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Discussion

Answer to Comments on "A survey on evaluating the fatigue limit under multiaxial loading" [Int J Fatigue 33 (2011) 153–165]

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1. Introduction

This document constitutes an answer to a four-page commentary [1] made by Prof. Luca Susmel and provided to me by Prof. Neil James, one of the co-editors of the International Journal of Fatigue. Prof. Susmel opposed the publication of my paper "A survey on evaluating the fatigue limit under multiaxial loading" [2], criticized particular points of my implementations of his method, and proposed that the readers

> "*...fully disregard what is written by Jan Papuga in his paper, simply because those considerations and assertions are wrong*". [1], p. 3 of 4

It is now my turn to publicly proclaim my findings concerning our **scientific** dispute.

2. Mean stress effect

The problem with the d_{Su} coefficient (Papuga) or the m coefficient (Susmel) for weighting the mean normal stress effect has to be clarified first. I will cite from my original paper:

> *"Susmel does not state in [3] how to set the d_{Su} parameter. His only commentary concerns its expected range (0–1) and a proposal for its value for various tested materials, which cannot be accepted as a general approach. In order to optimize the criterion results in some way, the value d_{Su} = 0.35 was set hard in this evaluation. Apparently, such a solution slightly underestimates the mean stress effect (Table 6), but the overall trends are acceptable."* [2], p. 160

I will attempt to explain in the following text why I can hardly change anything in the wording of this paragraph. In his response [1], Susmel refers to another paper of his [4] and to his book [5] (which was published shortly before my paper was finished). Though I explicitly noted the source of my knowledge about his method, Prof. Susmel is clearly right that I could have checked all outputs of his prolific publication career, and that I had failed to do so. Nevertheless, let us continue with his proposal on retrieval of the d_{Su} term value, which is set to be "*embarrassingly trivial*" ([1], p. 1 of 4):

$$d_{Su} = \frac{C_a}{N_m}\left(2 \cdot \frac{t_{-1} - C_a}{2 \cdot t_{-1} - f_{-1}} - \frac{N_a}{C_a}\right). \quad (1)$$

The notation used throughout my paper [2] was used here. The formulas for the mean stress effect parameters are usually derived on the basis of a third fatigue limit in repeated axial loading f_0 in addition to the already used fatigue limits t_{-1} and f_{-1} in reversed torsion and reversed axial loading, respectively. What is unusual here in Eq. (1) is the dependency of the final d_{Su} term on N_a, N_m and C_a critical plane stress components. Technically, other criteria could also be arranged in this way, but this would be of little use for the final users. Knowledge on the critical plane orientation is expected, so that the stress components can be evaluated and inputted into the formula above. Susmel defines the critical plane by the maximum shear stress range (MSSR) encountered during loading. In the case of pure uniaxial loading, the MSSR plane orientation can really be evaluated by calculation in hand e.g. with the help of Mohr's circles. More specifically, for the case of the fatigue limit in repeated axial loading f_0, which is the most often used for weighting the mean stress effect, the final formula derived from it is:

$$d_{Su} = \frac{2 \cdot t_{-1} - \frac{f_0}{2}}{2 \cdot t_{-1} - f_{-1}} - 1, \quad (2)$$

Susmel would have made it simpler for his many readers to read his papers if he had provided this formula, like other authors, who separately weight the mean stress effect (see [6–8]). His attempt to provide a more general formula Eq. (1) is ultimately ineffective, because it works easily only for cases of pure uniaxial loading. Otherwise a more complicated analysis for detecting the MSSR plane has to be realized. Once there is two-channel loading with a mean stress effective at one of the load channels, the loading is no longer proportional, and MSSR plane detection is no longer so simple. I myself had to use PragTic features, which should not be required of other readers. **If there is indeed some simple way to compute d_{Su} in such cases, which I am not aware of, I challenge Prof. Susmel to provide it to the readers in a closed form with the dependency only on actual nominal fatigue limits.**

An interesting consideration is that Susmel is apparently either capable of doing this, or is aware of the complexity noted here, because he made such a detection in [3] for load cases involving several load channels and mean stresses simultaneously. Several sets of experimental results are evaluated there. According to its appearance in the text of [3], this data was originally presented by:

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Table 1
Some of the Ggh experiments, when evaluated for detecting the MSSR plane and deriving the d_{Su} parameter.

