Log of scientific email exchanges between co-authors and other colleagues: December 2018 – February 2019

Supporting Information S2 for

Assessing minimum pyroclastic density current mass to impact critical infrastructures: example from Aso Caldera (Japan)

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Note: the knowledge exchanges recorded below involved five panel members concerned with assessing the probability of an Aso4-scale future eruption and four other colleagues providing detailed volcanological support. All names have been anonymized.

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28 December 2018
EXPERT A:
I went through Dade and Huppert (1998). Their equation for the area inundated by a flow/avalanche is:
A = lambda^1/3 (gMH/tau)^2/3

where A is the area inundated by the flow, lambda is the ratio of width to length of the deposit (lambda = 1 for an axisymmetric flow), g is gravity, M is flow mass, H is flow release height (or total fall height) and tau is the average resistance stress. They give a very wide range: 1 kPa < tau < 100 kPa, with tau being much smaller for highly mobile flows.

I just recast this as the minimum volume required to reach the TS1.
V(min) = tau (pi * r^2)^1.5/(rho g H)
where r is the distance from the caldera to the TS1 (130,000m), rho is the density of the deposit (700-1000 kg/m3), and H is the release height (< 10,000 m). The basic result is that the minimum volume needed to reach the TS1 is highly dependent on the average resistance stress, which must be very low. For example, using H = 10,000m, lambda = 1, rho = 1000 kg/m3, and tau = 1 kPa (lowest, most mobile value considered by Dade and Huppert), then V(min) = 100 km3, about.

I would say this is a reasonable result, but means the uncertainty is driven mostly by uncertainty in the value of tau.

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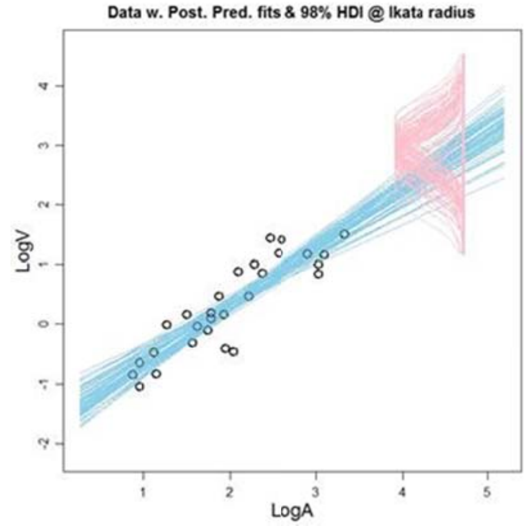
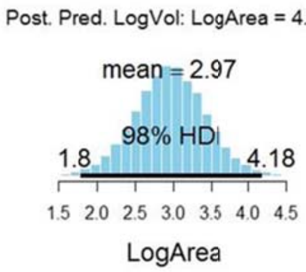
30 December
EXPERT B:
Reverting to Dade & Huppert (1998), I have extracted the “volcanics” data points from their Fig 4: Log Area -v- LogVol. Then, inverting these to regress LogVol in terms of LogArea, so that LogVol can be estimated for any given LogArea, I use these data to compute Bayesian posterior predictive fits to the data (e.g. 75 plausible fits, given these data, are shown on the next plot, for illustration). This plot also shows the corresponding posterior predictive LogVol spreads for the case of LogArea = 4.725 (i.e. radius 130 km to TS1). This involves extrapolation well beyond the range of cases in D&H Fig 4 data:

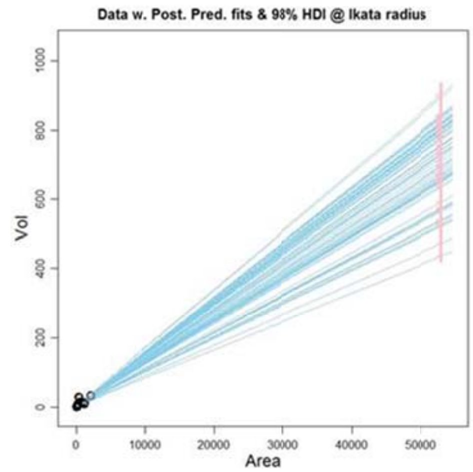


Pooling the Posterior Predictive LogVol fit spreads for an (Log) area with 130 km radius, we obtain a combined Log Volume estimator distribution:



… indicating a mean required erupted Volume = 933 km^3 to reach 130 km radius, with 1%ile volume = 60 km^3 and 99%ile ~ 15,000 km^3. In other words, according to this analysis there is a 1% chance an erupted volume of 60 km^3 could reach TS1, while the expected required volume -- given the D&H data -- is about 1000 km^3. Volcanologically, the probability of an eruption of order 1000 km^3 in the next 100 years would seem to be just not credible; Expert A's Vmin estimate of about 100 km^3, from his reduced version of D&H Eqn 5, is thus at about the 1 - 2 % probability level <u>on the basis of a Bayesian regression of the volcanic flows data that D&H relied on</u>.

!!!! LOGARITHMIC HEALTH WARNING !!!!
To put this regression of Dade & Huppert logarithmic data into a truer perspective, if the data are represented not by log values but by their linear values (in km^2 and km^3), then the extent of the extrapolation beyond the range of those data becomes obvious and, plainly, is unjustifiable:



Whilst, potentially, this is instructive background/contextual material, it seems clear we need to develop the Aso-specific box model approach …..

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30 December
EXPERT A:
I attach my revised Dade and Huppert calc including lambda (note that lambda varies from 0 to pi, not from 0 to 2pi...this confused me on the first round). I like the idea of eliciting ranges for the parameters in the model.

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31 December
EXPERT B:
What do you think to a variant of your calculation, expressed in terms of an elliptical flow area with the vent at one focus? Of course, an elliptical footprint can be tested for a range of aspect ratios.

Whilst the results may not be very different, numerically, it seems to me it would be more plausible, volcanologically. A narrow sector of a circle makes physical sense for, say, a topographically controlled dome release PDC, or for debris avalanches emanating from ground level sources, as in Dade & Huppert's case histories. But this seems less realistic for the dispersal of a PDC launched from a column at a goodly height, well above any proximal controlling topography.

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2 January 2019
EXPERT B:
Attached please find a very brief elicitation protocol for quantifying uncertainty distributions for input parameters for Expert A's Aso-4 Minimum Volume calculation (details are included in the second worksheet of the attached workbook). There is also a brief preamble sheet, reminding colleagues of the uncertainty elicitation concept and format.

There are just four query items to complete (third worksheet). When complete, please save the file with your name and initials, unless you are Donald Trump, in which case I've done it for you ☺

The plan is to combine your responses with equal weights (no calibration), and to re-calculate the Minimum Volume equation with Monte Carlo sampling to determine volumes that might reach TS1 probabilistically.

If you have any questions, it would be great to learn of these asap, so everyone can sing to the same tune. Could we aim to have responses back by 7 January, please (I will be hors de combat for a few days thereafter).

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3 January
EXPERT C:
I am very happy to follow your email discussion. It's very interesting, thank you. On my side, I have two observations:

1) The "box model" equations that we implemented in Pisa were inspired by Dade&Huppert1995 (https://doi.org/10.1029/95JC01917), and it is not the same model in Dade&Hupper1998. Originally focused on turbidity currents rather than on rockfalls, the '95 model is further detailed in Hallworth-Hogg-Huppert1998. In my opinion, the main difference is the presence of a particle deposition mechanism in its formulation, which links the runout properties to the particle-size and volume fraction of the flow, rather than on a constant stress tau. We believed that particle deposition was very relevant in CF flows dynamics. Not really sure about Aso-4. But I wanted to make clear that we are not speaking about the same Dade&Huppert paper.

   As a consequence of that, assuming axisymmetric geometry, I could also provide my (different) relations between invaded area and volume, depending on particle size instead than on tau. The original implementation in my PhD thesis and then used in Neri, Bevilacqua et al., 2015 (JGR), and then in Bevilacqua, Neri et al., 2017 (Frontiers) assumed a single particle size and we included some sensitivity analysis in the appendix of the former paper. Ongaro et al. 2016 (JVGR) generalized the model to a finite number of particle sizes, but we never implemented such version in any hazard assessment yet.

   Of course I can easily modify my box model implementation to use the '98 equations, if we believe that they are more appropriate in this case. What would change is that in one case we have to infer tau, in the other case instead the particle size and volume fraction.

2) Concerning your question about footprint shape of Aso-4, Shinji Takarada provided me an idea of what he thought was that shape, sharing with me a picture of the deposit footprint reconstructed on the field. Unfortunately, he also expressly told me to not show/use the picture in this study. However, it is almost axisymmetric, according to him.

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3 January
EXPERT G:
Had a preliminary look at EXPERT A's outline and the questions. I need more information and background to understand the approach.
On Expert A's calculations I would not clear why we need to elicit the radial sector. It seems to me that these kind of flows are axisymmetric so spread in all directions, so why do we need to elicit lambda at all? The theory would be better with lambda as a constant (2pi). I didn't really understand the basis of the minimum volume of 138 km3. Why is volume (mass) not an elicited parameter? I don't understand what density is being elicited. In the Dade and Huppert the density refers to the deposits density which might as well be a constant. However it seems to be the flow density that is being elicited? Given that these flows are highly density stratified (see papers I sent) then what is the meaningful density required? Density of collapsing column?
One of the issues for this assessment is that it's likely that most of the ignimbrites are multiple flow events. That's why it would be a very helpful to calibrate using the Taupo where we are fairly sure it's one main flow

event. As the Roche et al paper indicates it's also more likely to be a sustained source flux rather than a single volume related instantaneously.

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3 January
EXPERT A:
If the pdc is axisymmetric, lambda = pi (not 2 pi), since the way D and H defined lambda, A = L^2 * lambda (not A = L^2 theta/2).

Density is the deposit density, which is assumed to be constant (M = density * volume). It's not implied this deposit density would need a wide variation, but it is needed to calculate the minimum volume (wouldn't be needed to calculate the minimum mass, but everyone estimates deposit volumes.

The formula developed by D and H is for an instantaneous release (based on potential energy). The idea is this gives the minimum volume, since multiple flows will tend to add volume without necessarily increasing run-out. In other words, why worry about the total eruption volume if we are interested in the minimum volume required for the pdc to reach the TS1? Tau, for example, is not the mean value of all values of tau, but the values of tau that result in flows that reach the TS1. Similarly, lambda is not the range of all flows, as you point out, but the range of lambda for flows that can reach the TS1.

This is one way to calculate the minimum volume - there are several others. We could enumerate the others and their parameters into a logic tree, weighting alternative models. Some might be:

1. use analog deposits to define a minimum volume required to reach 130 km from the volcano
2. define a thinning rate of the deposit (using analogs or physics-based models) and use this
3. use a cellular automata approach to account for actual topography (still need information from (2, above)
4. revisit the Bevilacqua approach (from the paper Neri et al.) which I think is to set their Lmax to 130 km and investigate the parameter variation, as we are doing with the Dade and Huppert paper.

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3 January
EXPERT G:
Somewhat clearer, but I still don't understand some key aspects:

1. I would keep lambda constant pi. I don't understand why one would want to vary lambda is its axisymmetric.
2. Why do you use one particular value of tau (2000) in your minimum volume calculate. Why not elicit tau and calculate the volume needed to get to 130 km?
3. Don't we need to elicit volumes for individual Aso so that we find what is the conditional probability of getting a flow to 130 km or more?

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3 January
EXPERT A:
1. If it is axisymmetric lambda does not vary - it is equal to pi. Originally, I only formulated for axisymmetric flows, but Expert B wanted lambda back in. I suppose there is some chance of non-axisymmetric flows and the question is "what is that chance?"

2. The codes simply show example calculations (e.g., tau=2000 Pa). Tau is elicited (ave. resistance parameter) and it is definitely an uncertain parameter!

3. Agreed. I think there are two separate questions. A. what is the minimum volume of flow that can potentially reach the TS1, and B. what is the probability of a flow of this minimum volume occurring. So we have to get to both questions.

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3 January
EXPERT G:
Thanks Expert A it's all coming into focus. I would not vary lambda (=pi). Don't see the point in eliciting it as there is no obvious reason for asymmetry in these large events. I can now see that the elicitation as posed by Expert B gets us a range of minimum volumes to get to 130 km (or more). I don't see in Expert B's elicitation where the probability of a particular volume comes in.

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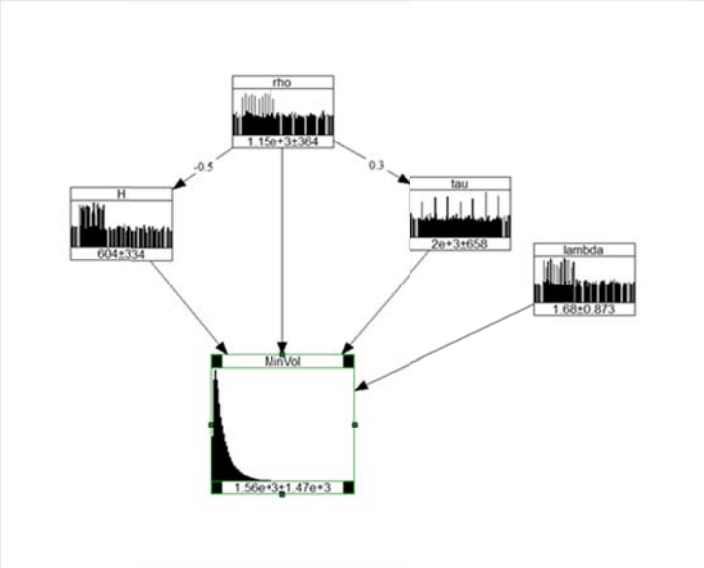
3 January
EXPERT B:
Here's the BBN scheme for calculating MinVol probabilistically, using Expert A's formulation.

I have entered placeholder uncertainty distributions for H, rho, tau and lambda (and also a couple of paired correlations: H on rho; tau on rho, for illustration). The functional node MinVol performs Expert A's equation calculation, using 50k samples from the variable nodes, and creates an uncertainty distribution for the equation solutions over these samples.

The corresponding means and std devs are shown below each node: for these dummy numbers, the MinVol 5th %ile = 235 km3; 50th %ile = 1102 km3 and 95th %ile = 4419 km3 (n.b. I am using a much smaller range for H than Expert A used: here, 100 m to 1300 m, hence larger MinVol is needed to reach 130 km).

The idea is to elicit the three (or four) indicator variables from the panel, and pool the responses to use in the BBN. With 50k samples (or more, if needed), we can extract any percentile of interest, e.g. with these placeholder numbers the 1st %ile MinVol is about 127 km3, while the 0.1 %ile MinVol is 68 km3. The latter is a more likely eruption scenario than one with a much larger volume, but its probability of reaching TS1 is much smaller, as indicated by the distribution.

Once we get a quorum of elicited values, I will re-run the calculation and circulate the results for comment/criticism.



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3 January
EXPERT G:
Not sure how helpful or accurate this is but I think we can at least get a sense of the likely values of tau from a rather rough and ready analysis. Tau is likely related in some way to column friction is a granular media. Its the case that large ignimbrite have depositional slopes typically between 1 and 2 degrees and I see this as a rough estimate of Coulomb friction. If we know the near final flow thickness and we can estimate a friction coefficient from the normal stress to shear stress ratio. For 1 to 2 degrees I get a friction coefficient that then enables me to calculate a resists shear stress. I get values of about 500-3000 Pa for a flow of 10 metres thickness (which is about Taupo average). One can envisage the flow thinning until the shear stress due to the flow thickness becomes comparable to the resistance and the flow stops when it gets thinner. Alternatively, for Taupo where I know run-out (80 km) and volume (15 km3) quite well I can calculate the value of tau for the observed volume and runout and for a column height of 5 km and get 3700 Pa. These are extremely rough and ready estimates but I think these might help us pin down the range of tau in an elicitation. Thus the value of tau used in Expert As illustrative calculation seems very plausible.

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3 January
EXPERT B:
Just to further illustrate the analytical capability of the UNINET BBN program, here are two additional demo plots (remember the parameter values are just examples).

The first shows the effect on the calculated MinVol distribution if tau is conditionalized (i.e. set) to the single value tau = 1500.
Now the 5th, 50th and 95th percentiles for MinVol are 213 km3; 891 km3; 3084 km3 (they were: 235 km3; 1102 km3 and = 4419 km3 in the unconditionalized run, when tau could take a range of values).

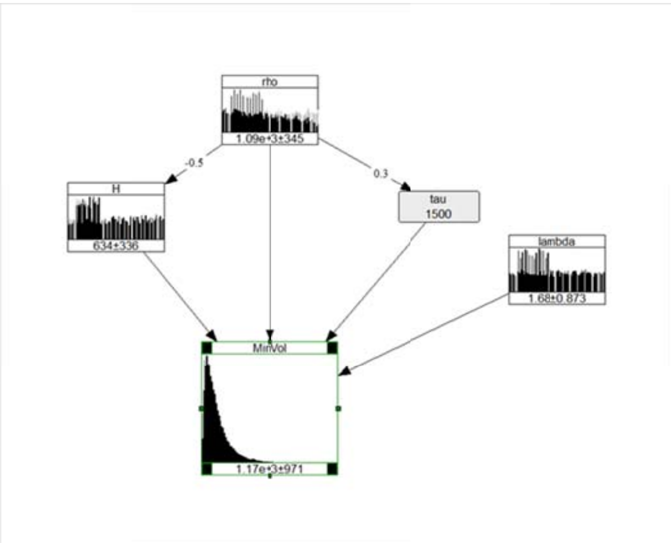
The second plot shows what happens if tau is set to 1500 <u>and</u> H is conditionalized to 1200 m. In this case, the 5th 50th and 95th percentile MinVol values are 144 km3; 524 km3; 1274 km3 respectively.

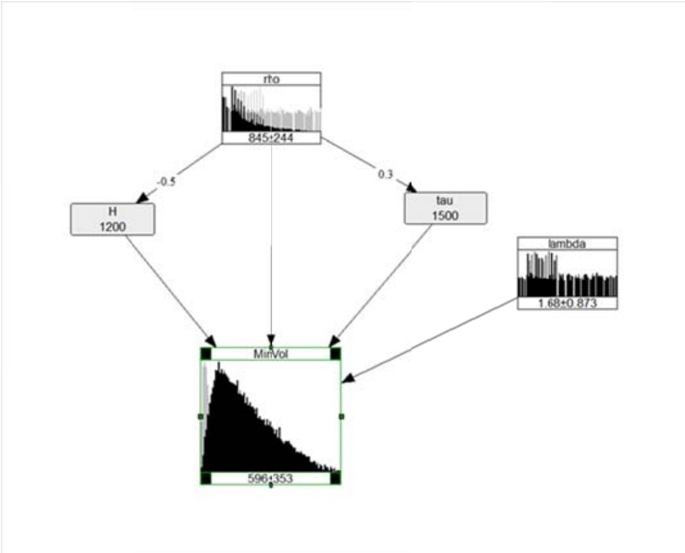
Although difficult to spot, the previous MinVol distribution, prior to conditionalizing on tau and H, is shown also in light grey, in the background of the MinVol histogram panel of the second plot. It is also shown on the first plot, but is very difficult to discern as the distribution is little changed when conditionalized on tau = 1500 alone. This demonstrates that the D&H equation is more sensitive to variations in H, as that is raised to the third power, whereas tau is linear. In this simple case, such influence is obvious from the equation; however, in more complex problems, such dependencies may be less obvious, but UNINET has ways of searching them out, which can be very instructive (this is what Thea Hincks did to discover which factors were controlling induced seismicity in Oklahoma for our 2018 Science paper).

But note, too, that the prior uncertainty distribution for rho is also modified when the other nodes are conditionalized (the posterior black distribution replaces prior grey in the rho node panel) -- under Bayes rule, these conditionalizations of H and tau add information about the uncertainty associated with rho, via their correlation arcs. Note, too these conditionalizations do not change lambda, as that variable is independent of the others.

This updating of the uncertainty distribution for one random variable by Bayes inference, using information from related variable(s), is, in effect, a more formal numerical way of performing Expert G's deduction of feasible parameter values for the Taupo case, but allows easy exploration of various different combinations, with the relevant uncertainties.

These are very powerful UNINET capabilities for diagnosing the influence on results of different scenarios of parameter combinations (not readily available in other software packages), and just await our elicited judgments ☺





4 January
EXPERT G:
Expert B - This looks like a very interesting set of results. I am still not convinced that we should elicit lambda; can you give a rationalisation for doing this? I would just keep it's as a constant (pi). On column height I attach my 1978 paper which shows a range of estimated columns collapse heights as functions of vent conditions (see Figure 2). I suggest that such a figure could be used to inform the range of H values. There has been a lot of subsequent research on collapsing column which Expert F, Expert H and Expert C will be familiar with and maybe they can point us to some updated plots of how collapse height varies with discharge conditions from more sophisticated models.

I have been thinking a little more about the friction. For Taupo one gets an average thickness of the deposit of about 1 m which would give about 370 Pa but this must be at the lower limit as it assumes the deposit is uniformly distributed which is clearly not the case. It's worth commenting that by characterising a flow by a single tau we are greatly oversimplifying the actual physics where the friction will vary as functions of particle concentration, size, pore pressure etc., height in flow etc. However, I think its reasonable (and maybe INGV colleagues can comment) to think that as the flow thins and mass in transferred to the basal concentrated parts of the flow granular friction will come to dominate in the late stages of flow; this is likely what the Roche et al. Nature Comms model is capturing. I rather like their model although I think their velocity estimates are pertinent to the basal concentrated region while the overlying turbulent regions can go much faster.

