

1 **Response to the comments of reviewer 1 with regard to the**
2 **discussion paper:**

3 Mergili, M., Marchesini, I., Alvioli, M., Metz, M., Schneider-Muntau, B., Rossi, M.,
4 Guzzetti, F., 2014. A strategy for GIS-based 3-D slope stability modelling over large
5 areas. *Geoscientific Model Development Discussions* 7, 5407–5445.
6 doi:10.5194/gmdd-7-5407-2014.

7

8 **We would first like to thank the reviewer for the constructive comments on our**
9 **paper. It is good to hear that, according to the reviewer, the topic is of interest**
10 **in principle and suitable for the journal. Below we address each comment in**
11 **detail. Our responses are given in bold blue letters.**

12 **Changes in the manuscript, compared to the initial submission, are highlighted in yellow**
13 **low colour.**

14

15 **General Comments**

16 The paper presents a strategy to improve the computational efficiency of a slope sta-
17 bility assessment model, *r.rotstab*, through multi-core processing and strategies for
18 geometrical and geomechanical parameters sampling. This model allows the as-
19 sessment of the susceptibility of slopes to shallow landslides through the computation
20 of factor of safety on potential truncated ellipsoid surfaces of rupture (3D). The paper
21 discussed parallel processing and several strategies for geotechnical parameters
22 samplings. For large areas, a sequential approach would request huge computational
23 times; therefore, the proposed approach is very valuable. The issues raised in the
24 paper are valid for every physically-based model, and in particular for landslide sus-
25 ceptibility assessment.

26 Therefore, the problematic is valuable for the GMD community.

27 However, the description of the background and the model (part 1 and 2.1) are very
28 similar to the paper from Mergili et al. (2014). Hence, the described background and
29 the state-of-the-art are more oriented towards the problematic addressed in the for-
30 mer paper (needs for finite slope, physically-based models) [..]

31 **We agree with the reviewer that much of the general framework is similar to the**
32 **one presented in Mergili et al. (2014). We chose to describe in depth this part to**
33 **present the reader with a self-contained paper. We have reduced this part in**
34 **the revised manuscript, referring the interested reader to Mergili et al. (2014). In**
35 **particular Chapter 1 is now focused on the existing last two paragraphs whilst**
36 **the remaining part was condensed to the absolute minimum necessary. In con-**
37 **trast, it was hard for us to substantially shorten the first three paragraphs of**
38 **Chapter 2.1 because (i) they are absolutely necessary to make the paper self-**
39 **contained and (ii) the reviewer asks for the more detailed explanation of a**
40 **number of aspects exactly concerning this part of the manuscript.**

41 [...] while the state of practice in computational efficiency and sampling strategies is
42 not really discussed. There is no information on why these sampling strategies have
43 been selected and there is an offset between the abstract, background and state-of-
44 the-art and the results and discussions.

45 The methods used (models, parallel processing and sampling) are already existing
46 ones. Therefore, in my opinion, both the subject (computational efficiency) and the
47 model are of interest to be published in GMD, but the paper should be totally restruc-
48 tured to reflect the reflections in sampling strategy and parallel computing before pub-
49 lication. Bibliography should also correspond to computational efficiency.

50 **We further agree that more background and discussion on computing-specific**
51 **topics and sampling strategies could add value to the article. We have modified**
52 **the manuscript (and the reference list) in the way to cover these aspects in suf-**
53 **ficient detail and to bring abstract, background and state of the art in line with**
54 **the results and the discussion. Moreover the use of the sampling strategies**
55 **chosen are better justified in the revised manuscript.**

56

57 **Specific comments**

58 It is not clear enough what are the differences between r.rotstab and r.slope.stability,
59 except the fact that r.slope.stability can be used with multi-cores computers.

60 **Indeed, as explained in section 2.1 (P5413 L18 – P5414 L3), the main differ-**
61 **ences between the old r.rotstab model and r.slope.stability are (i) the capacity**
62 **of r.slope.stability for parallel computing and (ii) the definition of a slope failure**
63 **probability.**

64 No information on computational times is provided. We cannot judge how necessary
65 are parallel processing and parameters sampling. What are the capacities of the
66 computers used?

67 **We have added information on the capacities of the computer used as well as**
68 **on the computational times. In particular we used a 48 cores (AMD Opteron,**
69 **frequency of 2.2 GHz and cache of 512 KB) computer with 140 GB of RAM and**
70 **running a 12.04 LTS Ubuntu GNU/Linux OS with the 3.5.0-26-generic kernel im-**
71 **age. The computational times fluctuate depending on the settings of the exper-**
72 **iments. Some values of T_p are specified in the revised manuscript (section**
73 **5.1): for $d_e=2500$, the computational time is approx. 110,000 seconds with 1**
74 **core and 1 tile, whilst it reduces to approx. 4,700 seconds with 42 cores and**
75 **182 tiles. Further, a new table (Table 5) was introduced, summarizing the eval-**
76 **uation results and computing times of test 2 (Section 5.2)**

77 Global confusion between “surface” and “plane” of rupture. Not every failure occurs
78 following a plane.

79 **We agree with this comment and have replaced the term “plane” with “sur-**
80 **face”.**

81 In order to be clearer in the description of the model, it would be good to mention that
82 no inter-column forces are considered in the r.slope.stability model.