Test mark	φ (°)	ϕ (°)	C_a (MPa)	N_a (MPa)	N_m (MPa)	d_{Su} (–)	R_{cp} (–)	ρ_{eff} (–)
Ggh02	2.356	1.570	276.5	276.5	133.2	0.40	−0.35	1.19
Ggh03	2.356	1.570	266.4	266.4	266.4	0.32	0.00	1.32
Ggh09	0.768	1.755	278.0	278.0	297.3	0.16	0.03	1.18
	2.356	1.570	278.0	278.0	−36.7	−1.33	−1.30	1.18
	0.756	1.332	278.0	277.9	293.3	0.17	0.03	1.18
	2.443	1.175	278.0	277.9	−9.3	−5.23	−1.07	1.18
	3.428	0.828	278.0	277.7	183.2	0.27	−0.21	1.18
Ggh10	2.883	0.820	278.0	277.7	42.4	1.16	−0.74	1.18
Ggh11	0.768	1.755	234.8	234.8	430.5	0.39	0.29	1.72
	2.356	1.570	234.8	234.8	96.5	1.76	−0.42	1.72
	0.000	0.785	234.8	234.5	266.1	0.64	0.06	1.72
	0.756	1.332	234.8	234.7	426.5	0.40	0.29	1.72
	2.883	0.820	234.8	234.5	221.3	0.77	−0.03	1.72
	2.443	1.175	234.8	234.6	123.8	1.37	−0.31	1.72
	3.428	0.828	234.8	234.5	316.3	0.54	0.15	1.72
	5.585	1.175	234.8	234.6	123.8	1.37	−0.31	1.72
	0.508	0.942	234.8	234.5	360.5	0.47	0.21	1.72

- Axial and torsion load combination:
  - Gough [9] (Ggh in Table 1 of [2]).
  - Lempp [10] (Lem in Table 1 of [2]) – Susmel refers to the appearance of this set in Kaniut's PhD thesis [11].
  - Heidenreich, Richter and Zenner in [12] (HRZ in Table 1 of [2]) – Susmel refers to a later appearance of this set in [13].
  - Froustey and Lasserre [14] (FLB in Table 1 of [2]) – though Susmel refers to the same paper, he omits two important experiments (FLB11 and FLB12 in [15]), which could serve for retrieving d_{Su}.
  - Simbürger [16] (SiB in Table 1 of [2]) – Susmel wrongly refers to this data set as originating in Issler's PhD thesis [17]. Issler's thesis preceded [16] in reality, and does not correlate with it at all.
- Biaxial loading potentially also including torsion:
  - Mielke [18] (Mi in Table 1 of [2]) – Susmel refers to [13], which I cannot confirm. Unfortunately, the set provided by Susmel is substantially shortened and above all the experiments Mi02, Mi04, Mi05 and Mi17 (all as marked in [15]) that could be used for defining d_{Su} are not reproduced here (Mi02 and Mi04 are uniaxial load cases, i.e. perfect candidates for in-hand calculation).
  - Issler [17] (Iss in Table 1 of [2]) – Susmel refers wrongly to [14], which has no relation to it at all. Once again, the set is substantially shortened and experiments Iss01, Iss03, Iss04, Iss07 and Iss08 are not reported at all.
  - Heidenreich [19] (Hei in Table 1 of [2]) – Susmel refers to [13], which I cannot confirm. Once again, some of the experiments (the uniaxial experiments with non-zero mean stress, above all) are not included.

First, Prof. Susmel's careless attitude towards original data sources and citing these sources should be noted. If I have been blamed for not reading all his papers, Prof. Susmel should reflect whether his own work, in this respect, is above reproach.

Second, the selection of the experiments used for deducing d_{Su} has to be checked. In the case of the Ggh data set, Susmel reports that he derived this parameter from an experiment with mean axial stress 266.3 MPa (Ggh02 in [15]). No reasoning is given for not using any other test from this group (at least Ggh03 relating to the fatigue limit in repeated loading would be a perfect candidate). Let us check several experiments from [3] for the resulting values of the d_{Su} coefficient, if it is derived from them – see Table 1.

The table describes the individual tests with some of the MSSR planes found by PragTic. The Euler angles φ and ϕ are defined in the same manner as used by Papadopoulos et al. in [20]. The same paper [20] explicitly describes how the stresses on an evaluated plane defined by these two angles have to be computed for the combination of axial and torsion loading. Thus anybody can check the content of Table 1. **If Prof. Susmel detected a plane with a higher shear stress amplitude I am ready to admit that something is wrong with PragTic. If not, my question is: What is the procedure for selecting the test for determining d_{Su}?** Apparently, only a small number of experiments from the whole data set allow d_{Su} to be defined in the range between 0.16 and 1, if holding within the predefined limits 0–1 [3]. Susmel derives d_{Su} = 0.41.