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4 January
EXPERT B:
Expert G, many thanks for this, and for the Roche paper which had slipped me by.
With UNINET we can fix lambda to pi() very easily, but also investigate what alternative values might imply.

For the friction parameter tau, if our experts offer ranges of values then the equal weights pooling will provide a distribution for tau, and the calculation is then not tied to any single value. Again, we can test for individual values, if that is of interest.

From my demo model, it is clear that column height is the major factor influencing runout estimation on the basis of this particular equation.

As Expert A said in one of his emails, the Dade & Huppert equation approach could be one branch of a logic tree, balanced - with expert weights - against other methods or analyses. To be comprehensive for Aso, I think we should aim to develop such a tree.

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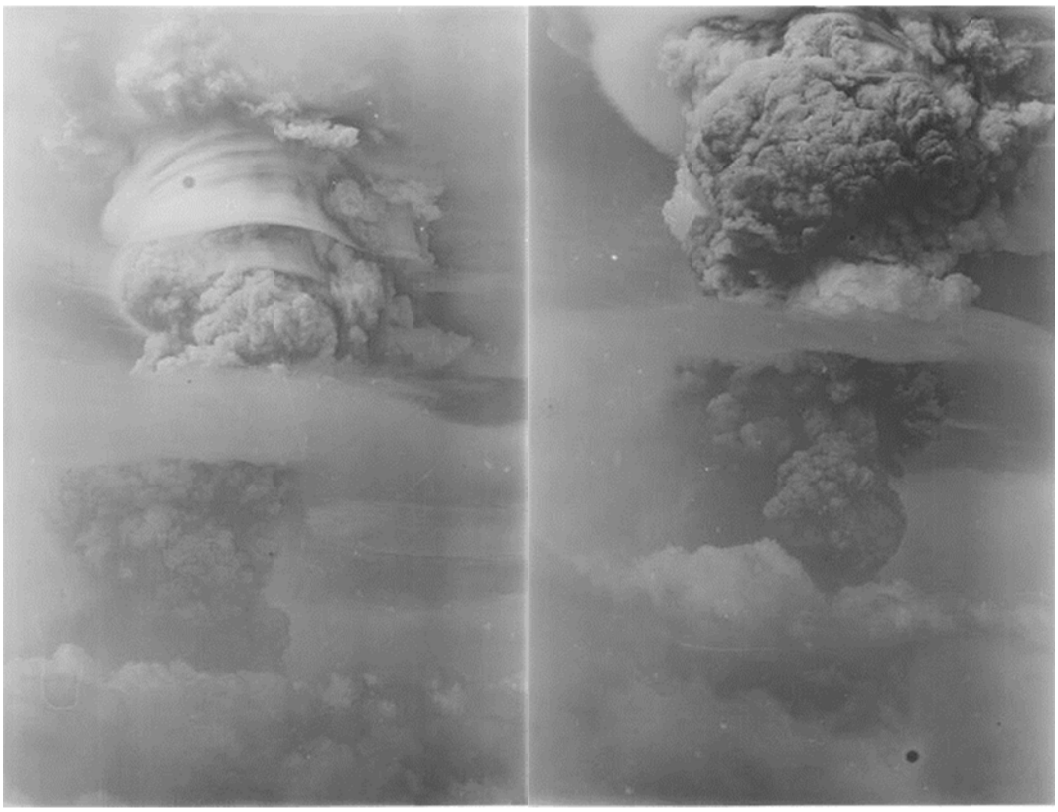
4 January
EXPERT B:
Expert G, The Oruanui PDC deposits (Wilson, 2001: Fig 8) and the Peach Spring Tuff deposits (Roche et al, 2016: Fig 1) appear to evince some directional ellipticity, even allowing for topo influences in the latter case. Do we have an explanation for this?

I am tempted to take us back to 1979 and St Vincent: for some of the explosion columns that could be adequately viewed without obscuring weather cloud, I was struck, at the time, by the evident vorticity in the rising columns (see photos from 17 April explosion). While I may be under-informed, is this a dynamic process that has received attention from volcanology?

Could it be that rotational shearing forces, in a much bigger, higher column, could spin off PDC material in a directed manner, at some critical function of rotation speed and column density, as appears to be developing in the upper part of the RH photo?

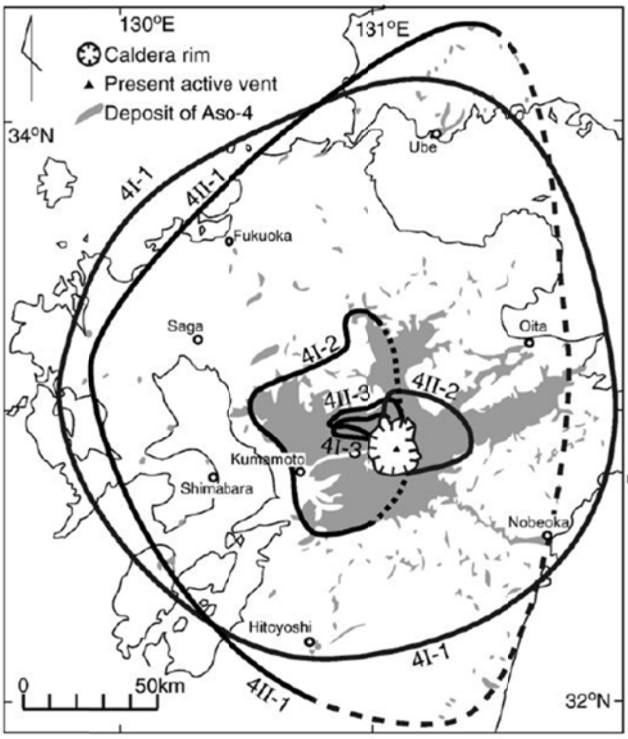


4 January
EXPERT E:
I've been following the fast and furious discussions regarding our implementation of the D&H model, and appreciate the insights as I endeavour to develop my model parameterizations independently. I did, however, want to share a perspective on the lambda discussion.

I would prefer to keep lambda as an elicited parameter, to allow for empirical interpretations of the Aso 4I-1 distribution. As shown in Kaneko et al's (2007) figure 1 map (attached), Aso 4I-1 is not strictly axisymmetric. If it were centered more or less uniformly on Aso caldera, distribution would only have extended about 100 km and, thus, we wouldn't need to ponder travel 130 km to the TS1 TS1.

As an additional perspective, lambda also can empirically represent the effects on PDC distribution caused by eruption from multiple/broadly spaced vents, as opposed to a point-source release.

Although I don't believe that we need to derive a sound theoretical basis for lambda, I favour incorporating an empirical function that captures potential eccentricities in deposit distribution like those observed at Aso 4I-1 and elsewhere. We can easily bring our judgments to bear on what sort of a distribution lambda should follow to account for such potential deposit eccentricities.



4 January
EXPERT B:
Useful inputs to the debate, as always!

I should add to my earlier emails on the BBN approach that, in our Aso minimum volume case, the elicitation shouldn't be regarded as a one-off only exercise - once the first results are produced, I anticipate one or two further iterations while colleagues digest the initial findings and then, if they so desire, modify their preliminary judgments in the light of the emerging arguments, information and critiques.

One appreciates that, in a strict scientific research sense, an iterative approach like this can seem biased, subjective or even invalid, but arguments are emerging elsewhere with Earth sciences hazards that this represents a rational way of reasoning under uncertainty and constraining judgments in the context of hazard/risk assessment.

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05 January
EXPERT F:
Just a couple of thoughts, not sure if appropriate.
I like the idea to use more than one model. The D&H98 models seems more appropriate for dense granular flows whereas the D&H95 (i.e. box-model) is more appropriate for dilute turbulent flows. As Expert G already mentioned it is likely that the two types of flows coexist in the real current also interacting and exchanging mass to each other. Therefore, they could be seen as two end-member cases to be properly considered and evaluated. Personally, and this is the hypothesis we were working on with Shinji about Aso-4, I think the dilute current is likely the more mobile one, mostly on such quasi-flat topography.
PS: be careful in comparing volumes between the two models since the original density of the collapsing volumes is very different in the two cases (one is dense the other has just few percent of solid volume fraction). We should compare the volume of solid involved in the two cases to make a reasonable comparison.

Re collapse height what we basically found from multidimensional simulations is that for fully collapsing columns a good proxy of it is $Hc=v^2/2g$ where v is the exit velocity at the vent. For more dilute "transitional" columns Hc can be somewhat larger than that although the density of the collapsing stream is in this case lower and therefore this effect counterbalance the larger collapse height. In general, I would say that the range of values reported (up to max values of 5-7 km) in the 1978 paper by Expert G and coauthors is very reasonable.

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06 January
EXPERT G:
I concur with most of Expert F's comments. In particular, the most distal ultimate runout is likely determined by the residual upper turbulent part of the flow which will progressively lose mass to the dense lower part of the flow and then into the deposit as the flow advances. Since friction varies greatly between the different parts of the flow our frictional parameter (tau) is a very approximate averaging. Also the model we are developing does not consider runout limited by buoyant lift off where the distal very dilute part of the flow becomes less dense than the atmosphere. One point which I don't agree with is the there is a difference in the density of the collapsing column as I don' think there is a difference. The collapsing columns will be largely dilute and the dense region forms from the flow as its transitions from collapse into the lateral flow.

I still don't wholly follow the lambda argument and how this will capture asymmetry. I think Peach Springs may be asymmetric largely due to basin and range extensional tectonics. I would be wary at asymmetric distributions for a deposits as old as Aso-4 due to the geological factors that affect deposit preservation.

07 January
EXPERT F:
About the mixture density I wanted just point out that in the application of the box model the volume Vo that collapses refer to the volume of the dilute mixture that collapse from the column. So this is different from the volume of a dense rockfall used in the D&H98 model and therefore we should compare the two volumes with the same density. I hoped I clarified the point. I agree on the other points you mentioned.

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7 January
EXPERT B:
Expert A's note indicates he used Dade & Huppert (1998), and stated rho is "the density of the deposit".

In D&H (1998) it says: "The overall runout of a volume of rock with mass M and bulk density ρ that is initially mobilized by one of a number of possible trigger mechanisms and that spreads under gravity g with speed U …". In his note, to solve for minimum volume for a given runout length L, Expert A substitutes ρV for M, where V is the volume of the deposit.

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7 January
EXPERT H:
Thanks for the clarification. However, we need to keep in mind that deposit density is not a constant, neither in space nor, especially, in time. So if we refer to the fresh rather than compacted deposit volume should be considered in accord to it. I agree with Expert G that, in the framework of Dade and Huppert simplified model, it is not very worth eliciting lambda (there are more important quantities as source of uncertainty!).

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7 January
EXPERT A:
The density in the elicitation definitely relates to deposit density. It is only used to convert total eruption mass to a volume. It has no dynamical role in this case.

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7 January
EXPERT H:
Hence to the current compacted deposit density? Fresh deposits can have much lower density with respect consolidated one (even a factor two sometimes). It's important to clarify to what we refer otherwise anyone will choose values that are apparently very different.

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7 January
EXPERT A:
Yes - current compacted deposit density. The reason I say this is that all of the volume estimates are based on the current deposit. Therefore, we should use this volume and the current compacted density to estimate the total eruption mass.

07 January
EXPERT B:
Does Expert A's clarification to the density issue mean the elicitation question should have read: "Density of deposit [kg/m^3]". Please let me know if anyone wishes to change their three quantile values for this item (i.e. 5^{th}, 50^{th} and 95^{th} percentiles).

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07 January
EXPERT C:
Just to keep you posted. Last weekend I sent to Expert B a short handwritten summary of the D&H95 box model equations and parameters, as well as a preliminary sketch for a tentative subnetwork of the BBN, in addition to the D&H98 model. He is currently looking at that. I hope that you'll find it useful.

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8 January
EXPERT B:
Many thanks for your discussion of Expert A's version of the Dade & Huppert equation, and your inputs to the elicitation questionnaire for quantifying uncertainties on the four parameters identified as variables in the equation.

The attached Word file briefly summarises the findings of the elicitation, for which equal expert weights were adopted (i.e. no performance calibration was activated).

Piece-wise linear minimal information uncertainty distributions for each parameter were exported from EXCALIBUR, based on the joint combination of the experts' judgments, and entered into the UNINET Bayes Net program. There, a functional node computes the minimum volume equation solution for 130 km runout, based on the four parameters, with 200,000 samples drawn from the BN to provide an uncertainty distribution over the minimum volume estimate.

Two variants of the BN were run: the first included lambda as a variable, using the experts' joint quantiles, the second with lambda fixed to 3.142 radians. The two sets of results are tabulated in the note, for comparison purposes.

Please review these findings, and share with us any comments or queries.

Aso-4: modelling minimum volume for TS1 130 km runout

8 January 2018

<u>Elicitation solution</u> (six experts; no calibration)

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Case name : MinVol_eqn_params       08/01/2019               Version W1.5.1
________________________________________________________________________________
Resulting solution (joint DM distribution of values assessed by experts)
 Bayesian Updates: no      Weights:  item       DM Optimisation:  no
 Significance Level:   0.0000    Calibration Power:   1.0000
__________________________________________________________________________
 Nr.| Id            |Scale|        5% |        50% |        95% |    Units   |
____|_______________|_____|___________|____________|____________|____________|
   1|Collapse Ht    |uni  |      2566|        5752|        9629|     m      |
   2|Flow density   |uni  |     686.3|         992|        1511|   kg/m^3   |
   3|Stress         |uni  |     244.3|        1868|        7666|     Pa     |
   4|Lambda         |uni  |     1.945|       3.044|       3.142|    rad     |
```

```
                             Range graph of input data
Item no.:   1 Item name: Collapse Ht Scale: uniform
Experts
  1         [---------------------*-----------------------------------]
  2                               [-----------------*-------------------------]
  3                               [-----------------*-------------------------]
  4              [----------------*-----------------]
  5 [-------------------------*------------------------------]
  6              [----------------*-----------------]
joint        [===========================*=================================]
    ~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~
    1500                                                             1E004

Item no.:   2 Item name: Flow density Scale: uniform
Experts
  1 [-----------------------*------------------------------------]
  2   [--------*-------]
  3 [------------*----------]
  4                    [----------*----------]
  5              [----------*-----------]
  6         [---------------------*-------------------------------------------]
joint [===============*============================]
    ~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~
    650                                                               2000

Item no.:   3 Item name: Stress Scale: uniform
Experts
  1       [-------*------]
  2    [----------*----------------------]
  3  [----*------------------]
  4    [--------------*------------------]
  5 [-----------*-------------------------------------------------------------]
  6 [-----*-------------------------------------------------------------------]
joint[===========*===========================================]
    ~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~
    100                                                              1E004

