be connected in structural modeling (see work by my co-authors N. Cherpeau and C. Julio). I understand these model perturbation approaches are not the focus of this paper, but it should be clear to the readers from the outset that there is more to geological uncertainty than orientation data perturbation. Therefore, I would recommend to replace MCUE by a more specific term (inclusing in the paper’s title). My two-dime suggestion would be "data-related structural uncertainty quantification", but the authors may find a better term. This distinction is essential and should be clarified.
3. I disagree with the statement (line 26, page 3) that data perturbation and kriging are “equivalent to running geostatistical simulation”: has this been mathematically shown or experimentally proven? I have 99
4. Geological modeling is bound to be implemented with software; however, a paper can gain much by clearly separating the mathematical and methodological principles from the software platform used for demonstrating these principles. So, in clear, I consider that this work is compatible with other implicit structural modeling methods, and that this could be argued before the introduction. Also, a key feature of implicit modeling schemes and a motivation for using them is this work is that they use orientation data. This could be stressed in Section 2.
5. Maybe merging Sections 1 and 2 could help more effectively set the scene and introduce the contributions of this paper?
6. I would recommend to comment on the links between this paper and Carmichael and Aillères, (JSG 2016). They also use spherical distributions in a 3D structural modeling context.
7. I am not sure I agree with the statement that “Uncertainty will then be best represented by disturbance distributions that are consistent with the Central Limit Theorem”. The CLT holds when N independent

random variables are added; in the case of orientation data independence may be assumed but is not granted, and N is likely small. I would, therefore, rather state this as a convenient hypothesis for the argument than as an ideal objective. More generally, the main point of Section 3.1 seems to be that Normal and Von Mises-Fisher distributions are appropriate for structural uncertainty management. I am ready to accept that they are very convenient and useful, but I don't think I agree with the term "appropriate". For example, Thore et al (2002) show that propagation of seismic velocity uncertainties through reflection data imaging yields non-symetric distributions about horizon positions. The same comments holds for the first sentence of the discussion (Section 5).

8. Section 3.2 is not really clear to me. When I first read the paper, I understood that Eq. (4) suggested that all orientation data samples have the same mean/dispersion (and was not clear about why this should be the for samples taken at different locations). I now think that (4) concerns a single orientation at a given location. But then, it would be good to state that X1, . . . , Xn on line 26 correspond to repeated measurements (or whatever it stands for), and to specify the meaning of n. Overall, expressing the idea and assumptions behind the equations would help the non-mathematically-inclined readers better understand Section 3.2 and how it can be applied. For example, it would be worth mentioning that xi are assumed to be independent samples before Eq. (9); That Eq. (10) is nothing else than the total probability formula. I don't get what N stands for in (11). Overall, after reading through, I get the general idea but I am still unclear about the details and whether n stands for the number of repeated measurements (which is not available in most data sets) or for some number of simulated points.
9. I don't understand the principles behind Eqs. (12-17). More explanations would be much welcome. In the end, I am not fully sure at the end of the section about how the development can be used in practice, and what a "propoer parameterization" (page 7, line 26) exactly means. If some orientation measurement device came up with a good evaluation of the orientation probability distribution based on a sound error propagation, would we still need Section 3.2?
10. Overall, maybe I missed a point, but it seems that section 3 summarizes some facts from the statistics literature. This is probably useful, but I feel that this is a bit long and I don't clearly see connections to the experiments made in Section 4, which essentially address the use of spherical distributions for sampling data uncertainty. So I am not really clear about the point on posterior predictive distributions (PPD) in section 3.2. Is it really needed in this paper? What does it bring in? Similarly, the discussion in Section 3.3 seems to mainly serve the point that two angle distributions instead of a spherical distribution entail heteroscedastic effects. So maybe it would help to get more rapidly to that point directly
11. in Section 4. on p. 10, I am a bit puzzled by the term "dip vector". It is clear that a plane should be represented by the spherical distribution of its pole; but it seems to me that the experiment mainly describes the "dip vector" by the dip and dip angles. If so, the term dip vector seems inappropriate (as it could also be described by a spherical distribution).
12. some more details about how the input uncertainty used in the Mansfield case study would be useful (see comments on page 11). Does this connect to Section3.2 ?
13. In the discussion, the authors may want to add a point on spatial correlation or C5orientation data. Does it make sense to sample orientation data independently? Wouldn't something like Gibbs sampling be good in that case?
14. Form: please check that all math symbols in the text have the same font as in the equations. (e.g., line 10, p5)