Let us continue to Lempp's data [10]. Because no purely uniaxial load case is referred, there is no apparent reason for selecting the Lem08 test. All experiments from the Lem test set including some mean stress are evaluated in Table 2. Apparently the value d_{Su} = 0.35 derived by Susmel is on the border of the real range 0.3–1 (if holding within the predefined limits). By the way, Lempp in [10] explicitly reports the found fatigue limits in repeated bending and in repeated torsion to be f_0 = 630 MPa and t_0 = 450.3 MPa, respectively. The use of the fatigue limit value for repeated bending results in d_{Su} = 0.63.

Table 2
Some of the Lem experiments, when evaluated for detecting the MSSR plane and deriving the d_{Su} parameter.

Test mark	φ (°)	ϕ (°)	C_a (MPa)	N_a (MPa)	N_m (MPa)	d_{Su} (–)	R_{cp} (–)	ρ_{eff} (–)
Lem05	−0.403	1.570	184.4	132.9	−92.1	−1.04	−5.52	1.24
	1.168	1.570	184.4	133.1	92.1	1.04	−0.18	1.24
Lem06	0.714	1.209	141.5	183.9	117.6	0.77	−0.22	1.94
	5.569	1.209	141.5	183.9	−117.6	−0.77	−4.55	1.94
Lem07	0.405	1.570	229.8	166.5	114.8	−0.46	−0.18	0.49
	1.976	1.570	229.8	166.5	−114.8	0.46	−5.44	0.49
Lem08	−0.403	1.570	193.8	140.2	236.9	0.30	0.26	1.09
	1.168	1.570	193.8	139.8	43.1	1.64	−0.53	1.09
Lem09	2.958	0.803	135.5	137.7	135.4	1.03	−0.01	2.04

We can continue with such analyses from one test set to another, but it is now Prof. Susmel's turn to prove that he applied some rigorous method for determining the d_{Su} parameter. My conclusion at this moment is that I was right in writing that there is no precise method for determining d_{Su}.

Nevertheless, if not focused only on the procedure, I do not understand Prof. Susmel's work with Mielke's data set [18]. I have already noted that several experiments described in Mielke's PhD thesis are missing in its transcription to [3], but this can be attributed to the secondary source of data [13] which Susmel used. Susmel clearly marks the experiment (Mi06 in [15]) that served him for deriving d_{Su} and provides the final value $d_{Su} = 0.43$. The problem is that the value that I have derived for the found MSSR plane $d_{Su} = 0.83$, if using the lowest value available among various MSSR planes (see Table 3) for this experiment. If the prediction results for both values of d_{Su} are checked in Table 4, we see that the lower value of the mean stress coefficient results in a distribution that is nicely centered around zero, in contrast with the results provided for the real value. Fig. 9 in [3] shows quite clearly that the value $d_{Su} = 0.43$ provided by Susmel is not a typing error, because a well-centered distribution can also be observed there.

My next question for Prof. Susmel is therefore whether he can provide some supporting facts for using $d_{Su} = 0.43$ here, and the way in which he derived it. Until he does so, I do not see any reason for not evaluating the d_{Su} parameters as set in order to obtain the best-fit distribution of the prediction results on the experimental data. Further, this casts some doubts on the only supporting research-like argument in Prof. Susmel's commentary [1] – Fig. 1.

Table 3
Some of the Mi experiments, when evaluated for detecting the MSSR plane and deriving the d_{Su} parameter. Mi06 is the only experiment referred by Susmel in [3], where it is used for deriving d_{Su}.