83 **We will mention this important aspect in the revised manuscript but this re-**
84 **quires that we keep a brief introduction to the model from a geotechnical point**
85 **of view.**

86 The randomization process for W and L is not discussed. Do the authors also use a
87 strategy to increase computational efficiency, or is a Monte-Carlo strategy used?
88 What is the methodology used to ensure a proper repartition of ellipsoids over the
89 whole area? Regular or random sampling?

90 **The ellipsoid parameters are sampled randomly. The corresponding paragraph**
91 **was reworked and the following sentence was added to the revised manuscript**
92 **to improve clarity: “Simple pseudo-random numbers, generated separately for**
93 **each parameter of each ellipsoid, are used to define the centre coordinates as**
94 **well as the values of L, W, D and z_b, constrained by user-defined minima and**
95 **maxima for each parameter. Testing a sufficiently large number of ellipsoids**
96 **ensures a proper repartition of the ellipsoids over the study area, and the con-**
97 **sideration of a large variety of possible ellipsoid dimensions.”.**

98 The hypothesis of soil saturation is not discussed, even if it is a quite conservative
99 hypothesis. It seems reasonable to make this hypothesis for the purpose of the pa-
100 per, but the results should be analyzed accordingly. The final Pf maps correspond to
101 “probability” of failure in the worst case scenario, and they do not correspond to cur-
102 rent probability of failure.

103 **This issue is a very important one. In fact, the computed probability of failure is**
104 **valid for the worst-case assumption of fully saturated conditions, as correctly**
105 **stated by the referee. Partial saturation is more difficult to treat from a ge-**
106 **otechnical point of view and shall be the subject of future studies. This limita-**
107 **tion is now clearly stated in the discussion of the revised manuscript.**

108

109 **Technical comments**

110 P2 I22: Is this zone the same one as in Mergili et al. 2014? In this case, why the area
111 is different?

112 **The area given in Mergili et al. (2014) refers to another definition of the bounda-**
113 **ries than the area given in the present paper.**

114 P3 I4: “consisting” instead of “consiststing”

115 **Thank You, this error was corrected!**

116 P3 I6: not all the physically-based models assume the surface of rupture to be a
117 plane. (i.e. circular assumptions with Jambu or Morgenstern-Price approaches).

118 **The term “plane” (issue already raised above) was replaced by the term “sur-**
119 **face” throughout the entire manuscript.**

120 P3 l14: “forces” instead of “forcess”

121 **Thank You, this will be corrected!**

122 P3 l23: references to Baum et al. (2002) and (2010) are missing in the references
123 section.

124 **Thank You – these two references in the text are not necessary and were re-**
125 **moved.**

126 P4 l29: is the notion of large areas really commonly related to number of pixels, and
127 not to sizes, or the existence of several objects (i.e. slopes)? A single slope can have
128 $\sim 10^8$ pixels, according to the resolution

129 **From a computational point of view it is related to the number of pixels.**

130 P6 l10-11: does the offset correspond to the offset mentioned l 15. In this case, it is
131 better to mention z_b l10.

132 **Yes, it is z_b – in the revised manuscript, we use the symbol already in line 10.**

133 P6 l.19 What could be considered as “relatively small pixels”? Could the ratio W/pixel
134 $\text{size} \sim 3$ be considered big enough?

135 **The issue of pixel size was discussed in Mergili et al. (2014) – this reference is**
136 **added at the corresponding place of the revised manuscript. Pixel size = $W/3$ is**
137 **certainly at least at the verge of yielding a shape resembling an ellipsoid. How-**
138 **ever, we note that (i) this is the extreme and most ellipsoids are much larger**
139 **and (ii) more importantly, the ellipsoid is an idealized shape which will not ex-**
140 **actly occur in nature anyway – i.e., if the failure plane is no ellipsoid but some**
141 **kind of polygon, this does not make the geotechnical computations wrong – it**
142 **just means that the shape tested is slightly different.**

143 P8 l5-6: also variability of the geometry parameters (i.e. d)

144 **Yes, thank You, this is mentioned in the revised manuscript.**

145 P8 l16: Does the number “ n ” of samples of samples to be collected correspond to the
146 samples from the ground, (in this case, not consistent with the “ n ” used in the rest of
147 the paper, e.g. p9 l3)?