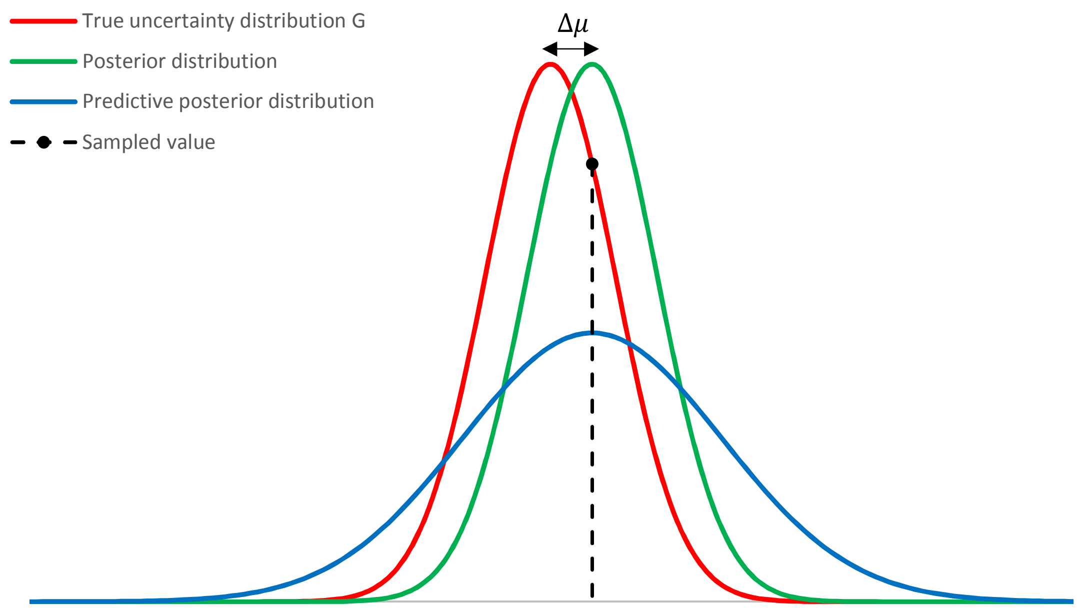
Author's answer to the general comment:

The authors thank the referee for his positive review and agree that assumptions need to be made clearer. The paper was reworked to address both referees' general, specific and inline comments.

Answer to the specific comments:

1. The paper was revised and edited to address the formal comments (syntax, fonts, symbols). Formerly implicit assumptions are now clearly stated. The descriptions of several equations were updated and, two equations were simplified to improve readability.
2. Title was changed to Monte Carlo Uncertainty Estimation on Structural Data in implicit 3D Geological Modeling, A Guide for Disturbance Distribution Selection and Parameterization. The method was not renamed to preserve consistency with previous works (Pakyuz-Charrier et al., 2017;Giraud et al., 2016a;Giraud et al., 2017;Giraud et al., 2016b).
3. The statement about geostatistical simulation was removed.
4. The case for MCUP being applicable with any implicit 3D geological modeling engine is now made clearly.
5. Initially, section 1 and 2 were one. However, this made for a very lengthy introduction that spilled into method description. Describing MCUP in detail is not the purpose of the introduction and that is why we had it split.
6. Reference was made to Carmichael and Ailleres 2016 work were due.
7. Structural data measurements arguably fall under the CLT due to the numerous source of uncertainty that add themselves in the surveying procedure (see inline comments). The authors agree that other types of data may or may not abide to the CLT. However, these other types are beyond the scope of this paper.
8. n and N are now explicitly defined. Equation 4 describes the variable of interest that the operator samples from when making measurements on the field at a single location. Our objective is to give the best estimate possible of G. Equation 10 describes G in light of the measurements. It is very different from Equation 6 in that it gives the empirical distribution of G instead of the distribution of the average of G.
9. A quick example to understand about section 3.2 and the meaning of equations 11 to 17
   When a single measurement is made at a specific location, the sample size is 1 ($X = \{x_1\}$) and the observed average is equivalent to the measurement itself ($\mu \equiv x_1$). If we assume the dispersion function to be completely deterministic then we already know ϑ_{true}. Obviously, the posterior distribution will be
   $$p(\mu = x_1 | X = x_1) = p(x | x_1, \vartheta_{\text{true}})$$
   Now we could think that $p(\mu = x_1 | X = x_1)$ is the disturbance distribution we want to draw from but that will lead to systematic underestimation of the effect of ϑ_{true} because $p(\mu|X)$ only quantifies our knowledge of μ in regard to x_1. Indeed $p(\mu|X)$ tells us about all the possible G that x_1 might be sampled from, it is a distribution of the average and ultimately, we do not know exactly how far μ is from μ_{true}. That is, if we choose to sample from $p(\mu|X)$ we are ignoring the fact that $\Delta\mu = \sqrt{(\mu - \mu_{\text{true}})^2}$ is unknown and that means we will be perturbing with an unknown bias. To account for this, we compound $p(\mu|X)$ to itself
   $$p(x | p(\mu|X), \vartheta_{\text{true}}),$$
   Which, is practically equivalent to a double sampling of $p(\mu|X)$. Consequently, regardless of the quality of our prior knowledge about ϑ_{true}, sampling from the posterior predictive distribution is better than sampling from the posterior distribution. Eq.11 and Eq.14 give easy ways to achieve that for the normal and vMF cases respectively.
   Equation 12 and 13 can be removed without damaging the meaning of the paper. Although, we would then fail to give credit to previous work and, in fact, "pretend" that no solution was ever thought of to obtain a vMF predictive posterior distribution.

The empirical approximation at Equation 14 is a byproduct of this research. I will probably submit the detailed process of obtaining it as a short note the future.
See the figure below for graphical explanation.



10. Yes and no, Section 3.2 gathers facts from the statistics literature and puts them together into a coherent procedure with a defined aim (MCUP). A mere summary would lack that aim. It is difficult to simultaneously have this section shortened and address the implicit assumptions issue that was pointed at earlier.
11. Dip vectors are now called dip vectors where due. As a side note, the dip angle and dip direction angle are an expression of the components of a dip vector under a specific spherical coordinate system.
12. The disturbance distribution parameterization used in the Mansfield case is reasonable in regard to previous metrological studies. However, it is by no means ideal and the Mansfield case is merely a proof of concept example. Defining the error functions is the responsibility of the practitioners. Practitioners may use existing metrological studies if their specifics match that of the survey. In absence of such data, one may define disturbance distribution parameters heuristically or conduct their own metrological studies.
13. Even if there is strong spatial correlation there is actually not much sense to let it alter the sampling itself. Indeed, the error about a structural measurement from, say, a regular compass is largely independent from the previous measurement. What may be correlated is the average, not the error. That is so even if one repeats measurements at the same location over the same feature. Spatial correlation comes into play when one considers final model uncertainty stationarity issues.
14. Symbol fonts in the text were adjusted to match the equations.

Answer to the inline comments:

Page 1, line 7:

G. Gaumon's comment:

structural would be more precise

Author's answer:

Updated to geological structural modeling

Changes to the paper:

…geological [*structural*] modeling…

Page 1, line 11-13:

G. Gaumon's comment:

There is more: the lack of klnowledge about what occurs bewteen the observations.

