DISCUSSIONS AND CLOSURES

Discussion of “New Approximations for SORM: Parts 1 and 2” by Yan-Gang Zhao and Tetsuro Ono


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The discusser was reading with interest the attempt of the authors to improve SORM concepts. Unfortunately, in his opinion, this attempt failed. Additionally, the discusser believes that the principal curvatures of the functions defined by Eqs. (24)–(25) in Part 1 has the form

\[ f(\mathbf{x}) = \sum_{i=1}^{n} \beta_i x_i^2 \]

and at

\[ f(\mathbf{x}) = \sum_{i=1}^{n} \beta_i x_i \]

This is, as the discusser sees it, exactly equal to Eq. (6) in Part 1.

Then the authors try to find a SORM approximation which avoids the eigenvalue analysis and the matrix rotations. They propose the use of the mean curvature, but they give no theoretical foundation why this approximation is good in an asymptotic sense. The discusser showed already some years ago that the asymptotic approximation proposed by him can be calculated without eigenvalue analysis and rotations (Breitung 1992; Breitung and Faravelli 1996), so he does not understand why the authors make this statement and propose an approximation without any mathematical justification.

Then the authors propose an empirical reliability index. The discusser does not understand the meaning of this concept. The other reliability indices have a clear probabilistic meaning. What is the probabilistic meaning of an empirical index? If it is defined only in an empirical way, should one change the definition of this index if additional examples are included? And if not, why is it called empirical?

Another misunderstanding of the authors is that they apparently believe that a good SORM approximation should give reasonable approximations also for small beta values less than 2. The SORM concept developed by the author shows that these approximations are based on asymptotic considerations and should give good approximations for high beta values. It is certainly not possible to derive a simple approximation for quadratic functions which is good for all values, for smaller beta values it appears more reasonable to use the method of Tvedt (1990).

In Part 2 of their papers the authors propose a new method for calculating beta values called IFFT. They make no comparison with the existing methods. In a sort of benchmark study Liu and Der Kiureghian (1991) have compared the existing methods. If the authors believe that their method is an improvement compared to these, why have they refrained from making any comparisons with the existing methods? Their proposal to use the fast fourier transform is not new. This was proposed already in Sakamoto et al. (1997).

The discusser does not see the advantage of fitting a quadratic surface with no cross terms instead of a linear plane as approximating limit state surface. The number of parameters is increased from \( n+1 \) to \( 2n+1 \), but the authors do not explain why this quadratic surface without cross terms gives a better approximation or speeds up the convergence.

Then the authors claim that their method requires less function evaluations in each step than a method which calculates the gradient of the limit state function. For calculating the point-fitted performance function in Eq. (1) in Part 2, \( 2n+1 \) function evaluations are needed. The same number of functions is needed for calculating the gradient by the usual numerical algorithm of calculating in the direction of each axis at a point with \( x_i \) replaced by \( x_i + h \) and at a point with \( x_i \) replaced by \( x_i - h \) the function values and then approximating the partial derivative by the difference divided by \( 2h \).

The discusser would be quite grateful to the authors if they could correct the wrong mathematical statements in their paper. Concerning the empirical reliability index and the IFFT method he sees, as outlined above, no reason why these concepts should be used instead of the already existing indices and approximation methods.
methods, but he would appreciate it if the authors could give more convincing additional arguments why one should use these methods.

References


Closure to “New Approximation for SORM: Parts 1 and 2” by Yan-Gang Zhao and Tetsuro Ono


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The writers appreciate the discusser’s interest in their work. The discussion offers some interesting insight and corrections of the curvature statements made with regard to the previous studies, which are very much appreciated. In the writer’s opinion, however, the discusser’s overall skeptical views of the proposed improvements are not justified. The reasons are briefly stated as follows.

Concerning the statement “the result of Tvedt (1983) is more accurate” than Breitung’s formula, the statement in the paper referred to Eqs. (24) and (25) of Part 1. Since all the contents of Part 1 are about the computation of failure probability for a given second order performance function in the standard space, which clearly corresponds to the second case mentioned by the discusser, as the discusser admitted in his discussion, “the result of Tvedt (1983) gives an improvement” in this case.