Item no.:   4 Item name: Lambda Scale: uniform
Experts
  1                                                              [-------*----]
  2 [-----------------------------------------------------------------------*-]
  3 [-----------------------------------------------------------*-------------]
  4                                       [----------------------------------]
  5                                            [---------------------------*-]
  6                    [-----------------------------------------------*-----]
joint                [====================================================*===]
    ~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~
    1.57                                                            3.142
```

Bayes Net calculation results

	Minimum PDC Volume [km^3]					
Model	**1%ile**	**5%ile**	**50%ile**	**mean**	**95%ile**	**99%ile**
Lambda variable	10	22	225	395 ± 470	1310	2260
Lambda fixed 3.142	12	23	230	450 ± 330	1295	2480

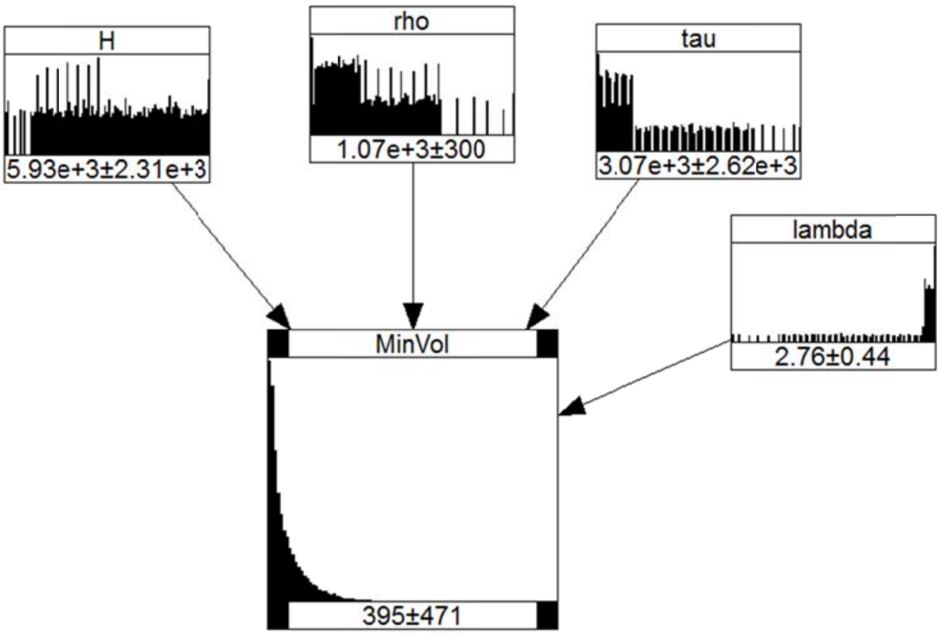
These results suggest the expected (i.e. mean) minimum volume required to produce a PDC that will have a runout 130 km, to reach TS1, is of order 400 km^3. There is about a 1% chance the minimum volume could be as little as about 10 km^3.

Fixing lambda to 3.142 radians – i.e. a circular PDC footprint -- slightly increases the expected minimum volume needed to reach 130 km (from 395 km^3 to 450 km^3). However, while the associated standard deviation of the latter estimate is noticeably reduced, there is substantial overlap in the uncertainty spreads of the two estimates.

At lower probabilities, the 0.1% chance (i.e. 1-in-1000) minimum volume would be about 7 km^3, and the 0.01% chance (i.e. 1-in-10,000) minimum volume would be about 5 km^3.

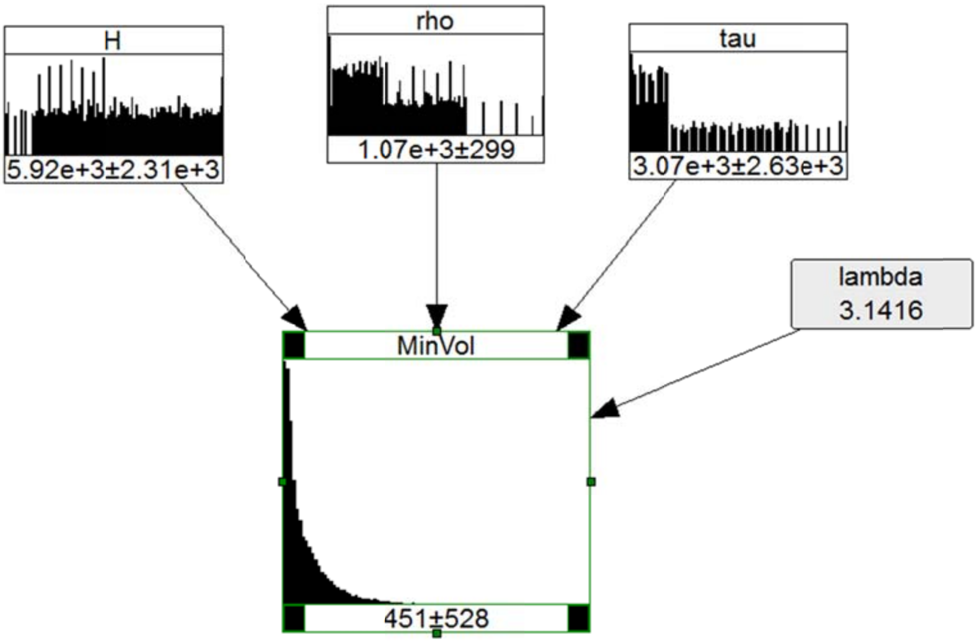
Acknowledging we are venturing into very marginal territory and recalling, for context, that the expected minimum volume is 395 km^3, the corresponding 0.001% probability (1-in-100,000) minimum volume is about 3.5 km^3.

(In these calculations, the variables are assumed independent of one another, and no correlations are included. Correlations can be easily implemented, if justified; however, tests suggest these would need to be strong to substantially influence results).



BBN with lambda variable

BBN with lambda fixed



08 January
EXPERT A:
These results are interesting! My interpretation is, based on this model:

1. We are reasonably certain (95%) that a pdc of about 1300 km3 would reach the TS1
2. We are reasonably certain (95%) that a pdc of volume 25 km3 would not reach the TS1.
3. The expected value of the minimum volume pdc that would reach the TS1 is around 230 km3.

08 January
EXPERT G:
These results seem at first pass quite reasonable and reflect the large uncertainties and simplifications that we have had to make. The smaller volumes at the 1% and 5% level seem to me to be a bit small, but I am not sure I can dismiss them. They must be a combination of large H and small tau.

08 January
EXPERT B:
One of the strengths of UNINET is that we can test combinations with node conditionalization. Hope to get to that tomorrow.

---

09 January
EXPERT B:
Point 3 of Expert A's interpretation is OK, with me.
The use of the natural language expression "reasonably certain", in Points 1 & 2, reminds me of Roger Cooke's views on such terms:
http://www.rff.org/blog/2014/deep-and-shallow-uncertainty-messaging-climate-change

For my part, I might rephrase Point 2 as "On the basis of our analysis, we assess there is a no more than a 5% chance that a PDC from an explosion involving a volume 25 km3 of erupted magma might reach the TS1". I think it is crucial to always state the initial conditional clause.

But I admit these are tricky concepts to communicate using the natural language, and it seems unlikely that other people's linguistic lexicons for probability, confidence, etc., (e.g. IPCC) can be adopted, especially for very extreme event likelihoods.

---

09 January
EXPERT F:
The main difference between my guesses and the group outcomes is about tau. I think we should stick it to the values obtained by D&H98 based on data (10-100 kPa). I am not aware of any other application with tau as small as 1 kPa but maybe that exist. In this case it would be good to have these other estimates for more mobile flows.

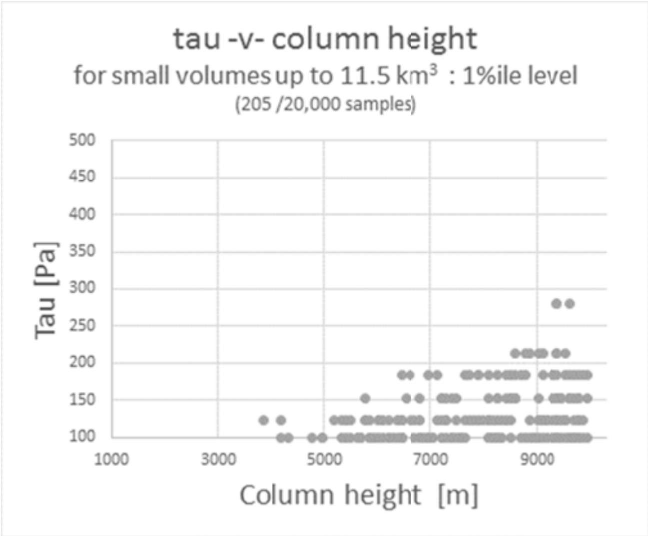
Given the uncertainty in the nature of the flow I would suggest to use also the box-model for dilute flows. I suspect we would get significantly lower values for volumes. However, this is my first impression.

---

09 January
EXPERT B:
To confirm Expert G's inference, this plot shows tau, column height sample values for all the computed small volumes up to 11.5 km3, i.e. the 1 percentile level (lambda is fixed at 3.142 for these calculations).

Note: my MinVol BBN calculation had tau values ranging from <u>100 Pa</u> to 10 kPa, to reflect the low-high span of the experts, so these small volume cases are all definitely controlled by tau values less than 300 Pa.

Recalling my earlier remark about this being an iterative exercise, perhaps colleagues should re-consider their stress numbers in the light of Expert F's comment about D&H98 data -- i.e. 10 kPa to 100 kPa? Or is a lower bound somewhere in range 100 Pa < tau < 10 kPa justified?



09 January
EXPERT E:
The results seem reasonable to me, given the broad range of uncertainties in the model parameters. To help with my understanding of reasonableness, I've been trying to gain some perspective on the modeled volumes with respect to the estimated volumes of Aso-4.

From my simplistic understanding, the modeled volume should represent both the ignimbrite and co-ignimbrite deposits, as the elutriated co-ignimbrite ash plays a likely important role in the development of shear stress as the flow travels outwards from the vent area (and would seem to represent an important component of potential energy at the release height).

Takarada et al. (2017) appear to have done a rather detailed calculation for the volume of the Aso-4 ignimbrite, with an average DRE of 370 km3 (ignoring min-max for now). They do not cite the deposit densities for Aso 4, but apparently used a range of densities depending on deposit character within each grid cell. Assuming the average density of 1000 kg/m3 (with magma of 2200 kg/m3, Kaneko et al, 2007) thus yields an average deposit volume of ~800 km3.

Although we don't know the exact contribution of Aso 4I-1 to the total volume of the co-ignimbrite ash, 4I-1 accounts for 75% of the pyroclastic flow volume for Aso-4 (i.e., Kaneko et al., 2007). Applying that relationship suggests >300 km3 of the co-ignimbrite ash deposit was from Aso 4I-1.

These volumes suggest the total volume of the Aso-4 PDC deposits was on the order of >1100 km3.

If you believe these numbers (both the volumes and the model results), it would suggest that Aso-4 flow volume represents an approximately 90%ile confidence of a flow reaching the TS1. Alternatively, a flow volume an order-of-magnitude smaller than Aso-4 would have an approximately 20%ile confidence of reaching the TS1. This might be a way to communicate the results without using more subjective terms, such as those used in the NRA regulations.

Would certainly appreciate any feedback regarding my possible misunderstanding of the role of the co-ignimbrite ash in our modeling efforts, as it does seem germane to other models that would parameterize "flow volume" directly.

---

09 January
EXPERT C & EXPERT H:
Concerning the D&H95 model for the dilute flows:
The equations to be used here is:
(1) V=lambda*L^(8/3)*ws^(2/3) / [4*phi0^(1/3)*gp^(1/3)*Fr^(2/3)]
Where:
V is the volume of the collapsing mixture of gas and particles
L is the maximum distance reached by the flow (TS1 130km)
g is gravity
lambda is one half angle (fixed to pi in what follows)
ws is the settling velocity for the effective particle class
phi0 is the initial (t=0) solid volume fraction
Fr the Froude number

gp is the reduced gravity:
(2) gp=g*(rho – rho_a) / rho_a
where rho is the effective particle density and rho_a is the air density at the emplacement temperature (i.e. the interstitial fluid is assumed to be air – otherwise the entire model needs to be generalized)

In this case we should elicit:
OPTION 1
phi0, ws, rho
[Fr = 1-1.2]
[lambda = pi]

That is, we can try to estimate ws directly. Another option would be to assume Stokes regime for terminal velocity (fine particles)
(3) ws=2*gp*a^2 / [9 ni]
and calculate ws from rho and effective particle diameter a (and ni is air kinematic viscosity):
OPTION 2
phi0, a, rho.

The values of rho_a and ni may be elicited as well, because they could depend on the temperature and on the presence of very fine suspended particles.

So, we should decide which option we prefer to follow.

Moreover, we already downloaded the DEM at 90m over the area, and we would like to use the energy conoid (Neri, Bevilacqua et al., 2015) approach to roughly estimate the potential effects of the topography for both box models (D&H98 – once a formula for the energy relation is calculated, D&H95 – using the formula in Neri et al.2015). Hopefully I can send you some results in about a week.

EXAMPLE1 with D&H95:

Assuming to follow Option 1 and fix the density of ambient fluid at 1 kg/m3. The ws=0.1 m/s, phi0=1%, Fr=1, rho=1500 kg/m3...obtaining a minV about 1390 km3 required to reach 130 km distance TS1. This should be interpreted as the volume of the collapsing mixture (solid fraction + gas).
Because the initial volume fraction is 1%, the DRE volume of the solid would be 14 km3 in this example.(about 20 km3 of deposit).

EXAMPLE2 with D&H95 – all the same, but ws=0.5 m/s. We get V about 4000 km3 for the mixture, that is roughly 60 km3 deposit.

A THIRD EXAMPLE
Another estimate can be done using the elicited value of H, the classical energy cone formula, and extrapolating the empirical relationships between H/L and volumes, for example considering the relationship reported by Ogbur&Calder2017 (other similar relationships can be used):
H/L=0.11 / V^0.14
For H=6 km and L = 130 km, in order to reach TS1 we would need V > 495 km3. Here V is the deposit volume. Much more that D&H95, but consistent with our estimates according to D&H98 (450km3 in average). We remark that the exponent 0.14 would become a power 7 doing the inversion of V, making the sensitivity extremely high.
Maybe other relationships exist for more dilute flows and we may think to use them as well.

To conclude, if we use different (complementary) models – D&H95, D&H98, and, the empirical relationship above, we should consider how to mix the models outcomes (e.g. elicit model weights/scores).

---

10 January
EXPERT G:
I like option 1. An elicitation as suggested by Expert C would be very good and complementary to the granular flow model. By far the most important parameter to elicit is the settling velocity (particle size). Since there are a huge range of particle sizes in pyroclastic flows it's not well contained what a representative grain size should be but I propose that we should calibrate against the observations for Taupo where most of the key parameters have already been estimated and some are constrained independently. The settling velocity that gets the Taupo (15 km3) to 80 km should guide our elicited range for settling speed. I am not convinced in using the Ogburn/Calder equation as this is purely empirical and only calibrated for flows which are orders of magnitude smaller than the ones we are considering.

---

10 January
EXPERT A:
I like your approach for eliciting for parameters in this equation. This equation was from your dissertation, correct? I still have trouble deriving it - although I have not looked at D&H95 (will try to today), but if you have a derivation it would be really helpful

---

10 January
EXPERT C:
Yes, it's from my dissertation, you remember well! It's inspired by the original derivation of D&H95.
Attached you can find the texified and corrected version of the V derivation according to box model with particle deposition.

---

10 January
EXPERT B:
I am going to set up the BN for the analysis, using your graphic sketch as my guide, and following Option 1.

In order to check my UNINET version, can you suggest a set of single values for input variables so that we confirm I get the same test results that you would expect??

---

10 January
EXPERT C:
In Option 1 we are going to elicit ws directly. Below I am providing to you a single value for each input variable. According to the box model equation, these values should output a collapsing mixture of MinVol=1390 km3 to reach 130 km runout distance. It's the Example 1 that I tested with Expert H.

Surely-to-elicit variables are:
rho = 1500 kg/m3
ws = 0.1 m/s
phi0 = 1%

Maybe-to-elicit variables are:
Fr = 1
rho_a = 1 kg/m3.

---

10 January
EXPERT B:
Do I need the Sauter no. a, too?

---

10 January
EXPERT C:
Sauter is only used in Option 2 to calculate ws. If we are concerned with option 1 we do not need it. However, I also think that having a diameter could be useful to set up our elicitation reasoning.

I am not sure of what value e should use for the Sauter. Maybe Expert F can guess a reasonable choice to test? Or better if we rely on Expert G's Taupo?

---

11 January
EXPERT B:
Draft BBN is now corrected; I will draft elicitation questions.

Thinking about Aso and the Bungo Channel stretch of water between Kyushu and the Sadamisaki Peninsula, prompts a typical simple-minded Expert B question.

This relates to the explosive interaction of PDCs with seawater, as observed more than once on Montserrat and discussed in respect of the base surge generation in the biggest event there in July 2003 (Edmonds & Herd; Geology, 2005). When a PDC enters the ocean, does not the violent disruption and dynamic boiling and vaporization of the sea surface produce an effective frictional brake on the flow? And can this be introduced in flow transport models? Or has that been done by anyone, and I'm just not aware of the work?

Sorry, three questions!

---

11 January
EXPERT G:
There is undoubtedly a lot of interaction when the dense basal region of a pyroclastic flow enters water (first pointed out by George Walker in 1982), but the overlying much thicker dilute (surge) part of the flow can move the water with limited interaction as observed at Montserrat of course. Empirically for large flows the surge clouds can easily move over tens of kilometres. An important point is that once they have reached the other side a dense layer can redevelop (as we know form the Belham valley secondary flow of 25 June 1997). I think that the mass loss rate from a flow moving over ware will be larger although I am not sure anyone has tried to model this. There is a paper by Jo Dufek but don't think this problem is addressed. Thus I agree that water travel is likely to reduce run-out all other things being equal. I am not sure how we estimate this beyond a elicitation. There is some empirical evidence from Kikai from Maeno's paper which indicates the flow went on another 60 km after crossing the sea, so its hard to see this being a huge reduction in run-out. We could ask Jo Dufek to comment?

---

11 January
EXPERT F:
I think it would be useful to find/have a total grain size distribution of the Aso-4 unit and then compute the Sauter diameter weighting the different classes. Including the fines.
Otherwise refer to a different similar unit in terms of mobility and scale of the flow. I am not sure if the Taupo would be appropriate, we have to ask to Expert G, I think.

---

11 January
EXPERT B:
The Sauter value is not needed if we elicit values for settling velocity, ws, but it seems Expert F and Expert C feel it may be informative for judging ws. Any thoughts from you, Expert G?

Back with the counterpart Dade&Huppert 1998 equation BBN, I am exploring the effect on the MinVol output distribution if the elicitation spread on resistance stress tau is conditionalized to be greater than the lower bound value of 370 Pa, which you suggested for Taupo. In other words, the lower tail of the elicited distribution below 370 Pa is zeroed out of the calculation (this sort of sensitivity test can be done straightforwardly with UNINET, which is a great advantage).

However, in order to perform this BBN interval conditionalizing in UNINET, the elicitation quantiles need to be re-expressed in terms of one of the standard statistical distribution forms (normal; lognormal; beta, etc). A preliminary trial using a Beta distribution for tau suggests the 5th percentile MinVol increases from about 28 km3 to 53 km3 if the Taupo lower bound censoring is applied, and the mean MinVol increases from about 336 km3 to 366 km3 (these first values differ slightly from the BBN results I circulated earlier, due to fitting the elicited quantiles to a Beta distribution).

One query arises: Expert F suggested using the tau range of Dade & Huppert 1998, i.e. “10 - 100 kPa”. Presumably the D&H range is 10 kPa to 100 kPa, not 10 Pa to 100 kPa? Am I right in thinking the Taupo 370 Pa lower bound is therefore more representative of a dilute surge flow, than the sort of dense debris/block & ash flows D&H were considering?

---

11 January
EXPERT G:
I am to entirely sure what Sauter is? In any case for the Date and Huppert 1995 case there are two key points. One is that assuming a single settling velocity is a huge simplification because real flows contain particles range from a few microns to decimetres so it’s not all clear how one could come up with a representative particle size. The only way I can see doing this is by calibration on Taupo.

I think your new results on minimum volume work much better for me and the logic you have applied makes sense.

I don't really agree with Expert F to have such a wide range. Dade and Huppert 1998 was based on debris avalanches not pyroclastic flows. While the transfer of the model from debris avalanches to mobile high aspect ratio ignimbrite seems to me fine but not the transfer of debris flow ranges to highly fluidised hot fine pyroclastic flows.

---

11 January
EXPERT B:
Wikipedia says: “In fluid dynamics, Sauter mean diameter (SMD, D[3, 2]) is an average of particle size. It was originally developed by German scientist Josef Sauter in the late 1920s. It is defined as the diameter of a sphere that has the same volume/surface area ratio as a particle of interest. Several methods have been devised to obtain a good estimate of the SMD”

---

11 January
EXPERT G:
Thanks Expert B for clarification. My feeling then is that the super parameter is a very minor contribution to the uncertainty in settling velocity and don't see need for eliciting it.

---

12 January
EXPERT C:
The ‘effective’ settling velocity of a multi-disperse mixture could be roughly approximated by the settling velocity related to the Sauter diameter of the mixture. The Sauter relates total surface of the mixture and total volume according to the same ratio measured in the polidisperse mixture, so the drag and inertia should be equivalent...and hence the ws too.

---

13 January
EXPERT F:
Expert G is correct saying that the assumption of s single diameter is a very rough approximation and that in principle is incorrect.

However, since we are talking about settling velocity and drag is the controlling variable this rough approximation has been used and it is still used in multiphase flow as a first proxy. Of course the wider the TGSD is and the rougher is the assumption. At the recent PDC workshop I have just attended almost all modelers adopted the Sauter diameter. Maybe we do not need to elicit it but simply compute it for a TGSD similar to that of the Aso (maybe the Taupo ignimbrite as Expert G suggested).

About the landslide model of D&H98 my concern was just that we were using it well outside the range of tau values that have been derived by the authors based on the experimental data presented in the paper. If we have other estimates of tau for other flows like Taupo we think more appropriate we should of course use them. This is just to clarify my previous point.

---

15 January
EXPERT H:
Independently of TGSD, for which we can use examples from Taupo or Campanian Ignimbrite and adopt the Sauter diameter, should we proceed with elicitation of the other parameters?

---

15 January
EXPERT B:
Yes – I'm working on exactly which parameters we need to elicit, and what related information should help frame the questions.

I gained an impression from the previous elicitation that those items were, perhaps, a bit too unconditional and non-specific. Any suggestions you or Expert C could make for conditionalizing the questions would be welcome. The Taupo AD182 event seems to offer a basis for some analogue guidance.

For grainsize distribution and settling velocity, can we adopt the approach outlined in the following paragraph, which is taken from a recent but undated NZ report on Taupo:

"For particle inputs we use a grainsize distribution modified from the 1980 Mount St. Helens eruption (Durant et al., 2009), but modified by Mastin et al. (2016) to account for ash aggregation by consolidating the fine ash into aggregate size classes that were fine-tuned to match four diverse eruption deposits . We adopt this size distribution due to similarities to those estimated from the 232 AD Taupo eruption (Walker,1980). The size distribution and density of aggregates were derived by systematic adjustment to optimize fit with mapped deposits (Mastin et al., 2016). One affect of using this size distribution which does not include particles coarser than 2 mm is that model thicknesses will underestimate the most proximal values (Mastin et al., 2016). To calculate particle densities we assume 65% vesicularity for 2 mm ash based the upper tail of pumice density histograms from Taupo (Houghton et al., 2010) that corresponds to a bulk density of ~800 kg/m3. We then increase the particle density by 200 kg/m3 every 0.5 phi size step down to 88 microns, where the total material in this size bin has a mean density of ~2600 kg/m3, assuming that the glass in 80% of the deposit at ~2400 kg/m3 and crystals and lithics make up the remaining 20% at ~2900 kg/m3. Ash3d calculates settling velocities using the formula of Wilson and Huang (1979), which considers ellipsoidal particles with a shape factor F = (b+c)/2a, where a, b, and c, are the semi-major, intermediate, and semi-minor radii of a 3-D ellipsoid. For particles, we use F = 0.44, which is the average measured by Wilson and Huang (1979) for natural pyroclasts. For aggregates we assume F = 1 (round aggregates)."

15 January
EXPERT H:
Thanks for the paragraph. That is a quite standard approach. Besides, Mt St. Helens, there are now several examples of TGSD that can be considered (see the attached Costa et al. paper for a review and Marti et al for Campanian Ignimbrite where both Plinian and CoIgnimbrite were reconstructed by Sam). Dioguardi et al. (2018) can be also very useful as it shows that results on real tephra particles don't depend strongly on the settling velocity model and even a simple model that combine Wilson and Huang (1979) with Walker et al. (1971), like in Pfeiffer et al. (2005), works well enough.

---

15 January
EXPERT C:
These are very interesting comments and references.

I am a bit confused about the best strategy to set up the reasoning. In my opinion would be:
1) Getting a TGSD from one or more of these cases study (Taupo TGSD or the MSH, maybe?),
2) calculating the d32 from that TGSD,
3) calculating the ws using one of the available formulas (possibly depending on super-parameters).
Did I miss anything?

I would try both Stokes drag and the Newton's drag (Dioguardi 'impact velocity' formula (2)). Sorry for the simple-minded question: "where we are with respect of the ranges of validity of these two laws?

But I suppose that the main uncertainty comes from the value of d32. If we do not have a Taupo TGSD, then we could use the Campanian Ignimbrite.

Also, does it make sense to consider some correction for hindered velocity? Isn't ignoring other particles (except in the buoyancy) a major approximation?

---

16 January
EXPERT C:
I fixed the box model code, which now runs consistently over the two DEM I have – one from srtm NASA, and the other from GSJ. Results over NASA dem are a little bit more accurate (cellsize about twice finer). But the code is running just fine in both cases.

Both the DEMs are lat-long, and I had to consider a correction for the angular projection, which was not negligible at this scale. In particular, distance in the N/S direction is corrected of about -15% with respect to E/W inside the code. So that the output pictures are metrically correct (having two DEMs was very helpful to double check my code). I spent a few hours on that to be sure.

I am attaching two examples:
blue plots assume
w=0.1; Fr=1; gp=14700; phi=0.01

In this way, according to our equations, collapsing minV=1390 km3 to reach TS1. Surprisingly, local topography seems to shield a bit the target site. I am including the invaded region assuming half and double volumes, to see sensitivity on the parameter.

violet plots assume
w=0.5; Fr=1; gp=14700; phi=0.01

In this case, collapsing minV=4070 km3. Results are different, and larger volume depositing faster is less affected by topography.

Tomorrow I am trying to see what can I do with D&H98 other equations.

---

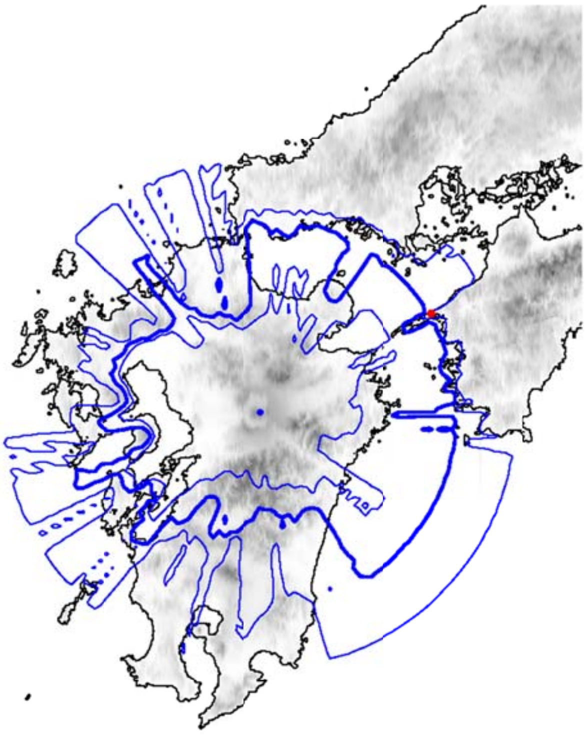
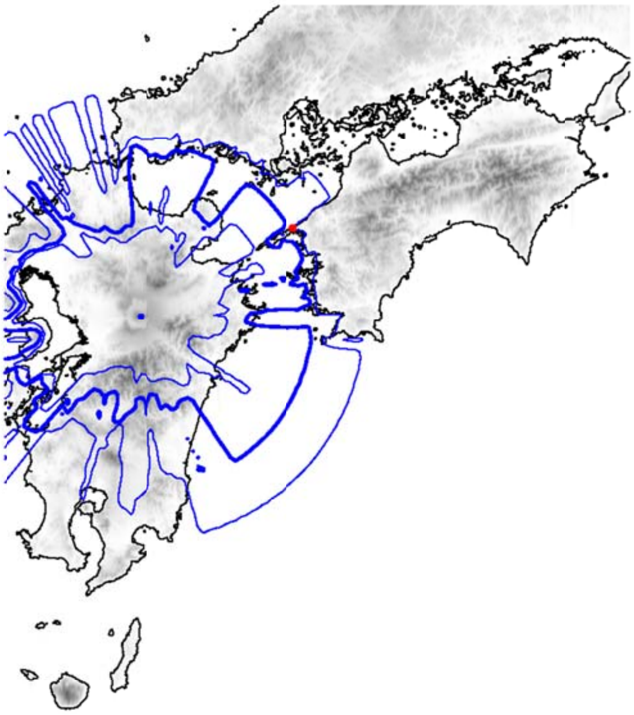
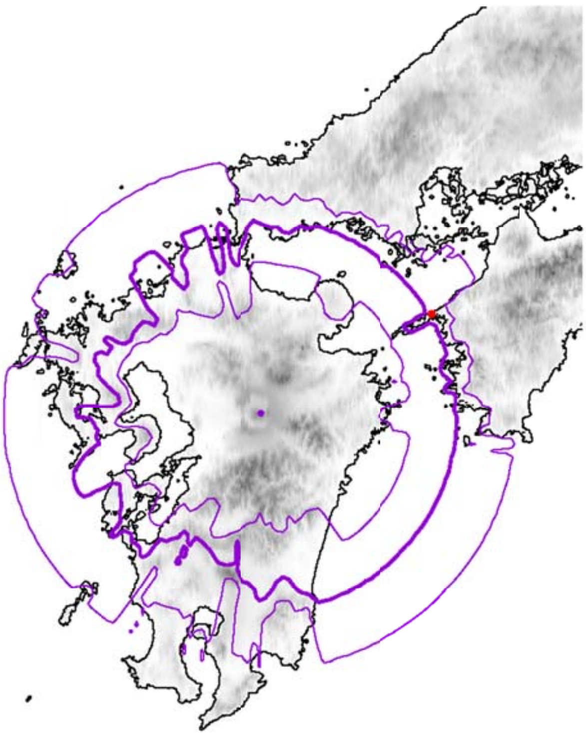
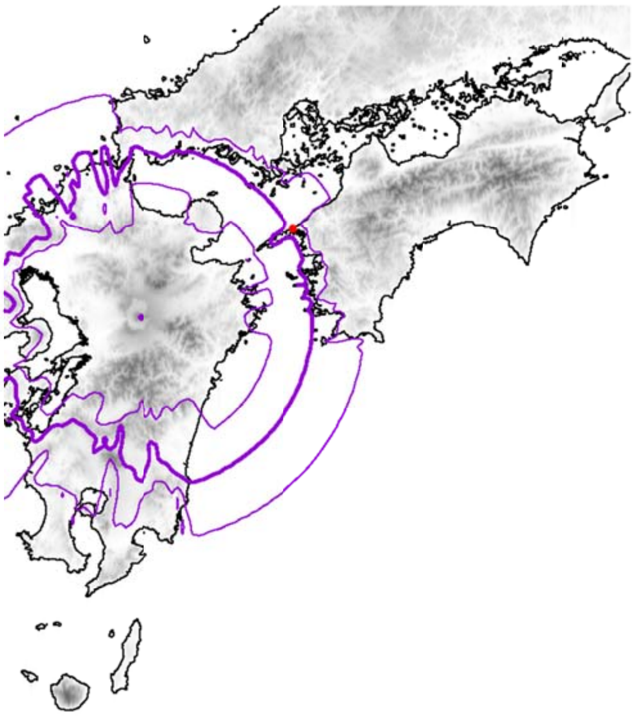
16 January
EXPERT H:
Thank you for the update. Glad you fixed the code to run over DEM. Once we will have the input parameters elicited we can run it considering the relative distributions.
It would be very helpful to use a similar approach also for D&H98.

---

16 January
EXPERT B:
I am a little uncertain about exactly which parameters now need to be elicited. To avoid ambiguity, can you and/or Expert C list them for me, please?

Also, can we propose numerical values and range, or a distribution form, for TGSD / Sauter diameter?

When, later, we come to consider the likelihood of a repeat Aso-4, my thinking is to suggest a median or best estimate for the volume of Aso-4, and then elicit upper and lower quantiles to use for probabilistic calculation. Given you guys have been working closely with Shinji, could you define the type of volume we should work with (PDC? Total? bulk? DRE? ….) and then indicate the central value that would be best to adopt for the purposes of our project?

18 January
EXPERT C:
We should proceed to elicit phi0, rho, rho_a, and ws. This is Option1 already endorsed by Expert G and, after further thoughts, we are ending to stick to that.

However, I believe that we need to use some preliminary calculations of ws based on the Sauter Diameter d_32 to anchor our discussion.

First I took the paper about Campanian Ignimbrite that Expert H attached. Before to adventure ourselves in a more detailed calculation that would require the full TGSD of an analog, I simply followed an approximation of the TGSD of the flow as the mixture of two monodispersed classes – one for the Plinian phase, and the other for the co-ignimbrite cloud. Also Expert E in their last email suggested us to consider this phenomenon in Aso-4 too.

So the two diameters are 177 microns and 31.3 microns, respectively. I calculated the volume and surface of a spherical particle of this size. Then, using the tephra volume in the paper – 54 km3 for the Plinian class, and 153.9 km3 for the co-ignimbrite, I was able to obtain the numer of particles in the classes, and then the total surface of them. Using the formula d_32=6*totVol/totSurf, I got a Sauter of 79.4 microns.

In the experimental paper attached by Expert H, I got the formula for ws:
ws = sqrt(4/3 * gp * d/Cd),
where Cd is a coefficient that depends on the sphericity of the particle and its Reynolds Number Re = ws * d / ni. Here d means the diameter of the particle considered, and ni the viscosity of the ambient fluid.

Expert H told me that the Stoke's formula:
ws = 2/36 * gp * d^2 / ni
can be approximated by the formula above, in the limit of Re low.

Then we could use the values of Cd from the experimental paper, summarized in the figure attached. The full range of Cd is 3 orders of magnitude.
However, if Re >> 1, then Cd in [0.75, 5]. If Re \approx 1 or less, I think we can use the Stokes' Formula, with ni=10^-5 Pa s.

So, using d=d_32 of Campanian Ignimbrite, gp from our preliminary example based on rho_a=1 kg/m3 and rho=1500 kg/m3, and Cd in [0.75,5], I got:
ws in [0.5,1.5] m/2.

Using the Stoke's Formula with the same parameters, I got ws = 0.5 m/s.
Now, if Cd is much larger, let's say 100, then ws becomes 0.1 m/s, and then, if Cd is 1000, it can decrease further, to 0.05 m/s.

This is all I got.

To respond to your question, I would propose the following ranges for our preliminary test of D&H95.
ws in [0.05, 2] m/s, from calculations above.

Then, but here I would need some backup from colleagues:
phi0 in [0.005, 0.02]

rho in [1000, 2000] kg/m3
rho_a in [0.75, 1.22] kg/m3

These values of density define gp too, according to the formula gp=g*(rho-rho_a)/rho_a:
gp in [8000, 26000] m/s2.
Beware it's not uniformly distributed in this range, if the densities are uniformly distributed.

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18 January
EXPERT C:
About the D&H98, I came to the conclusion that we cannot construct a nontrivial kinetic energy decay function (as a function of the radius from the source) like we do for D&H95.

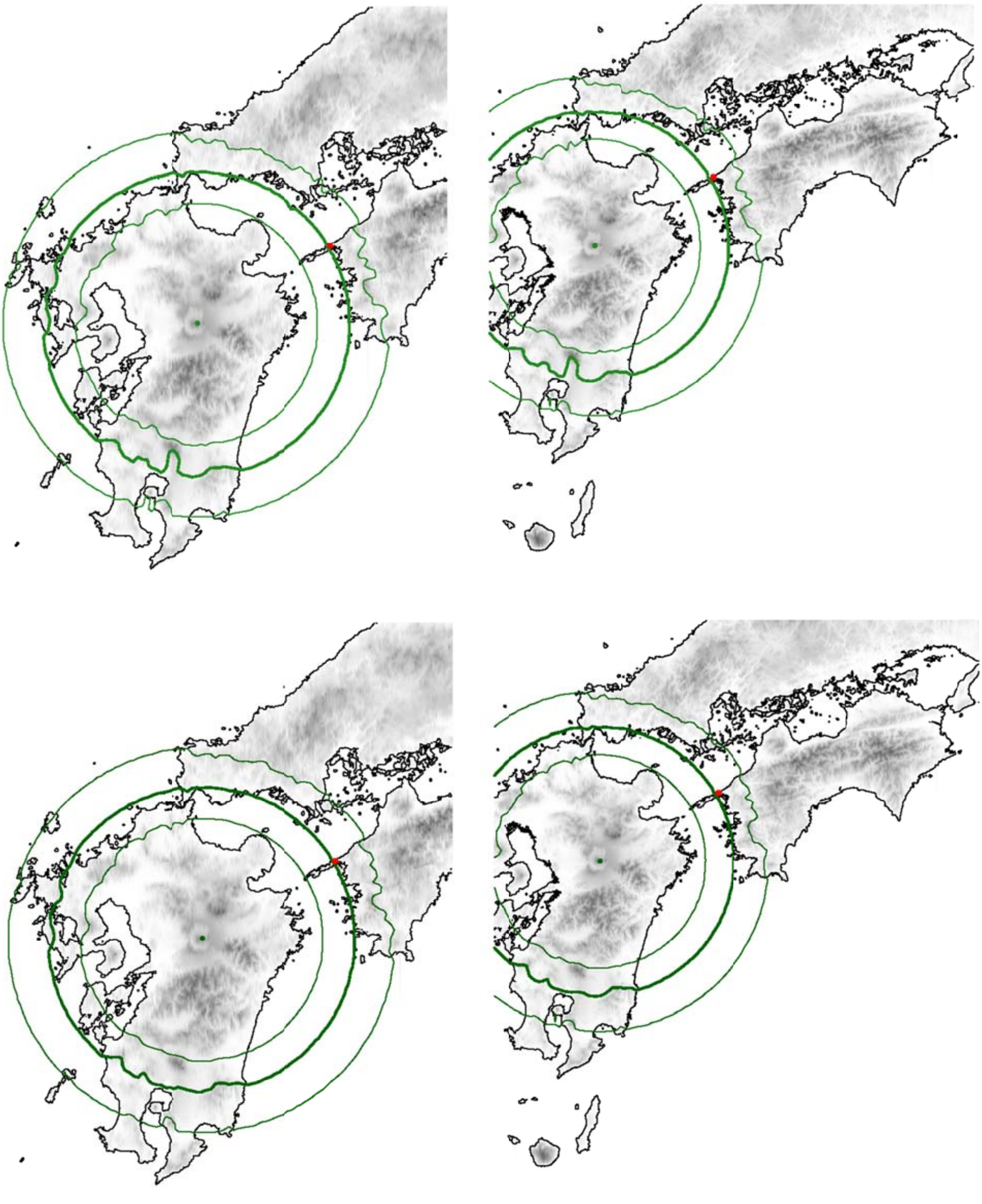
The reason is that the rockfall model is built over the simple equation, coming from seismic analogy, W=tau*A*Lmax.

W is the work made by a surface A sliding for a length Lmax with a stress tau.

However, here A is the total area inundated and L is the runout radius. I believed that we could obtain some other formula for the work, valid during the propagation of the flow, so to get a value for the kinetic energy as a function of the distance from the source. Like another energy conoid. But this does not make sense. If I integrate a formula like the one defining W, I will have to vary A=pi*L^2, and the integral sum with L in [0, Lmax] does not preserve that linear expression above.

So our tau cannot be assumed constant during propagation, like instead it's done in the famous Kelfoun2009 reformulation of the D&H98 idea (see attached paper). The Kelfoun tau is not our same tau. We should also be careful when we use the tau ranges using the Kelfoun model to calibrate our choice of tau. Our tau is averaged while the area enlarges, so there isn't any time dependent or distance dependent formula for the kinetic energy available during propagation.

What I am attaching here in the 'energyLinear' file is another inundation map, but assuming a linear decay of energy instead of the D&H95 box model. The angle is simply artg(H/L), where H is the height of the collapse that we assumed in the elicitation for the D&H98 model. So, first examples are with H=7000m, second examples H=10000m. This is like the 'classical' energy cone. We can however compare it with the box model D&H95, so that we can rely on two models at least when we prepare our report/paper.



18 January
EXPERT B:
Thanks, Expert C. I follow your reasoning, and the suggested uncertainty spaces for each parameter look sensible to me.

What about Froude number? From earlier emails, I think the working range for the Froude number was in [1.0, 1.2]. It seems to me to be sufficient just to take this as Uniform on [1.0, 1.2]. Or do we need to elicit it?

Based on your excellent explanation, I will prepare a brief elicitation questionnaire, and hope to disperse it to the panel for this weekend. Will it be OK to tell colleagues that, if they wish, they can adopt -- as their $5^{th}/95^{th}$ percentiles -- any of the lower or upper bound values we suggest, but they should also consider providing alternative values within the quoted range(s)?

---

18 January
EXPERT C:
Yes, I believe that the Froude could be assumed uniform in [1, 1.20] without further elicitation.

Yes, further alternatives for the percentile bounds, either interpolating or extrapolating the provided ranges would be very welcome. I was discussing with

Expert F that probably rho_a could even be lower than 0.75, that is air at 200 C°. Also the rho range could be made larger (Expert H has any suggestion for that? I remind that here it is the density of the particles, not of the fresh deposit).

Instead, ws and phi0 ranges are pretty wide already, in my opinion.

---

18 January
EXPERT H:
I agree for the Froude number. Particle density range can be up to 2300-2400 for the finest particles of such composition (dacitic-rhyolitic). It can be ~500 kg/m3 for the coarser ones. Air density range can also be wider (but it will spatially variable) because of effects of temperature, however I think also the parameters that introduce more uncertainty will be ws and phi0. For ws I suggest to use a starting point a TGSD of a well studied eruption like MSH 1980 (see also Costa et al., 2016 EPSL for comparison with other well characterised TGSDs).

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19 January
EXPERT B:
Sorry I haven't responded earlier to Expert C's latest box model runs, which look very good. Well done!

First remark: Expert C writes:
Surprisingly, local topography seems to shield a bit the target site

Personally, Expert C's finding is very comforting: in my original advice …….. I wrote:

In the case of the Aso-4 eruption, there is a further factor that would have influenced the behaviour of a PDC arriving at the coast [of the Sadamisaki Peninsula, i.e. TS1]. The topography of the Saganoseki Peninsula [i.e. on Kyushu] would have split the bulk of any northeast-directed PDC flow that reached thereabouts and, at the coast, some of the flow would have been steered north into Beppu Bay, and some southeast into Usuki Bay. This alteration and bifurcation of flow trajectory would have reduced substantially the portion of the flow that was initially directed towards the northwest, overland in Oita Province. Thus, in the direction of TS1, the extent of the area offshore, where the flow entered the sea, would have been much reduced* than if the local topography had been flat. Moreover, after this violent, disruptive entry zone, any remnants of the flow that carried further, over the sea, would necessarily have been very attenuated, the concentration severely diluted and associated temperatures rapidly reduced close to ambient.

Expert C: please can you clarify what the three contours on your plots represent, in particular the bold contour? I'm guessing they are, from inner to outer, the 95%, 50%, and 5% runout distance exceedance probabilities? Or is the bold contour the mean (expected) runout?

may I suggest we need a run with MinVol adjusted so that the mean value contour passes through the centre of TS1 (not just reaching the same 130km distance nearby offshore).

For our internal discussions, is it possible to also include on these plots the mean runout if topography is ignored - i.e. a flat Earth model? It is the obvious counterpart people may ask about (especially Expert G!).

Taking the conversation one step further, when we contemplate TGSD and ws issues, it seems to me it is important to consider the potential of the ocean to 'consume' denser components of PDC – bearing in mind, too, that sea level was much lower 89,000 years ago. (Expert A and Expert I are working on PDC loss into the sea in their cellular automata model). Thus, for Expert C's box model, and as far as the TS1 hazard is concerned, may I suggest a sensitivity test model run that estimates MinVol for runout in a PDC that comprises mainly finer grain sizes, on the basis it is the fines that would propagate over the sea to TS1? Expert H: any suggestions of an analogue? AMS_B?

*As far as I know, there is no computer model that can simulate PDC – ocean interactions with which to assess effects and parameters relevant to conditions appertaining to Aso-4. One possible line of investigation that could be pursued in respect of evidence for the physical processes involved in PDC-ocean interactions in the Aso-4 event might be to assemble information about any dateable volcano-tsunami deposits on coastlines around the inland seas of the region, and to develop a hydrodynamic model for numerical evaluation of the causal conditions that produced those deposits (see, e.g., Heinrich, P., et al., 1999. "Numerical modeling of a landslide-generated tsunami following a potential explosion of the Montserrat Volcano." Physics and Chemistry of the Earth, Part A: Solid Earth and Geodesy 24(2): 163-168; Tappin, D.R., 2017.The Generation of Tsunamis, In: Encyclopedia of Maritime and Offshore Engineering, doi:10.1002/9781118476406.emoe523).

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19 January
EXPERT H:
I agree Expert C's results look very important. I was interested to understand the topography control at such distances, simply neglecting effects of the sea, and it is the reason for which I asked Expert C to run the energy conoid model based on D&H95 with DEM. I hoped it could be done something similar for D&H95 but I read the explanation of Expert C and I agree we cannot use it in a similar way. However the point he highlighted on tau was also important for our previous elicitation.

The other option I suggested, which is a simple EXTRAPOLATION (I remark it because it seems Expert G didn't read carefully) of the empirical relationship between Energy Cone friction angles and volumes, to be used to get also some insights on topography control. The runs Expert C already carried out (sent us in a previous email) are showing the potential impacted area in accord to the Energy Cone model. However, I think he used very high values for the collapsing height. It shouldn't be more than 5-6 km, as indicated by 3D numerical simulations for large explosive eruptions (even our elicitation suggest 6 km).

For a sensitivity study, the most practical approach I would suggest is to use a TGSD described as a sum of two lognormal distributions (bi-Gaussian in Phi, see Costa et al., 2016), using the parameters (p,mu1,sgm1,mu2,sgm2) estimated by Costa et al. (2012) for Campanian Ignimbrite and Costa et al. (2014) for Toba (see attached papers) and then cut the coarser grain size population at different thresholds.