148 **The number n does not refer to the samples collected from the ground, but to**
149 **the statistical samples. We have collected field data in a much smaller count**
150 **and used that data to build the probability density functions for each of the**
151 **measured soil parameters. The values of the parameters used for each run are**
152 **given by sampling the PDF of the corresponding parameter. We have made this**
153 **aspect clear in the revised manuscript by referring to “statistic samples” which**
154 **are “considered”.**

155 P9 l4: “largest” instead of “lagest”,

156 **Thank You, this error was corrected!**

157 P9 I5: is it correct to consider the probability of failure for a pixel to be the largest P_f
158 computed for the different parameters combinations? The value could be representa-
159 tive of the propability, but not the propability of failure per see.

160 **From a strictly geotechnical point of view (which we follow here), it is the larg-
161 est failure probability out of all intersecting ellipsoids which is interesting for
162 us as, for each pixel, only the most critical ellipsoid is relevant. It is NOT the
163 largest value of P_f out of all parameter combinations we consider – P_f is a re-
164 sult of combining the values of FOS yielded with all parameter combinations.**

165 P9 I20-25: It is not clear how the sampling is performed: for a) and b), you select a
166 different combinations (c' , φ') for each ellipsoid, while for c), you pick one combina-
167 tion, and you consider the parameters homogenous over the whole area? Why don't
168 you use the last option also for a) and b)?

169 **We try explaining this issue more clearly in the revised manuscript. It is not
170 one combination of parameters we use in c), it is rather one set of parameter
171 combinations. As this set is not defined randomly, but deterministically, it is
172 not necessary to define a new set for each ellipsoid. With a) and b), the set of
173 parameters/combinations is sampled randomly for each ellipsoid. Our results
174 seem to show that the regular sampling strategy is an efficient alternative to
175 the classic Monte Carlo approach, at least in our geotechnical / geomorpholog-
176 ical settings, and using the current implementation of r.slope.stability.**

177 P9 I25: Please mention that in the paper, it is this solution (application to three pa-
178 rameters) which is applied. It makes confusion after in sect. 4

179 **The application to three parameters is made clear in the revised manuscript.**

180 P10 I14: According to Eq 4, the density is not in ellipsoids per pixel, but ellipsoid per
181 unit of surface (here, meter).

182 **The average number of ellipsoids per pixel is dimensionless. As long as the
183 pixels are much smaller than the ellipsoids, we consider it a valid approxima-
184 tion with regard to pixels. Equally, it is a valid approximation with regard to any
185 other square unit, as long as the square unit is much smaller than the ellipsoid
186 size (here, square metres would be appropriate).**

187 P10 I 16: "A" is not described here, and appears different from the parameter in Eq2

188 **In Eq. 4, A is the study area size. This is now explained, and the variable was
189 renamed to A_s in order to remove the confusion with A in Eq. 2.**

190 P12 I15: does the inventory correspond only to scarps, to reflect areas of departure?

191 **As most landslides in the Collazzone Area have a limited mobility, we have de-
192 cided to use the entire landslides. Using only the scarps would mean that most
193 of the landslide area is considered as observed negative as the deposit over-
194 lays most of the scarps which can therefore not be defined properly. There is
195 certainly a slight overestimate of observed positives due to this. This aspect is
196 made clear in the revised manuscript.**

197 P13 I24: Please also add the standard deviation of c'
198 **The standard deviation of c' is given in Table 2. However, as we use an expo-**
199 **ponential pdf, it is meaningless for the modelling.**

200 P16 I20: It is good to notice, but isn't it normal? If you don't have a new job to give to
201 an available processor, this processor is somehow useless for the computation.

202 **The referee is right, this is actually clear. The sentence is needed for introduc-**
203 **ing the following sentence, but it was reformulated to "There is, of course, no**
204 **gain in terms of speedup at $p > t$ (not shown in Fig. 5).".**

205 P19 I22 (and Figure 8): which parameter sampling strategy has been selected?
206 **It was equal density sampling (strategy c). This is now clearly mentioned in**
207 **Section 5.2 and in the captions of Figs. 7b and 8.**

208 In the discussion part: the recommendation for $n \approx 9^3$ is valid for area assuming a
209 unique parameterization over the whole area. Would it be the same with soil-type
210 specific areas? In this case, where ranges of variations of parameters could be prob-
211 ably be reduced, would n smaller than 9^3 suitable?

212 **This is a very interesting question. If we could reduce the parameter variations,**
213 **it is likely that also n could be reduced. However, with the data we have right**
214 **now, also the variations within each soil type are quite large. The comment of**
215 **the referee will be an interesting aspect to explore in a future paper, obtaining**
216 **more data (these efforts are going on) and/or conducting theoretical experi-**
217 **ments. A short note on this issue was added to the discussion of the revised**
218 **manuscript.**

219 Would it be possible to consider different soil water content conditions?

220 **In principle it would be possible and interesting, but (see above) partial satura-**
221 **tion is more difficult to treat from a geotechnical point of view and shall be the**
222 **subject of future studies. This is clearly stated in the discussion of the revised**
223 **manuscript.**

224 In references, Iverson and Major (1986) is not referenced in the text.

225 **Thank You – the reference has been removed from the bibliography.**