Test mark	φ (°)	ϕ (°)	C_a (MPa)	N_a (MPa)	N_m (MPa)	d_{Su} (–)	R_{cp} (–)	ρ_{eff} (–)
Mi02	2.356	1.570	168	168	75	0.57	−0.38	1.25
Mi04	0.000	0.785	130.5	130.4	224.8	0.59	0.266	2.01
Mi05	0.000	2.356	144.5	144.7	170.2	0.62	0.081	1.73
Mi06	0.000	2.356	130.5	130.6	85.09	1.55	−0.21	2.01
	0.035	0.785	130.5	130.5	79.07	1.67	−0.25	2.01
	0.686	0.785	130.5	130.5	113.5	1.16	−0.07	2.01
	1.319	0.785	130.5	130.5	159.7	0.83	0.1	2.01
	2.010	0.785	130.5	130.5	153.6	0.86	0.081	2.01
	3.734	0.785	130.5	130.5	110.3	1.20	−0.08	2.01
	4.313	0.785	130.5	130.5	144.1	0.91	0.05	2.01
	4.964	0.785	130.5	130.5	159.7	0.83	0.1	2.01
	5.623	0.785	130.5	130.5	115.6	1.14	−0.06	2.01
Mi17	−0.393	1.570	166.17	117.5	435.6	0.22	0.575	1.29
	1.178	1.570	166.17	117.5	269.4	0.36	0.393	1.29

Table 4
Results of the newest formulation of the Susmel's criterion [5] for the test data used by Susmel in [3]. The two variants differ in the value of the d_{Su} coefficient.

	ΔFI (%) $d_{Su} = 0.83$	ΔFI (%) $d_{Su} = 0.43$
MIE06	0.23	−10.26
MIE07	10.31	10.31
MIE08	21.25	8.73
MIE09	8.47	−2.37
MIE10	−1.52	−1.52
MIE11	−4.62	−4.62
MIE12	2.41	2.41
MIE13	9.09	5.32
MIE14	1.10	1.10
MIE15	20.47	6.84
MIE16	19.74	9.73

3. Stress ratio

If I think that I did not make an error in evaluating the mean stress effect in the case of using the stress ratio

$$\rho = \frac{N_a + d_{Su} \cdot N_m}{C_a}, \tag{3}$$

I have to admit I did not read Susmel's paper [3] carefully enough. I did not note the condition:

$$\rho \leqslant \rho_{\lim}, \tag{4}$$

which is explicitly stated in his book [5] to be:

$$\rho_{\lim} = \frac{t_{-1}}{2 \cdot t_{-1} - f_{-1}}. \tag{5}$$

Now, if the fatigue limit ratio κ is introduced as in [2]:

$$\kappa = \frac{f_{-1}}{t_{-1}}, \tag{6}$$

the final and more comprehensive formula can be achieved:

$$\rho_{\lim} = \frac{1}{2 - \kappa}. \tag{7}$$

I have found that Lazzarin and Susmel stated the condition Eq. (4) not in their very first paper [20], which was my primary source when I started to implement the criterion into PragTic, but later, in [21]. Here they reflected for the first time what high ρ values mean, when the limit state in Eq. (5) is the highest value attainable by their criterion, but the real local loadings on the MSSR plane exceed this value. Their final conclusion was then:

> *"In any case, when ρ is much greater than 1.0 the problem is very complex and a greater effort has to be made in order to understand better the material multiaxial fatigue behavior in the presence of high values of the stress component normal to the critical plane. An interesting contribution has recently been given by Kaufman and Topper[37], who experimentally demonstrated that when the tensile mean stress exceeds a certain material-based threshold, a further increase of this stress component does not decrease the material fatigue strength."* [21], p. 1175

A year later, Susmel published another paper [22], where he stated the following condition for the applicability of the method:

> *"Finally, it is important to highlight that, when applied to smooth components, this approach can be used to estimate both uniaxial and multiaxial fatigue limits, provided that the method is applied within its range of validity (Susmel* et al. *unpublished data). In particular, it can be safely used within a ρ interval ranging from 0 up to $\rho_{\lim}$, where (Susmel* et al. *unpublished data)[21]:...*" ($\rho_{\lim}$ definition from Eq. (5) here follows) [22], p. 397

By the way, I have not seen such a reference to unpublished data elsewhere before. In any case, at this moment, the criterion is still not applicable for quite a number of load cases on various materials (the usual condition, when Eq. (7) is violated, is loading with pronounced mean stress, but non-proportional loadings without any mean stress effect can also be found).

Because Susmel understood the limitation and its impact on the usability of the criterion, he continued in his research and a new paper was published [23]:

> *"In our opinion, the main drawback of extending the critical plane approach to situations having ρ is that the critical plane theory is based on the assumption that the shear stress amplitude is the main parameter controlling the fatigue crack initiation.... it is logical to suppose that the critical plane approach alone can be applied as long as the shear stress amplitude remains significant compared to the maximum normal stress value."* [23], p. 933

Technically, I do not see any reason why high normal mean stress in conjunction with a small shear stress amplitude cannot initiate some nice crack without getting beyond the scope of the applicability of some criterion. Apparently, the authors of [23] were still trapped in the concept of the MSSR type of critical plane definition and had failed to notice that there is also the MD family of critical plane criteria looking for Maximum Damage as the decisive variable for the critical plane orientation.