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20 January
EXPERT B:
I am preparing an elicitation questionnaire for elicitation #2, to ascertain parameter quantiles for the BBN deposition equation model calculation. From your comments, I sense we may need to revisit tau for the first elicitation/model. If this is true, please can you guide me on how to frame the tau parameter re-elicitation question(s)?

---

20 January
EXPERT H:
Concerning previous question by Expert B about the need to elicit again tau I have some doubts because, even if we don't consider Kelfoun's results for other applications, there is no easy way to try to estimate such a largely empirical value, so I would keep the wide range we got from previous elicitation. However, if you think can be useful to repeat the exercise discussing the main issues with our colleagues, I would be happy to make my re-assessment. Concerning rho, I don't think it's worth to elicit it as, if we use my suggestion to describe TGSD analytically, we can assume an average particle density varying with phi, using for example Bonadonna & Phillips (2003) JGR for a rhyolitic composition (see attached plot for rhyolite juveniles)

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20 January
EXPERT C:
Just to be on the same page about the parameters ranges of elicitation #2:

ws	[0.05, 2] m/s
rho	[500, 2400] kg/m3
rho_a	[0.6, 1.22] kg/m3
phi0	[0.005, 0.02]
Fr	[1, 1.2] (not elicited, but uniformly sampled)

I am only a little doubtful about using the very low rho of coarser particles as our lower bound, but all these bounds MUST be wide, so maybe t is on the same line of others.

I am not sure how we should re-frame tau, but now I am a bit worried of using such low values, that lay outside the range described in the original paper. We cannot rely on the fact that Kelfoun's model assumed them, because his tau is locally constant in time and space, while ours is averaged in time and space, and the average is done while the inundated area grows, so it's not a trivial averaging in time-space.

20 January
EXPERT B:
My instinct is to keep rho in the elicitation and then we can at least compare the group median or mean for elicited rho with the average particle density equivalent, derived as suggested by Expert H. It would be a sort of "sanity check" and, if there is rough agreement, then we can use that as "validation" evidence!
I will think about tau, and let you know…

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20 January
EXPERT F:
Your box-model analysis seems appropriate to me and I agree with most of your comments. The parameter ranges you define seem appropriate to me and should be representative of the TGSD that you mentioned for such a large ignimbrite.
Re the tau of the D&H98 model I was wondering if we have some estimates of it based on the application of this model to some case-study such as Taupo or similar. Since we are dealing with a process likely quite different from a landslide it would be good to have these estimates to properly elicit it.

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20 January
EXPERT C:
Responding to Expert B:

1) No, I did not assume a distribution of runout distance probability (we did not define it yet). I simply took the required volume V0 to reach 130km runout (it's the bold line), and then I took V0/2 and V0*2 as a comparison (they're the thin lines).
2) I can adjust the required runout to see what's the required volume. I remark the topografical effects are only crudely introduced, and only to closest obstacles to TS1 have an effect in our simulation (not the mountains on Kyushu).
3) I can include on these plots the mean runout if topography is ignored - i.e. a flat Earth model.
4) I believe that the effect of the sea 'consuming' denser components of the PDC could be easily tested looking at a monodisperse with only the finest ashes. Probably a lower volume will be enough, but that should be compared to part of the initial volume. I will try to do something about this.

Responding to Expert H:

1) I can run with lower H. The required H/L angle to reach TS1 is easily calculated. Then we can compare it with the extrapolations and see what volume comes out.
2) I will calculate Sauter from the case studies of CI, MSH and Toba, increasing the finer portion too to test the effect of segregation of components. I will need to use the Bonadonna&Phillips2003 expression of the density as a function of diameter.

I guess that what Expert H asks about TGSD and density is more important to 'anchor' our elicitation so I will do that first.

Finally, I wanted to point at D&H, 1996 Nature, and to the differences with D&H95. They are already using the box model with particle deposition on the case study of Taupo. They are using a reformulation which uses a full TGSD and an interstitial fluid different from ambient fluid (see grey box with equations). We should try to use it in our case study, if not for the inundation maps, at least for the volume constraining. Also, we may found interesting information on how did they constrain the parameters of Taupo.

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21 January
EXPERT B:
While I need to refresh my seismological understanding and read up on current theory, the seismological work W equation is in fact a gross simplification: in the real Earth, the stress(es) on an earthquake fault is not spatially uniform or time invariant - it depends on the locations and sizes of asperities, on rupture velocity and directivity, and on fault geometry. In relation to the latter, Lmax is only an indicative scale length, approximating the "length" of a geological structure (fault), usually at the surface; recent research in California suggests the "true" length of a fault at seismogenic depths may be significantly longer than its surface trace (partly, but not entirely due to shortcomings in geological mapping).

So, in seismology the equation W=tau*A*Lmax is a giant approximation. My question is: can we not invoke a similar gross assumption for the total work done by a PDC by the time it stops moving, as the sum of the work done by a set of annular areas x the radius step per annulus x average tau?

---

21 January
EXPERT B:
I have had a chance to look at the D&H 1996 Taupo paper, and completely agree with you that their model and results potentially represent an important reference benchmark for our Aso study. Please may I encourage Expert C to implement their Box 1 equations?

In the acknowledgments, the authors say not all the people with whom they discussed their model agreed with the views in the paper. I will email Herbert and Expert G and ask them if they can recall any points of dispute.

For your other models, as the results look so close, may I suggest you select one DEM to use - whichever is most easily implemented.

---

21 January
EXPERT C:
The epistolary exchange between D&H and C. Wilson from NZ maybe is what they were referring to, in their acknowledgements.

21 January
EXPERT B:
Elicitation No. 2
Expert C and I have prepared a second simple BBN calculation model to compute the Minimum Volume (MinVol) needed for a given runout distance, using the relevant Dade & Huppert depositional equation. For this we would like to elicit your judgments for four parameters, identified as rho, phi_0, ws, rho_a. On this occasion, we have investigated and discussed (with Expert H and Expert F) suggested intrinsic ranges for each parameter, based on empirical data, model results, published stuff, etc. These are indicative, and there is no requirement for you to place your lower and upper quantiles within the suggested ranges. However, we would welcome a brief note in the spreadsheet explaining why you think the range is inadequate.
As noted in the workbook, we will be particularly interested to see if there is coherence in the group's responses in terms of parameter skewness as indicated by offset, non-centred median values. If skew is indicated, we can use the quantitative elicitation results to define a suitable distribution to represent the parameter in question.
May I invite you to try to respond with your judgments this week, please?

Update on other activities
Meanwhile, Expert H and Expert C, with great support from Expert F, are developing a couple of DEM-based energy cone/conoid numerical models to calculate eruption volumes necessary to produce PDC runout estimates, and preliminary test runs by Expert C are looking very promising. We hope soon to have some preliminary results for circulation to the Advisory Panel - watch this space!
Similarly, Expert A and Expert I have been working hard on a cellular automata model for PDC propagation from Aso, including a novel capability for the ocean to "consume" a proportion of the denser components of a flow. Watch this space too!
Thus, before too long, we should have available a set of six (or more variant) model results which each estimate the Aso-4 eruption volume needed to reach the TS1. The plan is put these MinVol estimates onto a logic tree, with weights for each which will be elicited from the group, once we have had the opportunity to circulate all the models, and discuss their merits.

Remark
This aspect of the Aso study is turning out to be more complex, and challenging, than I had naively envisaged. This said, however, it has every promise of being an exceptional piece of novel volcanological research with the strong and insightful inputs of colleagues, and I am grateful to you all for your constructive contributions.
Please email me if you have any queries about Elicitation No. 2 or the on-going modelling work.

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21 January
EXPERT B:
Expert C and I are contemplating a numerical implementation the D&H 1996 Taupo model as a possible analogue for Aso runout modelling, but noted the acknowledgments indicate dissenting views from some person or persons who reviewed their draft. It may be this was mainly Colin Wilson.

Do you think it is worthwhile us working up the D&H equations for Aso, or are there fundamental volcanological shortcomings?

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21 January
EXPERT G:
Having read the discussion and reply it seems to me a case of describing an elephant with one describing the trunk and the other the tail. I am marginally, maybe 60:40, more persuaded by Colin and agree with his main point that a significant amount of the mass travels in the dense basal part, which is also consistent with the recent offerings by Roche et al (Nature comms) and Bread and Lube (EPSL). What Colin doesn't acknowledge adequately is that the basal avalanche develops out of the dilute current which is essentially what the more recent research establishes. He recognises that the current is dilute near the source but in his model the dense flow is formed very soon and so the mass is transported most of the way in the dense region. I am not so sure this is quite right and I envisage a more progressive transfer of mass from the turbulent dilute current to the basal region. Of course much of the eventual deposit is dominated by the basal region. As far as the ultimate run out is concerned it's likely that it's the dilute upper region that gets to the furthest distance and so I think its justifiable to use the Date and Huppert model. However, a weakness here is that the Wilsonian view or the more hybrid conceptual model that I have outlined requires a loss of mass as the current progresses. It might be that Expert C could introduce such a loss function such as being proportional to 1/u or even 1/u^2 so that the buoyancy diving the flow diminishes with distance. 1/u is suggested by equation 7 in Sparks et al (JGR 1978).
I am not sure if you are going to include lift-off as a mechanism to control the run-out. My feeling is that this is the key factor and I presume the change from negative to positive buoyancy can be calculated in a model?

---

21 January
EXPERT G:
I am assuming that the model is Dade and Huppert 1996 turbulent paper. However, I am not clear about the choice of the suggested intrinsic range of ambient density. I would not include 1.22 kg/m3 as this implies the air in the mixture is cold. However, this is the density of cold air before it mixes with hot magma so it seems to be the upper range is too high. The particle volume fraction (first elicited parameter) is in fact linked to the ambient fluid density by the temperature of the magma so one can calculate the ambient fluid density if one knows the magma temperature (something like 750C). We could elicit the magma temperature of course and there will be some cooling due to processes such as incorporation of cold vent lithics or water from Lake Taupo. I recollect the emplacement temperature of the deposits is 450C so this could be a lower limit, but the emplacement temperature could also be influenced by air entrainment during flow. Thus I would like to hear more about why you chose 1.22 as an upper limit. I am on a train so not conducive to doing the calculation but it would be quite straightforward.

21 January
EXPERT B:
In connection with Dade & Huppert 1996 Taupo paper, I was wondering if, or how they used temperature, as they say their results are for the assumption "….. an eruption column and flow with constant temperature 450 degC…". This sounds like an emplacement temperature, over the medial – distal parts of the flow, not a temperature for the PDC at its inception? D&H also assume 10degC ambient air temp, and rho_air 1.2 kg/m^3

21 January
EXPERT C:
Today I almost finished to implement a code for calculating the Sauter diameter if the pdf dMass/dPhi (here phi is the particle diamter in Krumbein units) is a mixture of two Gaussian distributions (i.e. bi-Gaussian). This is the way in which the TGSD is described in all the papers that Expert H suggested to us.

The calculation is not straightforward because the Sauter d_32 = 6* TotVol/TotArea, and the calculation of total area requires the integration over the number of particles. That is not over mass like in the TGSD pdf formulation. So I had to write dN/dPhi=dN/dM * dM/dPhi, using the function of BonadonnaPhilips2003 for having the density rho as a function of the particle diameter (in phi units) of a rhyolite. What comes out is a density which is not bi-Guassian anymore. However, I am doing the integral numerically, and I hope this is going to solve the issue and get those Sauter diameters.

Tomorrow I should finish, and then send you the Sauters of MSH, CI and YTT eruptions, as well as the corresponding ws using the formulas of particle falling velocity. This should be helpful to experts eliciting for the monodisperse D&H95 model.

Then I am going to try implement a polydisperse D&H95 model, from the Nature paper. I spoke with Expert F and he is not convinced of the assumption of constant flux, because the eruption was likely pretty short lasting. So it would be better to continue with a constant volume. But the polydisperse feature would be very helpful, as well as having an interstitial fluid which is not ambient fluid. This should be allowing for the lift-off that Expert G's seeking for. I am a bit confused by his suggestion of additional mass loss. The current modeling is indeed depositing mass already.

22 January
EXPERT G:
I reread Date and Huppert and they do have a loss function (last part of equation 1). The difference between DH and Colin is that they propose the flow directly creates the deposit, whereas in Colin's thinking the deposit forms from the basal flow. Thus my overall feeling is that DH is an ok model of the ultimate runout of the material to is retained in the turbulent dilute part. I will get back on the temperature issue.

22 January
EXPERT G:
It's clear from Dade and Huppert that they mean the temperature of the initial mixture in the collapsing column. They also involve the magmatic water but to me this contributes hardly at all to the density so we can simply consider the average proportion of cold air entrained into the column to get an interstitial density at the magma temperature. Thus I don't see 1.22 being realistic because this is the density of air at 1 atm at 10C so the air of course must be heated and less dense than this value. the mixing proportion of hot ash and cold air is of course not so straightforward and requires an entrainment model, but if we fix the proportion and fix the temperature of the magma then the air density falls out. Likely Expert F and Expert H have columns collapse models than give a range of values for the mass fraction of ash in the collapsing column.

McClelland et al. (Bull Volcanol. 66, 492, 2004) estimated equilibrium emplacement temperatures at 400°-500°C for the 1.8 Ka Taupo eruption (bulk volume 30 km3), up to 50 km from the source. In contrast, in proximal facies less than 30 km from the source, the emplacement temperature was evaluated to be between 150° and 300C. Given that the cooling is quite limited die to entrainment during transport the chosen value of 450oC seems ok. At this temperature air has a density of 0.46 kg/m3. The lower near source temperature is likely related to a lot of cold lithics in proximal deposits.

---

22 January
EXPERT G:
My proposal is a range of 0.35 to 0.55 kg/m3 for the air density in the collapsing column with a mean of 0.45. This reflects a range of plausible column mixture temperatures with the central value commensurate with the palaeomag temperature estimates. My numbers are a bit skewed because we cannot really justify going below 300C and the lack of welding suggests <600C Note that for Aso-4 with its welded so I would choose slightly lower but overlapping range. For the values of air density one can then calculate the volume fraction of magma/ash (question 1) assuming a magma temperature of 800C and doing a heat balance.

---

22 January
EXPERT E:
The Dade and Huppert (1996) model (equation 1) has parameters for both the ambient air density, and an interstitial air density. In your spreadsheet, however, we only are estimating the ambient air density. But the suggested lower bound for ambient air density is unrealistically low for ambient air temperatures in Japan, much less planet Earth (i.e., reflects temps of ~300C). Interstitial air density is not elicited in the spreadsheet.

Expert G appears to be assessing the interstitial air density, whereas the spreadsheet requests the ambient air density. However, the range for the "ambient" air density has a lower bound that only appears realistic for an interstitial (i.e., heated) air density.

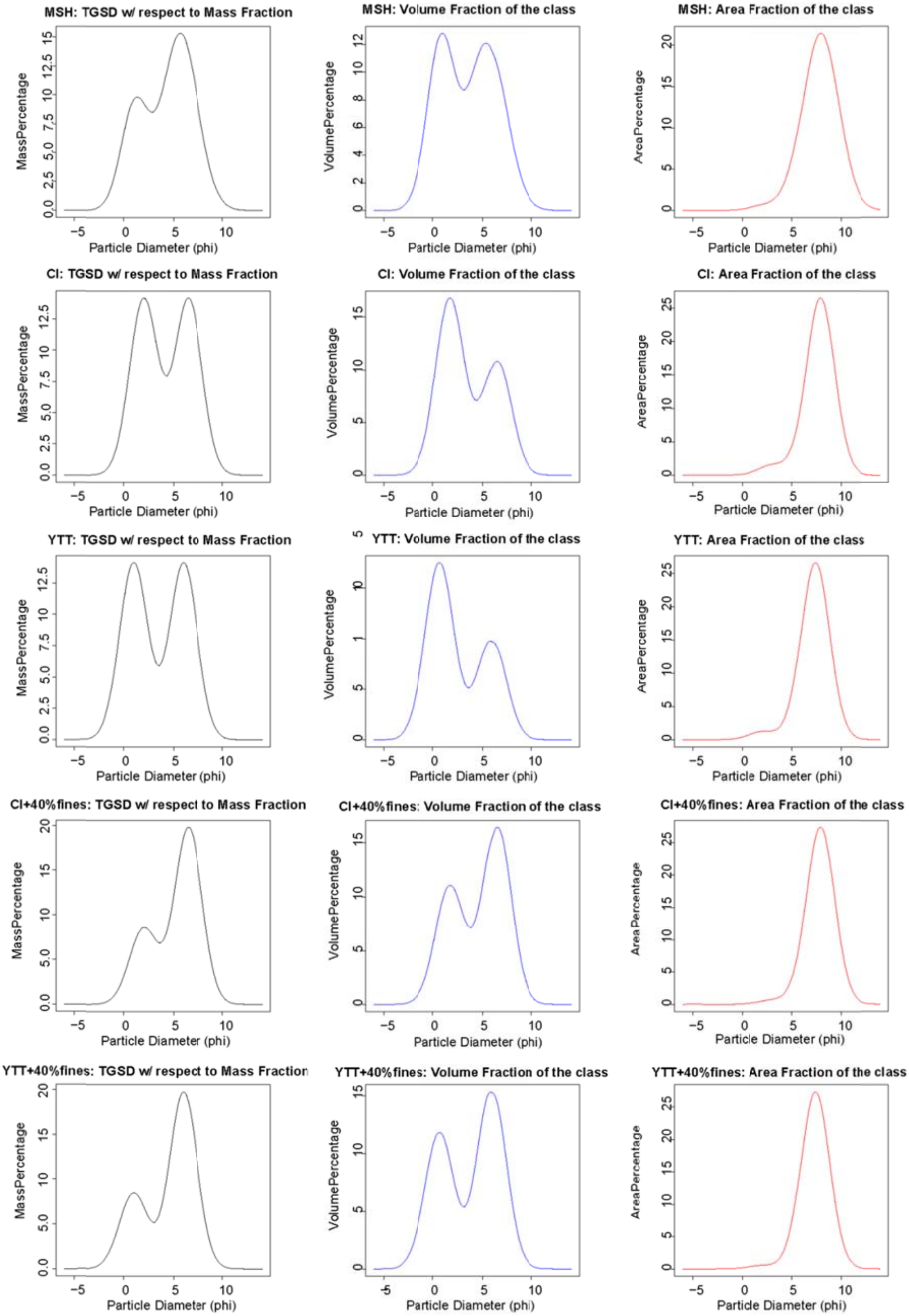
Does the elicited air density parameter reflect a combination of both ambient and interstitial fluid, or just ambient air? Certainly, I need a clarification.

22 January
EXPERT C:
Everyone now is looking at the D&H96 Nature – that is a DIFFERENT model from D&H95 monodispersed model that we used in Campi Flegrei papers... That's creating a lot of misunderstanding. The Taupo model assumes polydispersed mixtures, constant volume flux instead than constant volume, and interstitial fluid different from ambient air.