Author's answer:

Agreed. Added.

Changes to the paper:

…parameterization)[*and the inherent lack of knowledge in areas where there are no observations*] combined…

Page 1, line 15:

G. Gaumon's comment:

Is this paper only useful as a Geomodeller extension or can it be useful with other geomodeling approaches / tools?

Author's answer:

On principle, MCUE is applicable to any implicit modeling engine. Removed incriminated section.

Changes to the paper:

…modeling ~~using GeoModeller API~~.

Page 1, line 15-16:

G. Gaumon's comment:

This wording does not seem very standard. MC methods are a classical way to produce Uncertainty Quantifications.

Author's answer:

In general, they are, although their application to implicit 3D geological modeling engines is still emerging.

Changes to the paper:

Monte Carlo simulation [*for*] Uncertainty Estimation

Page 1, line 16:

G. Gaumon’s comment:

Not sure why MC is termed "heuristic".

Author’s answer:

This is a remnant of a previous reviewer’s remark. At the time it was argued that MCUE is heuristic because propagation of uncertainty from the random variables through the Kriging process to the estimated random function may be achieved analytically (under a series of more or less safe assumptions). I have no sympathy for this term and will therefore remove it.

Changes to the paper:

…MCUE), a ~~heuristic~~ stochastic…

Page 2, line 10:

G. Gaumon’s comment:

Wording unclear to me. Please rephrase.

Author’s answer:

Rephrased

Changes to the paper:

…uncertainty~~, which can be equivalent to their reliability for decision making.~~[*as an aid to risk-aware decision making.*]

Page 2, line 11:

G. Gaumon’s comment:

For what ?

Author’s answer:

For uncertainty estimation in implicit geological 3D modeling. However, the sentence is clunky, inelegant and, at the time this paper will be published, inaccurate.

Changes to the paper:

~~Nearly all the methods proposed in the past five years~~ [*Monte Carlo Simulation for Uncertainty Estimation (MCUE) has been a widely used uncertainty propagation method in implicit 3D geological modeling during the last decade*] **(references)**~~ are based on Monte Carlo simulation uncertainty estimation (MCUE)~~.

Page 2, line 12-13:

G. Gaumon’s comment:

I find this description confusing, as it mixes two related but distinct elements: methods to sample the prior distribution and Bayesian methods based on likelihood computation.

Reading this suggests that Wellmann and Regenauer-Lieb (2012) or Lindsay et al (2012) use Bayesian methods, which they don't.

Author’s answer:

Although this paper discusses solely the particulars of input data perturbation, MCUE itself is not limited to that and is supposed to include validation steps as a condition to merging (Figure 1). This validation step may or may not be based on Bayesian methods and in that sense the cited works are “partial/incomplete” MCUE. However, discussing validation steps is beyond the scope of this particular paper.

Changes to the paper:

…MCUE). ~~This~~ [A similar] approach…

Page 2, line 15-16:

G. Gaumon’s comment:

I understand that the main point of the paper is about data perturbation, but this statement needs to be modulated: Perturbing the data is only one way to sample geological uncertainties. Perturbing how the data are connected by geological features and perturbing the geometry of these features is another (and quite significant) way to sample uncertainty as well.

Author’s answer:

In its very general wording, MCUE could integrate “all” perturbing methods. Regardless of its type, if one wants to perturb data, a disturbance distribution of some sort has to be defined beforehand. This process of selecting, parameterizing and sampling from the disturbance distribution makes the perturbation strategies mentioned compatible with the basic definition of MCUE.

Changes to the paper:

~~Instead of estimating the uncertainty from a single best-guess model,~~ MCUE (Fig. 1) simulates ~~it~~ [*input data uncertainty propagation*] by…

Page 2, line 22:

G. Gaumon’s comment:

You could also add kriging estimation variance.