Let us continue:

"*...using engineering pragmatism, the use of our method can be extended beyond the limit express by* "Eq.(7)"*: the method is not that accurate, but predictions are always conservative.*

In our opinion the reason why our criterion accuracy drops down when ρ is larger than ρ_{lim} is due to the fact that, in these situations, the behavior of the material is strongly influenced by its static properties. Under high values of the ρ ratio, two fatigue limits determined under fully reversed fatigue loading are pieces of information that are not powerful enough to allow the material strength to be accurately predicted. For this reason, and as said above, the classical criteria accounting for the presence of non-zero mean stresses must be calibrated by using a fatigue and a static property." [23], p. 937

Thus, even in 2005, the authors used the same pragmatic approach as I did in [2] and, in fact, got the same over-conservative result. By the way, at that moment the load ratio was still defined as:

$$\rho = \frac{N_{\max}}{C_a}, \tag{8}$$

as in the first paper [20] (i.e. $d_{Su} = 1$), which certainly could also lead to the general conservatism of the output.

In my view, there are load cases in which the MSSR condition should not be preferred for determining the critical plane. One logically looks for the static analogy, where for example the use of the maximum principal stress is preferred for brittle materials, but this need not be the only case. I do not see any reason to include the static parameters, but a switch to another critical plane definition could be more viable, as I recommended in [2].

There is no substantial elaboration, either of the mean stress effect (kept still in the form of Eq. (8)) or of the stress ratio limit violation in the following papers of Susmel, until the publication of the quite new paper [3] in 2008, from which I used the new mean stress effect definition (Eq. (1)) and failed to read further to be informed about a new bright solution concerning the ρ limitation.

The logic behind the new solution is bullet-proof – if the criterion does not work well for high ρ, and results in excessively conservative estimates, its values should be limited by Eq. (7). Whenever violation occurs, the incorrigible coefficient should be seized and brought back to $\rho_{\lim}$. Note that the general over-conservatism of the criterion can be decreased by this operation.

The complete criterion should thus be rewritten with the help of Eq. (7) to the following closed form (so that other readers will not repeat my ignorance):

$$\begin{aligned} &a_{Su} \cdot C_{aS} + b_{Su} \cdot \frac{N_{aS} + d_{Su} \cdot N_{mS}}{C_{aS}} \leqslant f_{-1} \quad \text{for } \frac{N_{aS} + d_{Su} \cdot N_{mS}}{C_{aS}} \leqslant \frac{1}{2-\kappa}, \\ &C_{as} \leqslant \frac{t_{-1}}{2} \quad \text{for } \frac{N_{aS} + d_{Su} \cdot N_{mS}}{C_{aS}} > \frac{1}{2-\kappa}, \end{aligned} \tag{9}$$

where

$$a_{Su} = \kappa; \quad b_{Su} = f_{-1} \cdot (1 - 0.5 \cdot \kappa)$$

The subscript S by the stresses on the plane examined relates to the fact that the evaluated plane should be the plane with MSSR. Accepting the limitations imposed, we have to look above all at the second part in Eq. (9) derived by simple mathematical operations for cases when ρ is too high and $\rho_{\lim}$ replaces ρ. The strange meaning of this should strike us immediately.

Prof. Susmel devoted a long time to the subsequent evolution of his criterion. I would like to ask him to clarify if the second part of the criterion in Eq. (9) **sounds sensible to him, and what it means.**

Whatever it may mean, it is clear that the mean stress effect on the multiaxial fatigue limit is completely gone and the only decisive load variable is the shear stress amplitude. Another citation from Prof. Susmel's work is required here:

"*...when ρ is larger than a certain limit value, ρ_{lim}, the reference shear stress to be used to estimate multiaxial fatigue damage is assumed to be constant and equal to $\tau_{Ref}(\rho_{lim})$. This simplification was introduced because, for high values of ρ, the estimates obtained by applying the MWCM were seen to become too conservative.[21] It is the author's opinion that, according to Kaufman and Topper,[19] such a high degree of conservatism has to be ascribed to the fact that, when micro/meso cracks are fully open, an increase of the normal mean stress does not result in a further increase of fatigue damage.*" [3], p. 298