Concerning the simple quadratic approximation proposed to avoid the eigenvalue analysis and matrix rotations, the discusser mentioned that the proposed approximation is “based on asymptotic considerations and should give good approximations for high beta values,” but “give no theoretical foundation why this approximation is good in an asymptotic sense.” This is probably a misunderstanding on the proposed approximation. The writers wish to take this opportunity to affirm that their proposed approximation is not based on the discusser’s asymptotic considerations, and they do not understand why they have to give the “theoretical foundation why this approximation is good in an asymptotic sense” as $\beta_F \to \infty$. In fact, as shown in Figs. 3, 6, and 9, the accuracy of the proposed approximation is not so sensitive to $\beta_F$ values. The discusser also claimed that he already showed that his asymptotic approximation can also be calculated without eigenvalue analysis and rotations. The writers were unable to have a copy of the discussion’s paper at the time of writing this closure, and therefore we do not know whether their proposed approximation is the same as, or simpler than the discusser’s method. They are looking forward to having an opportunity to conduct a comparison with the discusser’s method.

Concerning the probabilistic meaning of the proposed empirical reliability index, as have indicated in the paper, it is an empirical approximation of integration (14) rather than from some “examples” as the discusser probably misunderstood. The writers agree that mathematical justification is very important, but in the writer’s opinion, practical applicability is also very important. The simplicity and accuracy of the proposed approximation have been shown through the numerical examples of the paper.

The discusser mentioned that it is a misunderstanding of the writers that “a good SORM approximation should give reasonable approximations also for small beta values less than 2.” The writers wish to take this opportunity to affirm that this viewpoint is the writer’s belief rather than a misunderstanding of SORM. In the writer’s opinion, a good SORM approximation should give reasonable approximations not only for small $\beta_F < 2$ but also for a large range of curvature radius $R$, the number of variables $n$ and first-order reliability index $\beta_F$, and this is not impossible as indicated in the paper. Figures 6 and 9 of Part 1 show that while Breitung’s formula provides significant errors, the present approximation gives reasonable results even for small $\beta_F$ values.

About the IFFT method, the discusser mentioned the writers “make no comparison with the existing methods.” In fact, however, the comparison with MCS can be found in Example 2 of Part 2, and the comparisons with indices of closed form can be found in Examples 2 and 4 of Part 1. The result comparison with Tvedt’s integration method is not conducted since both of them are accurate methods and they should give the same results despite computational error. Concerning the originality of the IFFT method, it should be noted that IFFT is suggested as a SORM approximation in the paper. As a probability analysis method, Sakamoto et al. (1995) have already been listed in the references.

Concerning the point fitted quadratic surface with no cross terms. It is an obvious fact that better results can be obtained by adding cross terms, but more computational effort is certainly required. There is no such statement “quadratic surface without cross terms gives a better approximation or speeds up the convergence” in the paper as the discusser probably misunderstood. Furthermore, as indicated in the paper, the purpose of developing the point-fitting SORM is to avoid the computation of the Hessian matrix, and this purpose is reached by using a response surface approach (RSA) in the standard normal space. Since the advantages of RSA have been discussed in many papers (although the writers have not claimed that “their method requires less function evaluations in each step than a method which calculates the gradient of the limit state function” as the discusser mentioned) the writers believe that the point-fitting SORM is more convenient because the computations of the Hessian matrix and the gradients of the limit state function are not required.

Concerning whether the proposed approximations “should be used instead of the already existing indices and approximation method.” In the writer’s opinion, it should be judged by the prac-
titioner rather than the discusser or the writers. It would be very helpful if the discusser provides some examples for which the proposed approximations are worse than the existing indices in the closed form equation including Breitung’s formula. Although there are still some disadvantages and further improvements are still required, as indicated in the paper, the writers believe that the proposed approximation are appropriate compared to the existing indices and approximation method.

Despite the overall skeptical views of the proposed approximation, the writers deeply appreciate the discusser’s corrections of the curvature statements made on the previously studies. As the discusser outlined, the principle curvatures corresponding to Eqs. (2) and (5), Eqs. (6) and (30) are the same at the design point.

Errata. The following corrections should be made to the original paper:

Page 80, 4th paragraph on the right, the description “the difference in the principal curvature between (2) and (5), and” should be eliminated.

Appendix II and the sentences that include the words “Appendix II” should be eliminated.