The elicited model is simpler. People should refer to the attached equations – you can forward them to the experts. The ambient density here is assumed equal to interstitial – that's why the numbers are lower.

---

22 January
EXPERT C:
Attached you can find the plots of Mass, Volume, and Area fraction as a function of particle size of MSH, CI, and Toba YT. These are based on the published TGSD w/ respect to mass, that is bi-Gaussian. The Sauter of MSH is 33.24 micron. It has 31% coarser and 69% finer particles. The Sauters of CI and YTT are 33.77 micron and 53.37 micron, but these are both assuming 50% of coarser particles. These are in pictures with number 05 in their name.


MSH: TGSD w/ respect to Mass Fraction
MSH: Volume Fraction of the class
MSH: Area Fraction of the class
CI: TGSD w/ respect to Mass Fraction
CI: Volume Fraction of the class
CI: Area Fraction of the class
YTT: TGSD w/ respect to Mass Fraction
YTT: Volume Fraction of the class
YTT: Area Fraction of the class
CI+40%fines: TGSD w/ respect to Mass Fraction
CI+40%fines: Volume Fraction of the class
CI+40%fines: Area Fraction of the class
YTT+40%fines: TGSD w/ respect to Mass Fraction
YTT+40%fines: Volume Fraction of the class
YTT+40%fines: Area Fraction of the class
MassPercentage
VolumePercentage
AreaPercentage
Particle Diameter (phi)

After speaking with Expert H I also calculated the Sauter assuming 30% of coarser (so increasing the fine). This is also on the line of the data in the other paper about Campanian ignimbrite. In this way, the diameters become 22.79 micron and 34.80 micron, respectively. These are in the pictures with number 03 in the name.

So, we have a range of Sauter diameters from 22.79 micron to 53.37 micron.
Using the formula:
ws = sqrt(4/3 * gp * d/Cd)
and Cd in [0.75, 5], and gp=50000 m/s2 obtained from a rho=2300 kg/m3 and air density of 0.45 kg/m3 suggested by Expert G
If d=22.79 micron we get:
ws in [0.55, 1.42] m/s
If d=53.37 micron we get:
ws in [0.84, 2.17] m/s.

If instead we assume gp=22500 m/s2 that's like using air density of 1 kg/m3 like in our previous tests:
If d=22.79 micron we get:
ws in [0.36, 0.95] m/s
If d=53.37 micron we get:
ws in [0.56, 1.46] m/s.
Somewhat lower, due to the denser ambient fluid.

What would make a lot of difference (like in my previous, simpler, example assuming only two classes) would be to consider much larger Cd (that mean a much less turbulent type of flow). These would reduce the ws significantly.

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22 January
EXPERT C:
Concerning some ambiguity in about which papers we are referring to, probably the most complete reference to the box model that we are eliciting it's Bonnecaze et al., 1995, J Fuid Mech. I have also taken some of the notation of Hallworth et al., 1998, J Fluid Mech, which includes the same equations and it's more general because includes a two-layer model (also attached).

Here a short list of most important literature I know:
Bonnecaze et al., 1995, J Fluid Mech
GENERAL CASE monodispersed

D&H, 1995, JGR
FIXED VOLUME, monodispersed, ambient=interstitial fluid, application to turbidity currents

D&H, 1996, Nature
TOTAL FLUX COSTANT, polydispersed, hot interstitial fluid, TAUPO application

Hallworth et al., 1998, J Fluid Mech
AGAIN REVIEW OF GENERAL EQUATIONS

D&H, 1998, Geology
Rockfall model that Expert A suggested to us. Based on tau.

---

22 January
EXPERT B:
Our position is this:

1) For elicitation #1 we addressed Expert A's equation calculation formulation, which was based on D&H 1998 Geology.
2) For the present elicitation #2 we should be assessing parameter uncertainties for Expert C's "box model with deposition" variant, which is based on D&H 1995 JGR.

Expert C has added two more papers, attached, which give further info - if you need it - on the provenance of the equations from his dissertation. At this stage, we are not directly considering D&H 1996 Nature Taupo, except as far as it may inform your judgments for elicitation #2, i.e. Expert C's box model with deposition.

---

22 January
EXPERT G:
I was confused and had been thinking all along that we were looking at Dade and Huppert Taupo model. I will have to redo my numbers. I am now not clear why we need to choose a range of ambient densities.

---

22 January
EXPERT C:
In our mind the reason for varying rho_a was that we are assuming rho_a=rho_i.
So hot ambient fluid means also lighter interstitial fluid.

It came from a discussion that I had with Expert H, asking ourselves if we should had had to fix rho_a=rho_i at 1.22 kg/m3, or to assume it a little bit lower.

---

22 January
EXPERT B:
Rho_a and rho are used in Expert C's formulation to enumerate g_p, which appears in Expert C's deposition boxmodel initial Von Karman and fiinal equations, where g_p = g * (rho – rho_a)/ rho_a.

---

22 January
EXPERT G:
Then I would just use one ambient density.

---

23 January
EXPERT H:
if we want to keep things as simple as possible I agree we can just use ambient density. I reckon that, in the framework of D&H model, it is not the most relevant source of uncertainty, compared, for example, to the assumption of a single effective terminal velocity.

---

25 January
EXPERT C:
Concerning the difficulties encountered when eliciting rho_a=rho_i, I would stick with the cold ambient air interpretation, because this version of the box model is not capable of modeling the flow take off effects anyway. I think that an artificially low rho_a is not physically meaningful, unless we allow rho_i and rho_a to be different.

I will provide a new version of the equations in which I enabled rho_i to be different from rho_a. Then we will finally be able to use all the additional information that Expert G provided us about the temperature.

---

26 January
EXPERT F:
About air density I also suggest a narrow range since, so far, we are working with a model that assumes rho_a=rho_i and therefore, in principle, this parameter should not be elicited. If Expert C will be able to implement the new model we can consider a different range of rho_i as next step.

---

26 January
EXPERT C:
I managed to rewrite the equations of V_min according to a variant box model that assumes the rho_i (the density of our gas) different from the rho_a (density of air). The formula is longer than in the simpler version of the model, but it is still analytical.

We could now use these formula to implement the initial temperature of the flowing gas in our analysis. The model assumes this temperature to remain fixed.

We might tell this to the other experts and let them elicitate rho_i for us before this round is completed. Or we might make a third round for that. What do you think?

A further possibility could be to implement energy conservation equations in our game, and let the temperature change through time, but that would require additional work and new parameters (and it's very likely to be not solvable analitically anymore). Not sure that implementing such complexity is making sense given all the other approximations, though.

It was not possible to calculate analytically a closed formula for the energy conoid according to this more complex model, as would be required to make inundation maps. I would need to implement a numerical solver – I can do that, if we want, but it would take additional work.

---

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---

26 January
EXPERT B:
Many thanks for modifying your box model #2 to a two-phase version, incorporating a separate interstitial air/gas density rho_i. Expert E was exercised about this, too.

Because I am still waiting on a couple of colleagues to respond, my suggestion is to ask everyone to review their rho_a values and offer values for rho_i in addition. Please may I invite you to update your judgments? I attach your own personalised Excel spreadsheet. Grazie!

Unfortunately I don't think we have the project leeway now to follow up your suggestion to introduce energy conservation equations.

---

27 January
EXPERT B:
Earlier, comments on the rho_a (and implicitly rho_i) issue were made mainly by Expert G and Expert E. My understanding, perhaps flawed, was that rho_a is not a significant parameter, and has small variation in possible value, IF it is restricted to ambient air. The challenge(s) arose if rho_a were to be considered a (fudged) combination of ambient air and interstitial air/gas densities.

My reading of Expert G's early comment was that interstitial gas density is the uncertain parameter, not ambient air density, which he eventually said should be 1.22 kg/m^3. But, of course, there is a temperature element in relation to rho_i. My presumption is eliciting rho_i would entail a range of values that span plausible (averaged?) temperatures within the flow.

Thus my proposal now is to see what comes back from colleagues on rho_i, pool the judgments, then perform the BBN uncertainty calcs using Expert C's revised equation, and settle on this as one set of model results for VolMin albeit less than perfect, to be assessed (and weighted) against the others.

27 January
EXPERT H:
Concerning the implementation of an energy equation, in this phase I don't think it's worth the effort.
As you wrote, it will be not possible to obtain an analytical solution and the model would lose its simplicity.
Obviously there are other relatively simple models based on different approaches with respect to D&H, that can be solved numerically (see for example Bursik & Woods, 1996). They may be used in the future in a comparison study, but for this application to Aso4 (for the report especially) I would suggest to use the two variants you proposed only (i.e. the last one and that presented in Neri et al. 2015).

Once we will have the results of the elicitation, we can set up the calculations for a probabilistic assessment of the runout distance and relative maps on the DEM using the energy conoid (and cone) model(s).

---

28 January
EXPERT A:
Concerning our cellular automata model, try running the simulation:
http://gscommunitycodes.usf.edu/gs/public/pdc.php

we seem to get the most convincing results if the pulse volume = the total inflated volume.

example inputs:
inflated flow volume = 5e12
pulse volume = 5e12
modal thickness = 80 m (modal thickness of the inflated flow - pyroclasts + air + gas)

In the interest of time we have kept this model simple. But this leads to some questions:
1. right now the thickness deposits on land is the same as sinks in the sea (80 m thickness per 1 km2 in the above example). Should this be adjusted?
2. Should the modal thickness decrease with distance from the vent?

Other comments? We can run probabilistic version once input parameters are elicited.

---

28 January
EXPERT C:
In my variant model, phi_cr is the 'critical' volume fraction at which the flow lifts off. My testing phi_cr is close to 3 x 10^-4. In this model we are still speaking of total total collapsing volume (gas+particles), but the variant should had likely reduced minVol.

I assumed rho_a=1, rho_i=0.5, and rho=1500 kg/m3, phi_0=0.01, and ws=0.5, Fr=1 I obtain MinVol=4894 km3. With the same parameters, the d&h95 'old' model requires MinVol = 4064 km3. These are volumes of the collapsing mixture of particles and gas.

In summary, in the variant formula for MinVol we have two changes compared to the original model:
The first change is:

1) The term
   phi_0^(1/3) = 0.21
   becomes
   phi_cr^(1/3) { sqrt[(\phi_0/phi_cr)-1]- arctan[sqrt[(phi_0/phi_cr)-1]] }^(2/3) = 0.17,
   with our parameters.

The second change is negligible.

2) g_p:=(rho-rho_a)/rho_a
   becomes
   g_c:=(rho-rho_i)/rho_a, but they are practically identical (because rho >> rho_a, rho_i).

So, basically, the change (1) increases the required MinVol of about 20%.
It seems that the hot gas can make the flow stop earlier, the other parameters being equal.

---

28 January
EXPERT B:
How does hot interstitial gas make a flow stop more quickly? That seems counterintuitive, if not counter-physical

---

28 January
EXPERT C:
Because in one model we stop the flow when it has deposited all the particles (phi(t) =0 at the runout).

Here the flow lifts off when phi(t)=phi_cr > 0. It makes sense.

This model is not based on potential energy of a falling object like in D&H98, but is based on the depletion of suspended particles below a threshold.

Something that we might have not considered is the effect of the hot gas on ws. The values that we are using are assuming the particles falling down with constant speed within the gas. If the gas is hot, is this assumption still correct? I believe that is correct, if we assume that the mixture is "well mixed", but maybe we should ask it to one of our experts?

---

29 January
EXPERT C:
After my meeting with Expert H, I am attaching the improved inundation maps.

Four are based on Model #1 (Linear decay of energy based on H/L for the mapping, and D&H98 for the volume calculation); the other four on Model #2 (D&H95 for either volume and energy decay function analytically solving Von Karman with reducing solid fraction).

Here is not implemented the D&H95 variant with hot gas – I will do it for recalculating the volumes. I chose to use the DEM centered on Kyushu (it's a bit coarser than the other DEM, but I believe this is not going to make any difference concerning our level of uncertainty).

NEWS:
1) dashed line shows the runout without topography.
2) I calculated the increased distance L required to inundate TS1. Because of local topography it can be higher than 130km, up to 170km. This increases the required volume, and Expert H said that is consistent with field observations (no deposits of Aso4 at TS1, but deposits found at further distances).
3) In model #1 I wrote the full volume (there is no gas there).
4) In model #2 I wrote the solid fraction volumes. They are pretty low. Maybe they would become larger if we follow the variant with hot gas.

Once I have the elicitation results I will provide all maps according to that. Right now I have still been testing graphics with a fixed V0 and half and twice it (they are not percentiles).

POSSIBLE IMPROVEMENTS:

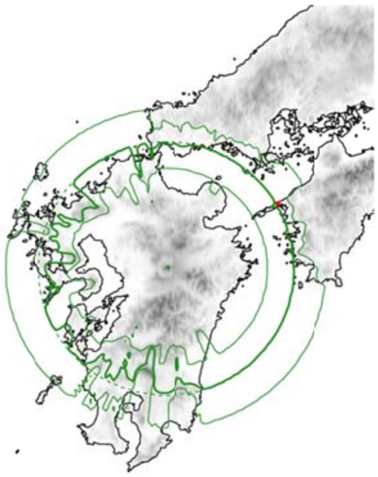
1) With Expert H I managed to close the equations for the calculation of ws based on the TGSDs. Our elicited speeds are producing very low Reynolds numbers, maybe lower than those number used for defining them. I guess that with this additional information we may want to rethink the elicited ws to lower values. I will write more details in a following email tomorrow.
2) I wanted to follow up with your suggestion of integrating a constant tau over circular annuli to find a modification of D&H98. Not sure if it is going to bring us anything useful, but I am curious to try.
3) The model of Bursik&Woods96 is interesting. It's different from the others. Their subcritical part (with low air entrainment) maybe is worth to be studied because they have analytical solutions in the Cartesian (canalized) geometry, and I believe these can be calculated also in cylindrical (axisymmetric) geometry. It might bring us to an additional model – Expert H believes that this could be complementary to D&H95. It may be interesting to implement it, in a follow up of this study.

MODEL #1 [D&H98]

tau=1000 Pa, rho(deposit)=900 kg/m3

L=130 km

Vmin=312 km3

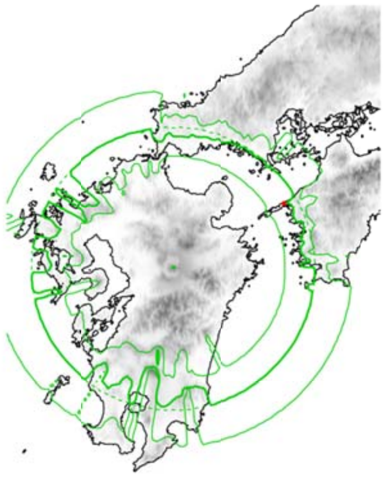


(with ENERGY CONE)
based on H/L

H=2.5 km
[arctan(H/L)=1°]

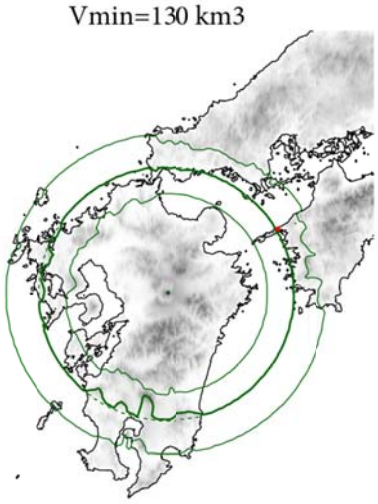
L=145 km

Vmin=434 km3



L=130 km

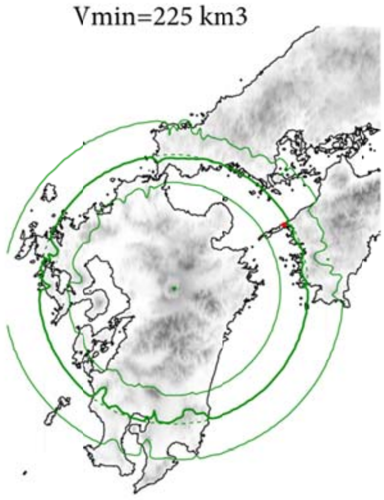
Vmin=130 km3

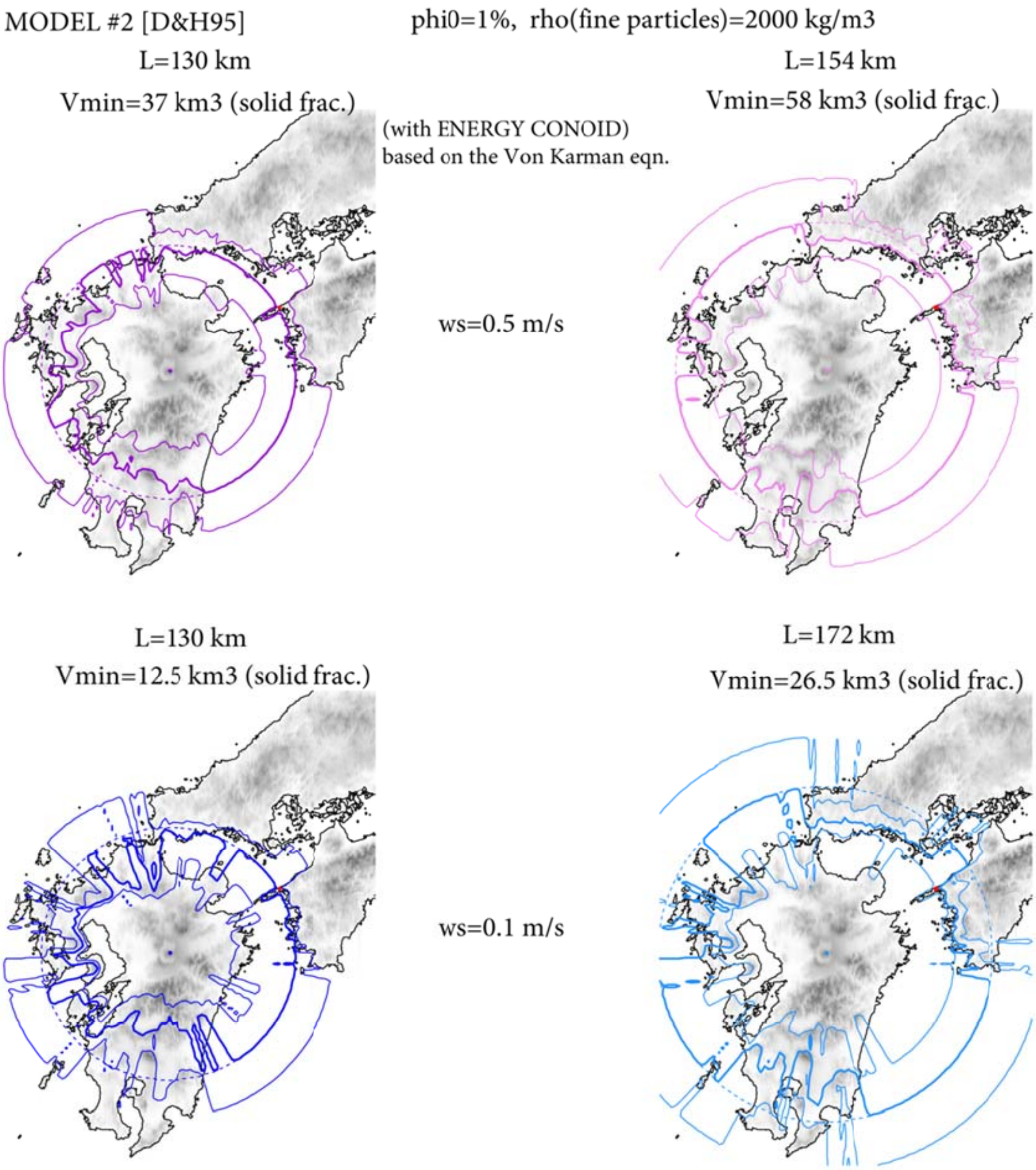


H=6.0 km
[arctan(H/L)=2.5°]

L=156 km

Vmin=225 km3





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29 January
EXPERT H:
Meanwhile on the train back to Bologna I made some rough estimations of the solid fraction phi_0 on the basis of the numerical simulations by Costa et al. (2018), as it is the most difficult model parameter to assess.

To do that I considered the average bulk density of the mixture just before the start of the horizontal calculated as the total mass divided by its volume for mass discharge rate of 10^10 kg/s and 10^11 kg/s (total collapse cases), then I divided that by the average density of particles (1500-2000 kg/m^3). This rough approach suggests slightly lower solid fractions that what we commonly assume, indicate phi_0 in the range of 0.2-1%. I can verify it again later on and may try a better approach to estimate volumes but I don't think we can improve too much and at least in a possible new elicitation I should consider this range as staring point.

Expert B, we may think also to elicit again settling velocity as it can be better constrained and maybe there was a bit of confusion on the estimation of settling velocity as the latter is mainly determined by the effective particle diameter only (being typically fine particles their settling velocity is given by the Stokes law, that is at small particle Reynolds number).

---

30 January
EXPERT B:
This is very useful input.

WRT phi_0, at the moment the elicitation results are: 0.1% (5^{th} %ile); 1.5% (50^{th} %ile); 3.8% (95^{th} %ile). One approach we can use in UNINET is to accept the elicitation distribution as is, and then test what happens if we apply "interval conditioning", say 0.2% – 1.0% and re-calculate. The interval conditioning option allows us to select sampling from a defined sub-range of values within the overall phi_0 distribution. This may be simpler than re-eliciting.

Is it reasonable to assume Ws may be inversely correlated with rho_i, and positively correlated with phi_0? This can be implemented in UNINET, and would apply some model constraint on Ws sample values. We would need to decide appropriate correlation coefficients. Any thoughts? Any other implicit correlations that might/should be included in the model?

---

30 January
EXPERT H:
I think it could be useful to use the range I estimated as "starting point" to infer values for the distribution as we need to consider the limitations on my estimations and the fact we don't exactly which is the correct Mass Discharge Rate. However, elicited values seem not very far! Anyway, I agree you're your suggestion.

Concerning Ws, there is no need to assume a correlation as there is a formula to calculate it on the basis of the diameter and particle density (and shape). The source of uncertainty in this case is mainly related to the estimation of the particle diameter. However, this is clear to Expert C now and he is testing several TGSD assumptions (e.g. MSH, YYT, Campanian Ignimbrite, empirical correlations) and approach to choose the effective diameter (e.g. Sauter, distribution mode) assessing a much more constrained range for Ws.

---

30 January
EXPERT B:
I've made a preliminary run of the Mark2 deposition box model, using the latest pooled elicitation parameter values.

The resulting MinVol distribution is very skewed, with a long, long tail to larger volumes (above 10^5 km3) for travelling 130 km. It may be the elicited ranges are wider than some may feel are justified, so we will likely need a discussion, as well as comparison of this MinVOL with other model results.