I assume I understand this part of Prof. Susmel's work correctly – for specific load cases the applied normal stress effect is so high that the "*micro/meso cracks are fully open*". Even if the normal stress becomes higher, it does not worsen the situation. My question then follows: If this new definition should better solve the load cases with effective normal stress much higher than this limit value, how it is possible that there is no normal stress at all? Is there some gap, or a sudden jump? At one moment, the mean normal stress affects the damage parameter, but when increased slightly, does the normal stress no longer play any role? What a strange quantum mechanics this is... **I challenge Prof. Susmel to support this theory by some experimental observation.**

As far as I know, this is the latest evolution of the Susmel method as regards its functionality for the fatigue limit evaluation, and it was codified in his book [5] in this way. Originally, I also wanted to provide computational results of its application over the data evaluated in [2]. I programmed it in several versions to PragTic in order to achieve the best possible results. Indeed, for the methodology described there, the results are now better with the standard deviation ΔFI being 8.4, range 76.6 and mean value 1.7%. **However, under current circumstances, I would prefer Prof. Susmel give attention to his method and improve it, because these results are not sufficient if compared to other criteria evaluated in** [2]**, and the logical gaps are too evident.**

4. Critical plane methods

"*Since, contrary to what proven by my calculations, Dr. Papuga affirms that all the critical plane approaches making use of the plain of maximum shear stress amplitude do not work well in the presence of non-zero out-of-phase angles, I have to come to the conclusion that Dr. Papuga, or, more probably, his software (which would be even more embarrassing since such a software can be downloaded from the web and used to design real components), simply miscalculated the stress quantities relative to the critical plane systematically.*" [1], p. 2 of 4

Prof. Susmel also discusses my ability to find the critical plane for the MSSR methods:

"*For several complex cases, he correctly shows that there exist a number of planes experiencing all the maximum shear stress amplitude, even if Dr. Papuga himself claims that he is not capable* *of deciding what is the critical one. The only conclusion which I draw from this is that he has not realised yet that the MWCM is a biparametrical critical plane approach, so that, in order to correctly apply it, a multi-parameter optimisation process has to be run to select the appropriate critical plane.*" [1], p. 2 of 4

I do not know what the 'biparametrical critical plane approach' means. Because of my experience as a programmer, I find the implementation of the MSSR method problematic. First I have to search for all planes with MSSR, then select the plane with the maximum damage parameter from them and try to manage the numerical truncation errors, which means that the application of the MSSR method is also lengthy. If the MSSR methods can prove that the additional calculation time improves their prediction capability, I would accept them. However, anyone who looks at Tables 6 and 8 in [2] can note that the scatter of data increases for the Matake, Susmel or McDiarmid MSSR methods, whenever out-of-phase loading is introduced, even when there are no mean stresses.

I hope I guessed the topic of Prof. Susmel's reproach, because Prof. Susmel did not write anything about the meaning of this biparametrical approach. However, this solution is commonly used in PragTic with MSSR methods. I assume these sentences of his are in response to my question in Sec. "Mean stress effect": Which value of the d_{Su} parameter has to be selected if there is more than one plane with the same MSSR? If the same logic is applied, the maximum d_{Su} value should be used, because it provides a higher damage parameter when other experimental data is checked than the lower d_{Su} parameters. The problem I find then is that the consequent use of this approach would usually lead to d_{Su} values greater than one (sometimes in the order of thousands, millions and more), simply in dependence on how small the mean normal stress is on the MSSR plane used for d_{Su} derivation. Prof. Susmel's premise $d_{Su} \leqslant 1$ can be applied, but I ask myself why it should be so?

In any case, if this unsupported limitation is accepted, the MWCM method gets back to its original definition of ρ (Eq. (8)) for the experimental data sets that I checked personally (data sets Lem, HRZ, FLB, SiB, Mi, Iss, Ggh in [15], which are also evaluated by Susmel in [3]), because a data item resulting in $d_{Su} > 1$ can be found in all of them. In this way, the improvement provided by Susmel in [3] in adding the d_{Su} parameter into the definition of ρ (Eq. (1)) is wholly annulled. According to the d_{Su} values that Prof. Susmel uses in [3], it seems he has another method for deriving d_{Su}, but I still do not understand what this method is.