Author’s answer:

True, although it could also be a statistic derived from it as kriging error and kriging is computed for each realization. In any case, this statement is about uncertainty indexes used for MCUE

Changes to the paper:

…final [*model*] uncertainty [*in MCUE*], including...2012), stratigraphic variability [*and kriging error*]…

Page 3, line 11:

G. Gaumon's comment:

And on a variogram model (or regularization : smoothness term for discrete implicit approaches)

Author's answer:

Agreed.

Changes to the paper:

…data [*variographic analysis*] and topological…

Page 3, line 19:

G. Gaumon's comment:

reference?

Author's answer:

Added.

Changes to the paper:

..used [*(Maxelon and Mancktelow 2005)*]….

Page 3, line 23:

G. Gaumon's comment:

This paper is not about the interpolator, but about MCMC perturbation of models by mathematical morphology.

Author's answer:

This is a mistake, I meant to cite only Part I of the "double" paper.

Changes to the paper:

…(Calcagno et al. 2008~~; Guillen et al. 2008~~; FitzGerald et al. 2009)…

Page 3, line 24:

G. Gaumon’s comment:

Not sure what this means.

Author’s answer:

It means there is sense to perturbing the input data to estimate uncertainty because kriging actually considers the dataset via variographic analysis.

This relates to kriging being a stochastic interpolator as there is arguably no meaning in propagating uncertainty using MCUE on a non-stochastic interpolator. Indeed, running MCUE with, say, a spline interpolator will likely generate numerous ridiculous model realizations that cannot be deemed “plausible”. In a practical sense, Kriging allows (perturbed) plausible datasets to produce (mostly) plausible models.

Changes to the paper:

propagated [*provided that the variogram is correct*]

Page 3, line 25:

G. Gaumon’s comment:

Provided that the variogram model is correct.

Author’s answer:

Correct.

Changes to the paper:

See above.

Page 3, line 26:

G. Gaumon’s comment:

I disagree.

Author’s answer:

More work needs to be done to ascertain this claim.

Changes to the paper:

~~Note that MCUE applied to the co-Kriging interpolator used in GeoModeller is, in effect, equivalent to running a geostatistical simulation.~~

Page 4, line 5:

G. Gaumon's comment:

Not sure

Author's answer:

True, this is an assumption that is stated explicitly later on.

Changes to the paper:

…many ~~independent~~ random…

Page 4, line 9-13:

G. Gaumon's comment:

Not sure. The CLT states that the addition of N random variables converges to a normal distribution when N increases. I am not sure that N is so large in the case of orientation data. In any case, Monte Carlo methods can sample from any distribution, not necessarily Gaussian.

Author's answer:

Numerous sources of uncertainty affect structural measurements among which

- device basic measurement error (in lab and under perfect conditions)
- user error
- local variability
- simplification radius (when several nearby measurements are grouped as one for practical reasons)
- miss-calibration issues
- rounding errors
- (re)projection issues
- magnetic perturbations emanating from the sun, infrastructures, rocks, vehicles, the measurement device itself, whatever the operator is carrying
- GPS related issues (numerous small issues)

If one abstracts each source of uncertainty to a uniformly distributed random variable (the worst case), a quick look at the Irwin-Hall distribution shows how fast the addition of these variables converges to normality: 4 added variables produce an already very convincing near normal shape. Regardless of this argument, MCUE does not a priori forbid the use of any kind of distribution.

Changes to the paper:

…1954) ~~as~~ [*if*] the variance of each source of uncertainty is always defined ~~even if it is unknown~~. Uncertainty [*would*] ~~will~~ then be [*better*] ~~best~~ represented…

Page 4, line 20:

G. Gaumon's comment:

Without any hypothesis on the type of the likelihood function? Please add supporting reference(s).

Author's answer:

If the likelihood function is Gaussian itself.

Changes to the paper:

…framework [*given that the likelihood function is normal itself.*]

Page 4, line 23:

G. Gaumon's comment:

The footnote makes the notation quite crypic.

Author's answer:

Arg.

Changes to the paper:

Page 4, line 24:

G. Gaumon's comment:

Under some assumption, I guess. Not sure why this precision is important here anyway.