I can run the model for, say, 170 km, to accord with Expert C's findings for TS1 incursion.

---

30 January
EXPERT H:
As I mentioned in my previous email, the most inconsistent range is that associated with Ws. Having particle settling velocity as large as the %95 percentile suggests is neither consistent with realistic TGSD nor with model assumption to consider one effective particle class only. I agree we need a discussion.

---

30 January
EXPERT C:
As Expert H was anticipating, there is something to say about ws.

But before that, I am responding to the last emails:

1) Expert B I remark that the Minvol range that you found is so large because it must be multiplied by the volume fraction phi0 to obtain the volume of the solid phase. Because phi0 is varying in your Monte Carlo, it is not easy to split that VolMin between gas and particles. I suggest to calculate the probability distribution of [VolMin*phi0] as the output. What do you think?

2) Concerning the correlations, I would expect ws to be positively correlated to rho (gravity is stronger in the Newton balance defining ws), and negatively correlated to rho_i (gravity is weaker, and drag is stronger, in the same eqn). May we try to see if adding tentative correlation values the skewed feature shall reduce?

3) I have read the paper that Expert H referenced (Costa et al. 2018) and it is a definitely useful source to discuss further about the likely range of phi0 with the others. How can we motivate the higher part of the elicited range of phi0 now?

---REASONING ABOUT WS OF Expert H AND I
First, I remind to you the values of the Sauter diameters of the three cases study of MSH (Costa et al.2016), CI (Costa et al., 2012), Toba 75ka (Costa et al., 2014). They are 33.24 microns, 33.77 microns and 53.37 microns respectively. I calculated them and told them to you in previous emails. Please see pictures of TGSD (mass), and derivate plots of volume and surface area with respect to particle diameters (in phi scale).

Increasing the fines of +40% in the last two eruptions, I got Sauters of 22.79 microns and 34.80 microns, respectively. This modified values are consistent with calculations that Expert H made with a model for TGSD based on mass discharge rate (validated for smaller eruptions).

I can make a few reasoning based on this particle scale:

1) using the elicited range of speed ws \in [0.07 - 2.5] m/s, the viscosity nu of 10^-5 Pa s, and the Sauter diameters d said above, and the formula of Reynolds number Re = d ws / nu, we obtain a pretty low Re \in [0.01 – 0.1].

Looking at the experimental pictures of Cd experimentally measured in Dioguardi et al.2017 (Fig.2 and Fig.3 of the paper), these Re would give a Cd above 100, up to 1000. Not a Cd \in [0.75, 5] like what we were assuming looking at the high-Re part of the figure (realted to larger particles).

2) We can use the following very simple formula: Cd = 24/Re * k, where k is a correction for considering non-sphericity of the particles. From the paper Armienti et al., 1988, we can assume k=0.43^{-0.83}=2, as a first approximation.

The Newton definition of terminal velocity (force balance between drag force Fd and gravity Fg) gives:

ws=sqrt(4/3 gp d / Cp). If I plug-in the Cd approx above, and the definition of Re, I get easily: ws= 1/(18*k) gp d^2 / nu.

Basically is Stokes Fall with the correction k. Because k=2, our terminal velocity is about half the stokes free fall of a sphere in the same conditions.

So, using the Sauters above, we get the following range of ws:
MSH : ws= 0.06 m/s
CI : ws= 0.06 m/s
CI +40% fines : ws= 0.03 m/s
Toba : 0.16 m/s
CI +40% fines : ws= 0.07 m/s

Assuming more spherical particles (as for example from the experimental dataset of Dioguardi), we get results closer to the classical stokes formula, i.e. twice these speeds. However, these are never above 0.3 m/s.

So – why elicitation results suggest speed values up to 2 m/s or above? Maybe we thought about Reynolds numbers larger than those justified by these speed themselves at the sauter size? Or we assumed larger particles than the sauter size may be important in the dynamics of the flow?

Should we ask ourselves if there is some physical reason to expect such larger velocities than those obtained from the particle diameter at the Sauter size?

Please let me know. I hope this is useful.

---

30 January
EXPERT B:
I have managed to run the UNINET calc on the train, and with a new node MinVol * phi_0 we still get a very long-tailed distribution.

31 January
EXPERT C:

Skewness is still a relevant feature the MinVol distribution, but we can at least figure out to what deposit volume this collapsing volumes could correspond (it may be necessary a further adjustment proportional to particle packing).

I would suggest to see what the results would be if we used the original D&H95 model (the ‘old’ version without hot gas, I mean).

MY FURTHER REASONING ABOUT MODEL #1 (D&H98)----

In the meanwhile, I thought again about the D&H98 (rockfall model), and how to find a connection to the Kelfoun assumption (of constant friction tau), and an expression for kinetic energy K as a function of radial distance L from the origin (i.e. an energy conoid).

I got to two different equations. They’re both useful, I believe.

1) Let’s see the work W as the integration of a costant friction τ over circular annuli of constant width $B << L$.

$$W=\int_0^L \tau (2\pi r) B dr= \tau (\pi L^2) B.$$

This is somewhat similar to the Dade and Huppert expression $\tau A L$, where the annulus width B substitutes the distance L.

The formula for MinVol is also similar to D&H98: $MinVol=\tau \pi B (maxL)^2 / (g H \rho)$ ---- i.e. F/L times the original volumes.

The energy conoid K has the formal expression of a parabola: $K(L) = gHM – (\tau \pi B) L^2$.

2) Otherwise, if we assume the work W as the integral of friction τ over the whole previousl inundated region, the integral becomes:

$$W=\int_0^L \tau (\pi r^2) dr= \tau A L /3.$$

This is one third of the Dade and Huppert expression. If we follow this expression, we have to assume the local “Kelfounian” τ to be three times the elicited τ. This make sense to motivate such small number that we found, but conflicts with the original range suggested in the original paper of Dade and Huppert.

The formula for the volume is: $MinVol=\tau \pi (maxL)^3 / (3g H \rho)$ ---- i.e. one third of the original volumes.

The energy conoid K in this case has the formal expression of a polinomial of third grade: $K(L) = gHM – (\tau \pi) L^3 / 3$.

Should we use one or both these results? What do you think?

---

1 February
EXPERT C:
after having completed my calculations based on sauter diameter I would like to revise my response about settling speed ws. Also Expert F maybe wants to revise his.
Can I send to you a revised questionnaire?

Similarly, I would like to slightly revise my response concerning phi0, after having read the paper of EXPERT H about super-eruptions.

---

1 February
EXPERT B:
Yes, please update, both!

If we go back to the first UNINET model (for Expert A's equation for 130 km), and substitute the latest elicitation distribution for rho, we get:
17 km^3 (5%ile) 138 (50%ile) 204 (mean) 615 (95%ile)

The previous values were:
23 230 450 1295 km^3

If we extend this updated rho version to L = 170 km runout, we get:
38 308 456 1375 km^3

In other words, MinVol needs to be doubled to get from 130 km runout to 170 km (recall the latter roughly offsets local topography near TS1).

---

1 February
EXPERT C:
We should not substitute in D&98 the density obtained in the latest elicitation in the, because that is the density of solid particles, while here it should be the density of deposited material. Model #1 was for a granular flow, while Model #2_bis is based on particle deposition with hot gas reducing its runout.
I have the following immediate thoughts:

1) We should calculate the volume results with the 'cold' Model #2. I am sure they're going to be lower than these.

2) Model #1 had a very high H elicited. Now I guess that 9 km may be an excessive H value because we make fall all the mass from that height. H it's not the height of a cylinder that collapses down. We may try to reduce (halve??) the H values elicited.

I also guess that my reasoning about tau would also bring us to slightly different values than those elicited, but I don't think that would be going to significantly reduce VolMin.

1 February
EXPERT H:
Difference between models should be calculated converting all the volumes in DRE in order to make confusion. As suggested by Expert C I would run also the original D&H95 model with not hot air.

---

1 February
EXPERT B:
Can you assist with guidance on DRE conversion(s), please. While I try to restrict my models to DRE values wherever possible, it seems to me conversion of different types of deposit to a coherent corresponding DRE is tricky, entails different relationships, and yet more uncertainties to account?

---

3 February
EXPERT B:
As we need to move on this month to the issue of the <u>probability</u> of a future big Aso eruption, I would be very grateful for your thoughts and guidance on the way ahead. That assessment will be a function of eruption volume potential, in terms of our understanding of reservoir size, state, etc. Thus it is necessary, I think, to bring our current models into alignment, so that the estimates of MinVol can be used to inform the eruption probability question. One fundamental issue is how to determine MinVol in particle density terms, or in converted density (DRE?) terms, so results can be expressed in an appropriate form for the purpose of considering reservoir eruption potential?

At the moment, we have six models in play (I think):
#1 Expert C's energy cone D&H98
#2 Expert C's energy conoid D&H95
#3 Expert A's D&H98 equation (in WA UNINET)
#4 Expert C's one-phase rho_a only thesis equation (UNINET)
#5 Expert C's two-phase equation model (UNINET)
#6 Expert A & Expert I's cellular automata flow model

My first question: is model #4 redundant?

Next, given models #3 - #6 are expressed probabilistically, can we make them coherent with one another in terms of the MinVol values they compute? At present, we may be juggling apples, oranges and bananas. It seems some of the model results may need to be multiplied by phi_0? And, is this the appropriate conversion to unify the results for assessing reservoir eruption potential?

How do we include results from models #1 and #2 into the eruption probability discussion? Might these be treated as "story-book" scenarios, complementary to the probabilistic/stochastic models. I attach an interesting new paper discussing this topic in relation to climate change modelling, which I think has real pertinence to volcano hazard assessment, and may be of value for INGV's crisis thinking.

For the exposure of TS1, I think one very important finding from Expert C's models #1 and #2 is the increased MinVol that is needed to overcome local topography to reach the TS1 -- for instance, the runout across flat distal terrain may need to have a radius ca. 170 km in order for the flow to climb up to the TS1. And, I think this ignores loss of material into the ocean, which would further increase the needed MinVol? (Expert A is developing his CA model to include particle loss into the ocean, but we await his feedback on progress).

I apologise for floating so many questions, and look forward to your thoughts and guidance.

---

3 February
EXPERT F:
A few thoughts:

1) Model#2 and Model#4 should be the same if I understood correctly.
2) Re the volumes and the inter comparison issue probably the easier think is to work in terms of Mass. We have three situations:
a) D&H98 uses directly a mass from a Volume of the initial landslide/granular flow and its bulk density with is the initial density of the landslide/flow.
b) D&H95 uses a collapsing volume of a gas-particle mixture and therefore we can compute the total mass considering the volume of this collapsing volume times the value of phi0 and times the density of the individual particles (assumed single size with Sauter diameter and with ws).
c) The DRE estimates that simply assume a density of the rock without bubbles and so (typically the density of 2500-2800 kg/m^3 depending on the type of magma.
So in other words the M (mass) is fully and simply comparable in the three cases.

---

3 February
EXPERT H:
I agree with Expert F, at the end we can express all our results in terms of mass or DRE (for rhyolitic magma density is 2300 kg/m3; for the deposit density should range 1000/1500 to 2000 at most depending on the degree of compaction).

Concerning models, we have:

#1 Expert C's energy cone, from which, extrapolating the empirical relation between friction angle and volume, we can get a rough estimation of the volume (actually from that relationship we may also estimate the associated uncertainty);
#2 Expert C's energy conoid D&H95, for this we have all the parameters already elicited but I haven't seen the results so far, apart those sent by Expert C;
#3 Expert A's D&H98 equation (in WA UNINET): OK;
#5 Expert C's two-phase equation model (UNINET): OK;
#6 Expert A & Expert I's cellular automata flow model: looking forward to seeing the results.

Concerning parameters, as you know, Expert C and I suggested to repeat the elicitation as we think settling velocity range is not realistic and the model is terribly sensitive to the effective particle settling velocity chosen.

---

3 February
EXPERT C:
Some of the models are indeed redundant in your list, Expert B. It's because we are mixing two different problems.

The two problems are:

1) calculating MinVol based on the elicited parameters and a fixed distance L that we need to reach without considering topography

and

2) seeing the effects of the topography on the required distance L to reach TS1, based on the energy cone/conoid assumption.

Problem 1) is our target. Problem 2) is ancillary to it. In problem 1) each physical model can give us responses related to any L. Problem 2) gives us L.

Actually, to be more precise, problems 1) and 2) should be solved at the same time, because the required L to reach TS1 depends on the chosen parameter sample. So L should be implicitly calculated inside the Monte Carlo.

If we want to do that, I should bring the uncertainty distributions outside UNINET and use them in a Monte Carlo that chooses L sample by sample as required to reach TS1 based on the energy decay and the topography. I am not sure if this 'clean' approach is really necessary here, because it could be an excessive accuracy, given the other uncertainties.

However, please let me know if I should do that. Otherwise, we can limit ourselves to try with L = 130 km and then L=170, fixed. That's much simpler. This values have already been motivated with my 'offline' runs of the energy conoid maps, that we can surely include in the report.

In summary, concerning problem 1), we actually have five models, each corresponding to a different physical equation for MinVol(L):

#0 energy cone based on extrapolation results
#1 D&H98
#2 D&H95 original equation
#3 D&H95 including hot gas
#4 cellular automata flow model

Until now I received only VolMin distributions based on L=130Km or L=170km, and models #1 D&H98 or #3 D&H95_hotGas. I think it would be important to see also the MinVol calculated with model #2 D&H95_original.

The volumes obtained from a model without hot gas are indeed not redundant with any of the other approaches.

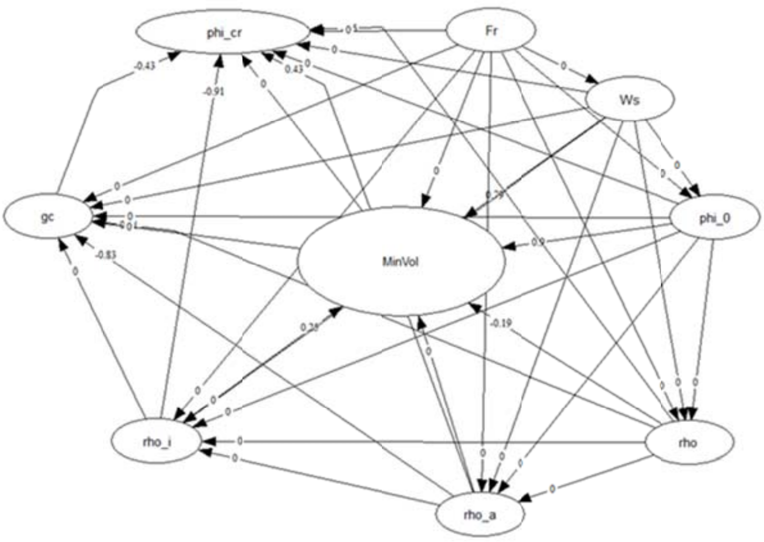
Also a relatively trivial model like #0 energy cone extrapolation could be interesting. We can easily get a distribution for H/L ratio, using the same H elicited from model #1, if this make sense to you. Please let me know and I can do it with Expert H.

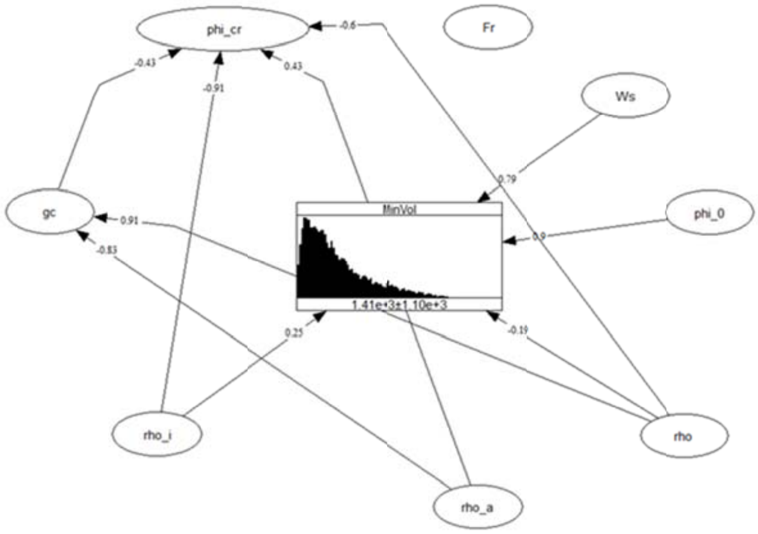
---

4 February
EXPERT B:
Just for info at this stage, the two plots below illustrate UNINET's data mining capability, when applied to the two-phase boxmodel equation formulation (n.b. some parameter distributions need updating for latest judgments).

We start with all variables linked together as a "saturated BBN" (first plot), and then use the UNINET BBN sampling mode to determine correlations between pairs of nodes. In the second plot, all arcs for which abs correlation is less than 0.1 have been removed. For example, Fr (Froude no.) makes a negligible contribution to the problem (as you expected), and only four nodes of the BBN influence MinVol significantly (i.e. Ws, phi_0, rho and rho_i).

At a minimum, this allows us to sanity check the MinVol calculation variable relationships, but you will readily appreciate this is a powerful tool for mining models containing plentiful empirical data of different types.





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4 February
EXPERT C:
I figured out that the 'original' D&H95 without hot gas is simply embedded into the variant model, once we assume:
rho_a=rho_i
(i.e. we can impose the same input distribution and correlation 1)

This is going to imply phi_cr=0, and so, the expression fot VolMin will become exactly the 'original' one. I hope UNINET calculator will allow us to do arctg(+inf)=\pi/2.

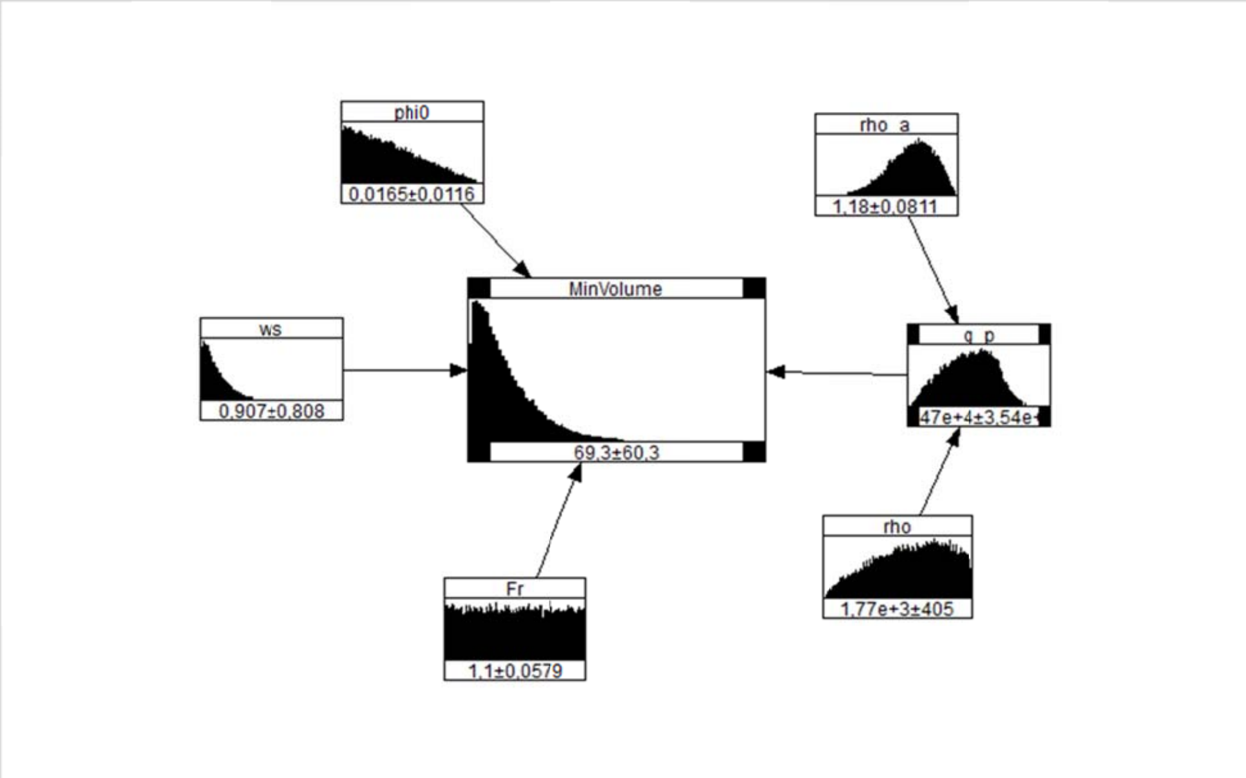
---

5 February
EXPERT C:
You can find attached my UNINET results with the model without hot gas.

In this test I manually fitted variables to the percentiles of the decision maker: phi0 and rho are Beta, Fr is uniform, ws is Weibull. I can repeat the test with the full distribution files, if you think it is useful and you send them to me. This decision maker has not been updated with revised responses yet, so results are preliminary.

This VolMin is related to L=130km. I already multiplied it by phi0 inside UNINET, so Vmin here is just the solid fraction.

I got 5%ile, 50%ile, 95%ile as [6.4, 52.3, 188.5] km3. Please let me know what you need and I shall do it.



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5 February
EXPERT B:
Attached please find five EXCALIBUR ***_3.dis files to use in UNINET; these will have to be the final elicitation products as Japanese are pressing for report and we can't wait any longer for some colleagues to provide their final judgments.

UNINET evaluates atan(10^10) as 1.5708 ….. it doesn't recognise "inf". I look forward to seeing how these compare with your hand-crafted distributions.

---

7 February
EXPERT B:
Just wanted to share a few remarks with you concerning the elicitation *.dis files.

As a matter of mathematical principle, Roger Cooke adheres to the position that an output probability distribution from EXCALIBUR should be expressed with minimal assumptions about the shape of the distribution away from the derived (three) quantiles. Thus, the output dis files are simple piece-wise linear graphs, anchored at five points: the three Decision Maker quantiles, plus the Intrinsic range values determined from the experts' lowest and highest expressed values, extended (usually) by 10%.