When I reflected on Prof. Susmel's accusations, I noticed that I have not been fair enough and I therefore provide Table 8 from [2] in a modified form in Table 5 with information on the range of ΔFI results for experiments on ductile materials only ($1.25 < \kappa < 1.73$). It is apparent that the functionality of the MSSR methods is better, which could be expected. So, really the items in my conclusion should be modified e.g. as follows:

2. All criteria of the critical plane approach based on the maximum shear stress range (CPA–MSSR type) show negative properties under out-of-phase loading, when applied to brittle materials.
3. The criteria by Crossland, Dang Van and Matake face problems when they are used for out-of-phase load cases (Table 8 in [2]). These methods are not suitable for evaluating multiaxial load cases. The Sines criterion should be ranked in the same category, but its failure is even more general.

Though I have had to reformulate this critical part of the conclusion, the superiority of MD approaches (FIN, PCR, QCP, ROB) is obvious from Table 5, because, if the mean stress effect is not involved, they do not suffer from the same limited applicability as MSSR methods.

5. PragTic application

The citation from the start of the previous section is worth some further analysis. First, Prof. Susmel attacks the validity of my calculations. It is true that every programmer's nightmare is the discovery of some undetected bug. I do not think there is any bug in PragTic regarding this aspect. However, though the test set is so huge (407 experiments) and the number of tested methods is also huge, I have managed to bring to life a system which allows anybody to check my calculations (unlike the calculations of Prof. Susmel, provided e.g. in Fig. 1 of [1] and the speculative results of his computations in [3], about which I have already written above).

Anybody can access the FatLim database [15], anybody can download PragTic, anybody can download the input file [24] for PragTic and run it for any experimental input that I refer to. Anybody can inform other users about bugs he finds in PragTic. I do not know of a better description provided by any researcher doing the same job for a similar number of data items.

I myself have tried to contact some researchers to ask them to check the experimental data or even the calculation results of some methods. It seems, however, that researchers are not interested in similar evaluations, because there has been no helpful answer yet. I myself even wrote the following e-mail to Prof. Susmel:

"*. . .I wanted to discuss with you your HCF method. I have implemented it into PragTic and tested it, but it gives for some cases not a very good prediction. I would blame either sig_n,max/tau_a ratio or my implementation, but would bet on the first:-)...*" [25]

There was no answer, so I checked his latest papers (too hastily), found the new ρ parameter formulation including the improved mean stress effect (Eq. (1)) and published the results.

I regret that one of the reviewers asked me to remove the following part from the conclusion of [2]:

"*11. The individual experimental data, material properties, as well as prediction results reported here are freely available in the FatLim database (http://www.pragtic.com/experiments.php). The database is also accessible for adding new data, and any help in expanding or checking the database will be appreciated.*
12. The PragTic fatigue solver allows quick implementations of new variants of methods and very efficient solutions for the whole FatLim content. Interested researchers can contact the author to gain access to its source code."

Table 5
ΔFI ranges for the most basic experimental groups. SUS refers to the Susmel method as described in [2], i.e. without the latest changes.

Range of ΔFI in individual groups (tests)	CRO	DV	FIN	FOG	GAM	LM	LZ	MAT	MD-MD	MD-MSSR	PAP	PCR	PIR	QCP	ROB	SIN	SPM	SUS
NMS (171)	48.3	52.7	33.5	70	31.8	36.1	30.7	77.2	57.3	56.4	37.9	22.7	21.9	28.3	33.5	61.6	29.3	57.6
NMS,OP (40)	48.3	52.7	33.5	31.5	31.8	36.1	30.5	77.2	52	52	36.1	22.7	21.9	28.3	33.5	49.6	29.3	57.6
NMS,IP (131)	21.3	22.3	22.3	70	20.5	20.8	24	22.2	47.7	48.4	21.3	16.5	20.6	20.9	22.3	51.9	22.4	20.3
NMS,ductile (118)	41.4	42.8	30.8	26.9	29.2	31	26.2	41	44.5	46.6	29.4	22.1	20.9	28.3	30.8	43.4	29.3	33.3

Perhaps Prof. Susmel would read at least the conclusion to the end, find this information and reflect whether it is not better to cooperate on solving the puzzle that we all are facing. In his words, it seems that I am offending whole fatigue community by stating my conclusions. I have to be careful not to attend fatigue conferences any more, in order not to see the brightest scientists lining up to crucify me.

In the introductory section to my paper [2] I wrote quite brusquely, because I wanted to highlight that the mainline research in this field is no longer functional. All those researchers **whose criteria** I criticize have done a good job, but they have worked on limited data with limited means (i.e. hardware and software, above all). We can work today in a more sophisticated and complex manner, but we are not using this opportunity. As a result, nobody knows what to implement in commercial fatigue solvers, because there are so many criteria each claiming to be better than the other.