Author's answer:

If the likelihood function is Gaussian itself. This is of importance because it greatly simplifies the procedure (RNG sampling for the vMF distribution is much easier than for other spherical distributions).

Changes to the paper:

…itself [*given that the likelihood function is vMF distributed.*]

Page 5, line 20:

G. Gaumon's comment:

It would be nice to explain:

- what you mean by Bayesian approach in this context (what prior distribution would be updated by what observation).

- In what sense that is "optimal".

As I understand, this is an introduction to the development below; then, this would help the reader to phrase it as an intro: "We now propose to....", and keep the comment on the optimality for the discussion.

Author's answer:

The prior disturbance distribution (obtained from metrological analysis) is updated by the measured data over a CLT compatible likelihood function.

Changes to the paper:

…parameterization is [*proposed*] ~~optimal (Sivia and Skilling 2006)~~. [*More specifically, a prior disturbance distribution is updated by measurements over a CLT compatible likelihood function to generate a predictive posterior disturbance distribution.*]

Page 5, line 21:

G. Gaumon's comment:

Not sure whether this precision is needed.

Author's answer:

It isn't.

Changes to the paper:

…distributions ~~for MCUE models~~.

Page 5, line 25:

G. Gaumon's comment:

Do you mean all measured data or repeated measured data at the same location? Please explicitly state what n means.

Author's answer:

I mean repeated measurements at the same location.

Changes to the paper:

…data [*at a single location*] are…

Page 6, line 2:

G. Gaumon's comment:

OK, but it is unclear to me why the prior average distribution should depend on the prior dispersion distribution.

Author's answer:

This is a mistake; the relationship is either reversed (heteroscedasticity) or nonexistent (homoscedasticity).

Changes to the paper:

…expected to be [*a deterministic function*] estimated ~~based on~~ [*via*] rigorous…

Equation 5

$$\cancel{p(\mu|X,\vartheta) = \frac{p(X|\mu,\vartheta)p(\mu|\vartheta)}{p(X|\vartheta)} \propto p(X|\mu,\vartheta)p(\mu|\vartheta),}$$

$$p(\mu|X,\vartheta) \propto p(X|\mu,\vartheta)p(\mu,\vartheta),$$

Equation 6

$$\cancel{p(\mu|X) = \frac{p(X|\mu)p(\mu)}{p(X)} \propto p(X|\mu)p(\mu).}$$

$$p(\mu|X) \propto p(X|\mu)p(\mu),$$

Page 6, line 6:

G. Gaumon's comment:

I am willing to admit that the pdf of the dispersion of another pdf could be an overkill, but is it reasonable to remove the dispersion in this likelihood term?

Author's answer:

Prior dispersion is expected to be a deterministic function of the measured values themselves obtained from previous metrological studies (see above answer). In a sense the term is merely "hidden" for legibility because there is indeed

Changes to the paper:

See comment above.

Page 6, line 7:

G. Gaumon's comment:

You could more simply explain this is corresponds to the uniform distribution.

Author's answer:

Not exactly, a continuous uniform distribution must be bounded otherwise it will not integrate to unity. Jeffreys prior is not a proper distribution of any kind but rather a normalized constant that aims to simulate a complete lack of knowledge at the prior step. In this case Jeffreys prior is similar to $f(x) = k$, with $k \equiv \text{cst}$, therefore, $\lim_{z\to\infty}\left(\int_{-z}^{z} f(x)\right) = \infty$, making it an improper prior.

Changes to the paper:

None.

Page 6, line 16:

G. Gaumon's comment:

Assumes independent samples.

Author's answer:

Correct.

Changes to the paper:

…and, [*under the assumption of independent by computing,*] ~~is given for all possible values of μ, is obtained with~~ the joint density function for *X*.

Page 6, line 24:

G. Gaumon's comment:

What does N stand for?

Author's answer:

p^N stands for the posterior predictive distribution.