Over the years I have endeavoured to fit suitable parametric distributions to such DM quantiles but, more often than not, it turns out to be very difficult to honour all three quantiles simultaneously. Thus it often boils down to the analyst to decide whether a given parametric distribution, albeit not a perfect fit, is a more appropriate choice for modelling the particular variable than the basic *.dis result.

---

7 February
EXPERT C:
I will soon send also an up-to-date BBN based on the hot gas variant model. I wanted to analyze correlation with VolMin too. Yes, actually it was not straightforward for me to define the parametric distributions. For example with the Beta I was not able to replicate the three quantiles of ws, and I had to rely on the Weibull. What confuses me is that the histograms plots based on the nonparametric distributions look "toothed". Parametric histograms 'skyline' looked much smoother. Is this a consequence of the UNINET sampler?

---

7 February
EXPERT B:
I suspect it must be sampler thing: have you tried increasing the no. of samples in sampling mode?

---

7 February
EXPERT C:
Yes, I tried with that. Increasing the samples, parametric distributions get smoother and smoother while nonparametric distributions from dis file do not change at all.

---

7 February
EXPERT C:
I got what's going on here. The .dis file is a cumulative distribution – if you look at the histogram view of that, is really smooth. But there are a few histograms that are flat – this is interpreted as no-density in those regions. And then the sampler ignores them.

So this is a consequence of how EXCALIBUR creates the .dis file. Could you increase the number of steps of the .dis files? I guess that this is going to reduce the issue – I mean, the flat regions will be still there, but at a finer scale they won't affect the histograms in UNINET.

---

7 February
EXPERT B:
The number of percentiles reported to *.dis is fixed in EXCALIBUR. One could apply post hoc interpolation, but isn't this really just an appearance thing, if there is no meaningful added information with finer cumulative probability resolution?

I suppose one argument might be that, if one wants more precise or smoother distributions, one could elicit parameters for any parametric distribution of choice. In effect, that is pursuing the sort of elicitation procedure which the Sheffield group espouse when they ask experts to drag a given graphical distribution shape to reflect whatever parameters the expert wishes. I guess this can work with individual experts who can judge quantiles a priori and then adjust the shape accordingly, and I suppose one could form a weighted combination distribution from the experts' string pullings.

I don't know whether this has ever been done - perhaps we should try it one day! Of course, there is always the question: which parametric distribution to adopt. I think the counter-argument is the three-quantile Classical Model involves much less effort than this, but is still effective

---

7 February
EXPERT C:
I tried a bit if I could reduce the number of bins inside UNINET to better match the fixed number of bins in EXCALIBUR output. It seems that I cannot do it in the 'small plots' over the graph, but only if I select one and open a specific tab. OK let's go ahead without bothering too much of this.

how did you calculate the correlation with the functional variables? I am not finding that option in UNINET. It shows me only the correlation between input variables.

---

7 February
EXPERT B:
You need to save a BBN sample file *.sae, then go to File > New > Data mining model, ad open the SAE file. Normally, for data mining, one would have a samples file from, say, observational data, not from a BBN run.

---

7 February
EXPERT C:
Attached you can find the results of the two models BBNs.

If rho_i=rho_a:
VolMin*phi0 = [4.61 km3, 41.5 km3, 202km3]
A bit lower than yesterday – not sure if this is because of revised responses or of nonparametric input dists.

With elicited rho_i, as you already realized before me, there are some problems with P(phi_cr > phi0) >0. This is not a consequence of our elicited data, but of how EXCALIBUR represents them in the .dis files. In fact, the three percentiles of the two variables were not imposing phi_cr > phi_0.

This is a real problem because if the flow lifts off at t=0, VolMin=+infty…and this creates the excessive skewness that we observed.

I forced (phi0/phi_cr) to be greater than 2, and this is what I get:
VolMin*phi0 = [8.53 km3, 53.1 km3, 227km3]

But if I force (phi0/phi_cr) to be greater than 1.2 instead, what I get is:
VolMin*phi0 = [9.78 km3, 56.5 km3, 238km3]

If I force (phi0/phi_cr) to be greater than 1.02 instead, what I get is:
VolMin*phi0 = [10.3 km3, 58.9 km3, 269km3]

Not a lot different, so that this uncertainty is not changing the order of magnitude of the volume, but it's different. And in the last two cases histogram starts to be awfully skewed (up to tens of thousands of km3 in the third case) because of a bit of mass (related to the event phi_cr > phi_0) tends to +inf.

Not sure if choosing parametric dists is going to solve the problem, if these are fitted over the flawed .dis files. Should I explore that approach?

---

7 February
EXPERT B:
I doubt going to parametric distributions will significantly change upper tails, beyond 95%iles. I think these 95%iles look plausible, and may be we just need to be careful where we truncate/report results beyond these anchor points.

---

7 February
EXPERT H:
I think Expert C's results using the original formulation of the simplified model are very interesting and can help us to better constrain our assessments.

It would be very useful to have a summary to share among all colleagues about the answer of the four models
#1 D&H98
#2 D&H95 original equation
#3 D&H95 including hot gas
#4 cellular automata flow model
plus the empirical extrapolation using the relationship for energy cone model.

Obviously for a potential scientific article we can refine the approach. If we agree to proceed with that I suggest Expert C leading it, but obviously it's up to you.

---

7 February
EXPERT B:
Absolutely agree with your summary and proposal to share results, and for Expert C to compose the musical setting -- I can wield the baton to conduct the chorus!

But I need to chase Expert A to check on his progress introducing material loss into ocean in his CA model.

As far as our Japanese sponsors are concerned, time is of the essence for a report; however, they have also re-hinted that follow-on work in the new financial year is on their radar.

8 February
EXPERT C:
Preparing the summary report, I tried to solve the 'skewness' issues encountered in the model with hot gas. First, I realized that the minimum value of elicited phi0 was 10^-5, where the maximum of phi_cr was 10^-3. Even if in our discussions we never spoke about values of phi0 below 10^-3, this value is at the 6^{th} percentile of the collective DM… a simple rejection of the phi0 values below max(phi_cr) is going to affect the rest of the distribution. However, we have to exclude the case of having (phi_cr > phi0) if we want to avoid the immediate lift-off of the PDC.

Given that another re-elicitation of phi0 is not feasible at this point, I did what follows:

I operated on the .dis file of phi0, and I linearly re-scaled the percentiles 0th-50th so to have the new minimum equal to 10^-3 and the same median. I'd rather do this than truncating and rescaling the entire distribution of phi0. What do you think?

I also included a table with minumum mass, assuming the density values suggested by Expert H. Now the results are pleasantly coherent between the models (given the different assumptions behind them).

Before to share this with the others, I wanted to add a couple of things:

1) If you please provide me the .dis files of the first elicitation (model D&H98), I would like to include a picture of its BBN with correlations.

2) I also want to re-do all the calculations assuming L=170km instead than L=130km. I am sure we're going to get larger volumes, but I would like to see how much larger.

3) If you don't like the alteration of the phi0 .dis file that I performed, we can follow a different strategy. For example, I could conditionalise phi0 > 10^-3. I cannot do that inside UNINET because we are dealing with a nonparametric distribution, but I can do that outside UNINET. However, it would add mass also above the median… and I guess that my strategy is preferable to this because I am not even touching the upper half of the phi0 distribution.

I was planning to include also a short example with phi0 uniform in [0.2%, 1%] based on Expert H 3D model, and ws uniform in [0.04 m/s, 0.3 m/s] based on our Sauter diameter lengthy calculations.

---

9 February
EXPERT B:
Please find attached the original model and DIS file versions which I have on my laptop.
In UNINET, I replaced lambda by a single discrete value distribution = pi.

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9 February
EXPERT C:
I just realized that sometimes when UNINET calculates the correlation of variables (X,Y,W) with variable Z, it's actually giving cor(X,Z), cor(Y,Z|X), and cor(W,Z|X,Y). The conditioning is imposed when choosing the ordering

in the variable list, and the algorithm tends to not conditionalise correlations on the first variables in the list. If I change the ordering, the coefficients change radically...

So I got the unconditional correlations only after input MinVol as the first variable in the list.

---

9 February
EXPERT B:
Sorry, I should have remembered this feature/limitation …. There is a lot going on “under the bonnet” in UNINET! But now you know about it, I’m sure it will be helpful in future applications.

---

9 February
EXPERT C:
My report will include some variants of the models:

1) results based on 130km or 170km maximum distance
2) results based on out sauter diameter-based ws and the MDR modelling-based phi0.

I also including the BBN with correlations, and minumum mass corresponding to the suggested densities. I tried to be as clear a possible with footnotes.

---

9 February
EXPERT B:
To give the experts and our Japanese colleagues a basis for judging the results, can we add the notes on equations etc into the same document, please? (colleagues must translate our reports into Japanese this month ☹)

---

11 February
EXPERT C:
I prepared the summary report including the equations of the three models as requested (including a short new doc about D&H98, following the same style of the other two models). I included the picture of inundated regions too, so that we can motivate the optional choice of 170 km runout distance, beside the 130 km actual distance.

Once I have a couple of hours I was planning to tidy up the LOG – probably at the end of this week. Right now I did not include the details of our reasoning on Sauter diameters into this summary, but only the results of our reasoning. Indeed, that is an optional strategy, alternative to the elicited values of phi0 and ws. I guess that we may want to include that part in a more extended report/paper.

11 February
EXPERT H:
Thank you very much for such a useful summary report! Expert B, when do you plan to circulate it among all? I guess you are waiting for Expert A's model results.

11 February
EXPERT B:
Dear all,
Expert C, with Expert H and Expert F's kind help, has been working hard on three alternative runout equation models, and I am very pleased to copy his interim report to you, for review please.

The point about 130 km / 170 km runout distance is that, in various tests with energy cone/conoid models, Expert C determined that a 130 km runout contour corresponds to the reach of the flow at the coast, whereas it required an additional 40 km of oceanic run to climb the local topography up to the TS1. Thus the necessary erupted volume/mass is greater in the latter situation.

All being well, Expert A will shortly be sending out a link to the cellular automata runout model he and Expert I have been developing, and we hope to get some early (but limited) feedback on that, please. See remark below, about follow-on work … for developing our models further.

Another point to bear in mind, please is that I am being chased hard for draft report sections, so that he can start translating them … by the end of the month. And, because he indicates … are interested in taking this stuff forward in next financial year (i.e. April on), I think our best course is to bring together what we have generated thus far: Expert C has collated the technical exchanges we have had over the attached runout models, Expert D and a Japanese colleague have compiled the reference/information database, and I'm working on the event probability BBN formulation. From our Japanese colleague's emails, there is every hope that follow-on work will be commissioned for … next financial year, which would create opportunities to further develop our approach.

Aso-4: modelling minimum volume and mass

10 February 2019

Elicitation solution #1

Case name: D&H98 08/01/2019

Resulting solution (joint DM distribution of values assessed by experts)

Nr.	Id	Scale	5%	50%	95%	Units
1	Collapse Ht	uni	2566	5752	9629	m
2	Flow density	uni	686.3	992	1511	kg/m^3
3	Stress	uni	244.3	1868	7666	Pa
4	Lambda+	uni	1.945	3.044	3.142	rad

+in the sequel lambda will be fixed to pi unless differently stated.

Elicitation solution #2

Case name: D&H95(optional hot gas) 02/02/2019

Resulting solution (joint DM distribution of values assessed by experts)

Nr.	Id	Scale	5%	50%	95%	Units
1	phi0	uni	0.001789	0.01103	0.03675	
2	Ws	uni	0.04492	0.4405	2.460	m/s
3	rho	uni	1089	1814	2357	kg/m^3
4	rho_a	uni	1.023	1.193	1.284	kg/m^3
5	rho_i	uni	0.3184	0.4853	0.7957	kg/m^3

Modified input range based on MDR modelling and Sauter diameter of analogs

Nr.	Id	Scale	MIN	MAX	Units
1*	phi0	uni	0.002	0.01	
2*	Ws	uni	0.04	0.3	m/s

*these results approximately impose input values in the range [min, median] assessed by joint DM.

Bayes Net calculation results, Volume

Model	Maximum distance	Minimum PDC Volume [km^3]			
		5%ile	50%ile	mean	95%ile
D&H98	130 km	26	263	450	1474
	130 km*	22.4	227	395	1309
	170 km	59.1	588	1007	3297
	170 km*	50.2	507	883	2927
D&H95~	130 km	6.8	43.5	64.5	201
	130 km**	5.1	12.1	12.9	23.0
	170 km	13.8	89.0	132	412
	170 km**	10.3	24.8	26.3	47.1
D&H95~ w/hot gas	130 km	10.2	55.9	77.9	226
	130 km**	7.3	16.7	17.4	30.0
	170 km	20.9	114	159	463
	170 km**	14.9	34.2	35.6	61.4

*variable lambda according to joint DM distribution. It is fixed to pi otherwise.
**modified phi0 and ws based on MDR modelling and Sauter diameter of analogs.
~volume of the solid fraction.

Bayes Net calculation results, Mass

Model	Maximum distance	Minimum PDC Mass [10^{12} kg]			
		5%ile	50%ile	mean	95%ile
D&H98[+]	130 km	26	263	450	1474
	130 km*	22.4	227	395	1309
	170 km	59.1	588	1007	3297
	170 km*	50.2	507	883	2927

D&H95++	130 km	15.6	100	148	462
	130 km**	11.7	27.8	29.7	52.9
	170 km	31.7	205	304	948
	170 km**	23.7	57.0	60.5	108
D&H95++ w/hot gas	130 km	23.5	129	179	520
	130 km**	16.8	40.7	40.0	69
	170 km	48.1	262	366	1065
	170 km**	34.3	78.7	81.9	141

+density of 1000 kg/m^3 (deposit) assumed.
++density of 2300 kg/m^3 (rhyolite) assumed.

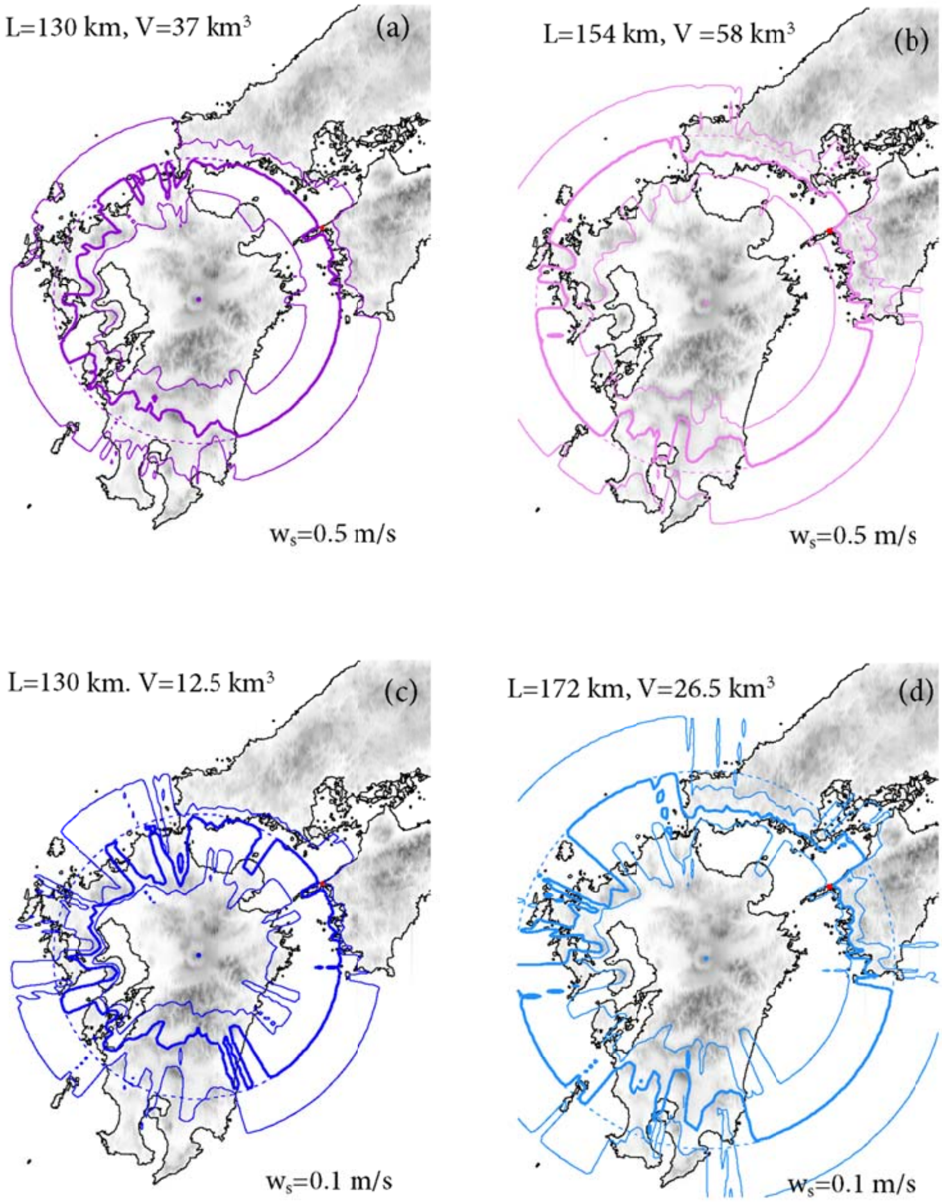
Examples of inundated regions as a function of runout distance L and volume V

The maps are based on the comparison of kinetic energy available to the flow and local topography. Energy equation is based on the equations of model D&H95.

(a), (c) show the inundated region assuming L=130 km^3, (b), (d) show the inundated region assuming the minimum L required to affect the TS1 considering shield-effect of local topography. (a), (b) are based on ws=0.5 m/s, and (c),(d) assume ws=0.1 m/s. All the pictures assume phi0=1% and rho=2000 kg/m^3, and the minimum volume estimate V is displayed.

In all the pictures a colored dot marks Aso volcano, and a red dot marks TS1. A bold colored line marks the boundary of the inundated region based on a volume V. A thin dashed line marks a circle of radius L. Thin colored lines mark the boundaries of the inundated region based on V/2 and 2V, for comparison.

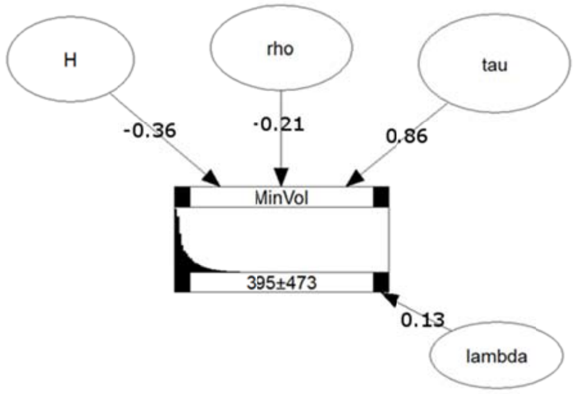
MODEL (2) [D&H95]   phi0=1%, rho(fine particles)=2000 kg/m3



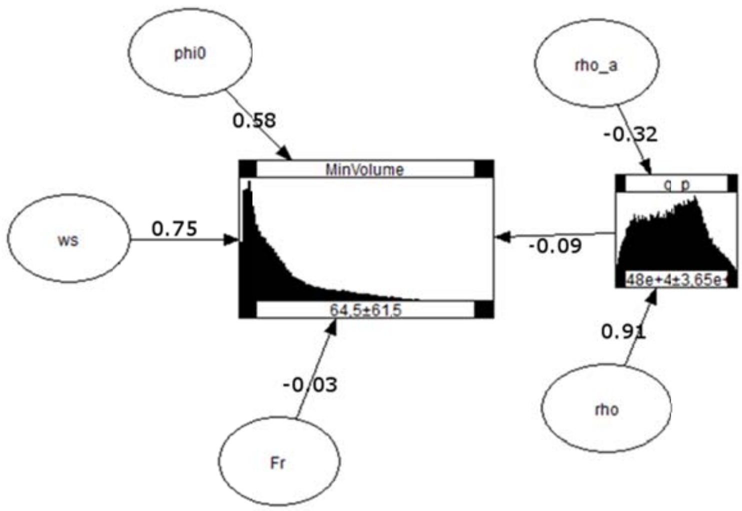
BBN with correlation coefficients

`correlation is based on the joint DM distribution and 130 km maximum distance`

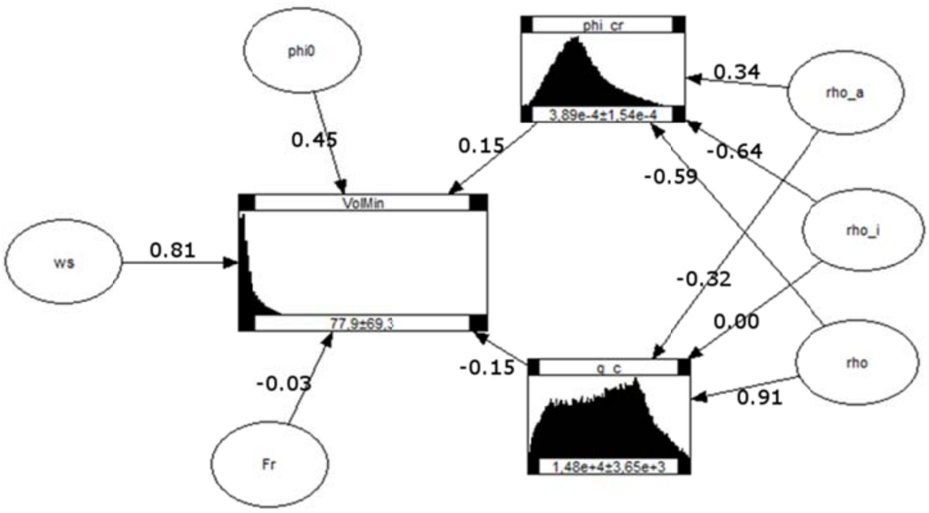
Model D&H98



Model D&H95



Model D&H95 w/ hot gas



11 February
EXPERT G:
Good news on all fronts. I will review the document and get back. Am I right in thinking that our focus is on the pf runout for this project? We haven't had much discussions on probability of an Aso4 eruption or an elicitation etc. It would be useful to know the time line and expected work for the final stages of this project.

---

13 February
EXPERT G:
I have now had the chance to look through the Aso Min Vol document. I get the idea of course and the results are nice. It's terribly cryptic with no references and not a lot of explanation. One finding is that the Dade and Huppert (98) requires significantly larger volumes and I think the turbulent model is more realistic for the ultimate run out so I would weight this 3 times the DH98 model. It occurs to me that in a future project one should take these models and do a much more detailed model near the TS1 to understand how local topography affects the flows. One could take the output of the models at say 120 km and then see what they do after that. I have reservations about the idea that the flows have to go around to the north and cover much more sea as these flows can easily cross ridges. How high is the ridge from the south?

---

13 February
EXPERT B:
Expert weighting of the various models is on my agenda for pursuing shortly, once colleagues have had a chance to explore Expert A's model.

The area of interest is on the north coast of the Sadamisaki Peninsula, which starts > 100 km from Aso. Thus TS1 is on the blindside to Aso, in the lee of a 30 km long ridge that rises to 180 – 230 m elevation directly behind the TS1. If the flow can't surmount this from the SE (but see next remark), it has to travel around the peninsula for more than 30 km along the coast.

Of more relevance for flow direction control, I think, are the hills about 30km NW of Aso, on the line to Sadamisaki Peninsula, which peak with Kayu-san at 1788 m elev (Aso is 1592 m) and the ridge S of Oita City which starts about 40 km from Aso, is 50 km long and reaches 600+ m elevation. Jointly, these topo features will "steer" at least part of an axisymmetric radial flow into two sectors, leaving TS1 in something of a shadow zone. Of course much depends on the effective eruption height at Aso for nullifying these topo highs -- this is one of the intriguing aspects of this case.

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13 February 2019
EXPERT D:
180-230 m doesn't strike me as much of a barrier to a large pyroclastic flow. The surge cloud may well exceed that thickness and the flow of over 30 m/s could surmount those heights. In the next stage of the project focussing on higher resolution modelling how the topography might affect runout and distribution would make an interesting topic.