Changes to the paper:

…predictive distribution [p_{N}] is…

Page 6, line 26:

G. Gaumon's comment:

Unclear to me. Prior knowledge, in general, is not very reliable.

Author's answer:

Not all forms of prior knowledge are unreliable. However, we usually remove the "reliable" terms from the equations because they are either constants or deterministic functions. In this instance, σ is supposed to be extracted from a deterministic error function itself obtained via metrological analysis.

Changes to the paper:

…knowledge [*obtained via metrological analysis*].

Page 8, line 8:

G. Gaumon's comment:

observed or assumed ?

Author's answer:

Provided that instrumentation is properly deployed, maintained and free of external noise gravimeters' measurement error function is homoscedastic. However, this is rarely the case and a power law error functions are more common in practical cases.

Changes to the paper:

…commonly ~~observed~~ assumed in gravity surveys…

Page 8, line 13:

G. Gaumon's comment:

This term encompasses the following ones in the enumeration.

Author's answer:

Correct

Changes to the paper:

…observed in ~~physical modeling (Ogarko and Luding 2012),~~ electrical…

Page 10, line 3:

G. Gaumon's comment:

Please explain what they represent debore commenting.

Author's answer:

Done, the following changes address the next 3 comments.

Changes to the paper:

Blue clusters [*are the direct result of pole vector sampling and*] always describes the plane's behavior accurately in terms of pole vectors.~~; they are the direct result of pole sampling~~. Green clusters [*are the result of pole vector sampling (blue) converted back to dip vector and they describe the plane's behavior accurately in terms of dip vectors. These clusters*] have varying shapes and may not be modelled appropriately by any existing spherical distribution for all possible cases.~~; they are the result of pole sampling converted back to dips and they describe the plane's behavior accurately in terms of dip vectors~~. Red clusters ~~have constant point density and are isotropic; they~~ are the [*direct*] result of dip [*vector*]sampling and fail to describe accurately the plane's behavior.

Page 10, line 5:

G. Gaumon's comment:

Dip angles or dip vectors?

Author's answer:

Dip vectors.

Changes to the paper:

See comment Page 10, line 3.

Page 10, line 6:

G. Gaumon's comment:

Dip angle sampling, right? Please say a word about how this is done?

Author's answer:

No, the dip vectors were always sampled from a spherical distribution either directly (red clusters) or indirectly (green clusters) using pole conversion (blue clusters).

Changes to the paper:

See comment Page 10, line 3.

Page 10, line 18:

G. Gaumon's comment:

Do you mean a dip angle? A vector in the sphere should be described by a spherical distribution.

Author's answer:

Indeed, we are talking about a dip vector.

Changes to the paper:

See comment Page 10, line 3.

Page 10, line 23:

G. Gaumon's comment:

Not the dip direction.

Author's answer:

Correct.

Changes to the paper:

…Fig. 6) [*as standard dip angles are*] ~~the dip, dip-direction system is~~ constrained…

Page 10, line 28:

G. Gaumon's comment:

dip/ dip direction angles?

Author's answer:

A dip angle + a dip direction makes a dip vector.

Changes to the paper:

…vectors [*using the dip, dip-direction system*] (green…

Page 11, line 16:

G. Gaumon's comment:

Unit ?

Author's answer:

Meters

Changes to the paper:

…25[*m*]. The…

Page 11, line 19-20:

G. Gaumon's comment:

Unclear what this means in practice. Please develop how this ties to Section 2. Some more information (here or in Appendix) would be really useful for practitioners.

Author's answer:

We have estimated values for the dispersion of orientations on the basis of the variability of plane measurements observed by other authors in a variety of settings and for different types of devices. Ideally, an MCUE user would need in depth metrological data relevant to the particulars of the survey from which the structural data comes from. For our specifics, the literature is quite poor on this matter. However, several authors have picked up on these gaps and started working on it very recently.

Changes to the paper:

…data ~~(Nelson et al. 1987; Stigsson 2016)~~[*That is values for the dispersion of the spherical disturbance distributions used for the foliations were estimated on the basis of the variability of plane measurements observed by other authors (Nelson et al. 1987; Stigsson 2016; Allmendiger et al. 2017; Cawood et al. 2017; Novakova et al. 2017) in a variety of settings and for different types of devices.*] ~~while~~…

Page 11, line 21:

G. Gaumon’s comment:

Please explain how this was done also. Lark et al use several interpretations by several geologists. How did you do this on the Mansfield model?

Author’s answer:

We assumed that the observed end variability of the interfaces’ locations in their models can be transposed to our case. This is of course an imperfect process and actual metrological studies would be needed to improve it. It is important to keep in mind that MCUE depends on sound metrological analyses.

Changes to the paper:

…~~while~~ Perturbation parameters for interfaces were designed to meet [*observed GPS uncertainty (Jennings et al. 2010) and*] observed experimental interface variability in previous authors’ works (Courrioux et al. 2015; Lark et al. 2014; Lark et al. 2013). [*More specifically it was assumed that the observed end variability of the interfaces’ locations in their models can be transposed to the presented cases. This is of course an approximation in the absence of specific metrological studies.*] ~~observed GPS uncertainty (Jennings et al. 2010)~~…

Page 12, line 2-3

G. Gaumon's comment:

I don't agree.

Author's answer:

Claim is weakened.

Changes to the paper:

As described in sect. 3.1, CLT distributions [*can be appropriate options*] ~~should be preferred~~ as prior uncertainty distributions (and disturbance distributions) because they ~~better~~ [*generally well*] describe the behavior of uncertainty.

Page 12, line 6-8:

G. Gaumon's comment:

THis is not very explanatory. Unclear to me. More explanations would be welcome.

Author's answer:

This happens when the variable of interest is given by

- the log of the quotient of two uniform i.i.d. variables
- the difference of two exponential i.i.d. variables

Both of which lead to a symmetric, long tailed, exponential distribution: the Laplace distribution. This might sound unlikely for geological structural data inputs in 3D geological modeling. However, when one considers measured thicknesses on a geological log from a drillcore (which are used as data input in implicit codes) it becomes a serious possibility.

Changes to the paper:

…1923). [*For example, to model the uncertainty on the thickness of a geological unit along a drillcore, one might observe that the uncertainty of the location of the top and bottom interface of the unit is best represented by an exponential distribution. In this instance, the Laplace distribution would be a suitable option to model the thickness' uncertainty.*] ~~That is, the Laplace distribution "replaces" the normal distribution.~~ Under [*similar*] ~~the same~~ circumstances…

Page 13, line 3:

G. Gaumon's comment:

I think dip vector sampling is not the same as dip/dip direction sampling. this should be clarified.

Author's answer:

Here we are really talking about dip vector sampling. Sampling independently for dip angles and dip directions is not the topic of this paper. However, the conclusion drawn here would show that this kind of sampling is needlessly difficult because of the added heteroscedasticity that originates from using dip vectors (that dip angles and dip direction angles describe).

Changes to the paper:

Vectors are now called vectors where due.

Page 13, line 28:

G. Gaumon's comment:

I still don't agree. Metrological considerations could yield other distributions, depending on the measurement hardware.

Author's answer:

This is correct, our claims need to be weakened.

Changes to the paper:

… to ~~always be more optimal choices for~~ [*be valid and practical choices*] for…

Page 13, line 30:

G. Gaumon's comment:

I am not sure I understand what is meant here, and how this point comes into play in the numerical experiments.

Author's answer:

The use of predictive posterior distribution as disturbance distributions for MCUE means that dispersion is not underevaluated. That is possible because of the application of Bayes' theorem. In theory, it is one can obtain the same result with a frequentist approach using compound distributions although it is much more difficult to express in a legible manner and clunky to use.

Changes to the paper:

[*A Bayesian approach to disturbance distribution parameterization*] is shown to avoid an underestimation of [*input data*] dispersion.

References

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