Prof. Susmel proposes to conserve the MSSR theory, Sines or Crossland criteria, because all the authors he refers to in [1] worked hard on them, are or were respected people and thus should not be wrong. I must say that this philosophy is quite strange to me, but am very happy that not everybody thinks in the same manner, otherwise we would not have got over the phlogiston affair, or indeed we would never have come down from the trees, where some ancient early researchers presumably lived contentedly.

Secondly, Prof. Susmel attacks me for providing access to PragTic freeware. As far as I know, the only methods implemented today in dominant commercial fatigue solvers are those by Dang Van, McDiarmid and Findley. Anybody who reads my paper to the end can understand my frustration resulting from such an attack. **I challenge Prof. Susmel to find some cases which PragTic has not computed as presented, or as they should be calculated, and provide such problematic parts to me and to the readers.** At this moment I believe my PCr method gives the best results for this specific problem, and I see no reason for hiding PragTic away from other people, or why I should not enable it to be used.

6. Proof of Prof. Susmel's results

Finally, we come to the only scientific-like information provided by Prof. Susmel in his commentary, which is Fig. 1 in [1]. Because Prof. Susmel systematically attacks my computations, I challenge him to provide complete information on the individual test sets that he used (with correct references this time, please), the way he retrieved the d_{Su} parameter, and why he chose it. I have challenged some of his published results, and I am very interested in his response to my detailed comments here.

7. Conclusion

I have tried hard but unsuccessfully to find the answers to highlighted questions of mine in the works of Prof. Susmel and in the finalization of items 1–4 [1]. If I did my search wrongly, I am sure Prof. Susmel can refer to those parts of his papers where the reader can find responses to my questions. Prof. Susmel has claimed in [1] that my testing of his MWCM criterion in the version provided in [2] is unfair. However, how could I have done the testing rightly, when he does not explain how his criterion is to be used, and when so many questions remain open?

If the MWCM method is to be used correctly, the reader needs to know the response to items 2 and 4 of the list below, and since I am accused of miscalculating something somewhere, an answer to item 6 would also be helpful. From unknown reason unfortunately, Prof. Susmel decided not to provide any answer to my questions, though he read them and reformulated his document for the final publication into the version presented in [1]. The items of my final conclusion thus follow:

1. Items 2 and 3 of my conclusion should be reformulated:
   - All criteria of the critical plane approach based on the maximum shear stress range (CPA–MSSR type) show negative properties under out-of-phase loading, when applied to brittle materials.
   - The criteria by Crossland, Dang Van and Matake face problems when they are used for out-of-phase load cases (Table 5). These methods are not suitable for evaluating multiaxial load cases. The Sines criterion should be ranked in the same category, but its failure is even more general.
2. The way Prof. Susmel derives the mean stress sensitivity coefficient d_{Su} is not rigorous at all. He should provide detailed information on its computation for the tests evaluated in [3], and should define the reasons, which made him to use the specific tests he used for determining it. What happens when there are several MSSR planes leading to different values of d_{Su}? Why cannot d_{Su} be higher than 1? The FLB03 test from [15] results in two values of d_{Su}, but both of them are negative. What does this mean, and how are we to proceed in such a case?
3. The systematic use of ρ_{lim} limiting the calculated ρ ratio was first announced in 2008 [3], and Prof. Susmel himself used the same method as me in [2] in 2005 [23]. Because I have not written anything about the newer version in this aspect, it was apparent that I have been using the older form of the criteria. I therefore see no reason for Prof. Susmel's attack. If he wants a fair judgment concerning the newer version, I want to ask him first to reflect on the problems I have highlighted in this text. I do not want to spend more time than one week already spent on programming something that has such strange logic.
4. There are serious conceptual problems in the definition of Susmel's latest criterion (published in [3,5]). Readers are warned not to use it in the current form and if they do, then with the greatest possible caution. I ask Prof. Susmel to explain the functionality of the criterion in the highlighted cases.
5. Though Prof. Susmel has tried to place emphasis on revealing the threat that I pose to the fatigue community, I persist in offering cooperation in future implementations of new fatigue criteria to PragTic. I believe that this is the future of research in this field, and the only way to move forward.
6. I ask Prof. Susmel to support with some results his assertion that PragTic works wrongly for his method and some specific data items described in [2] or in Tables 1–4 here. I understand now that his method was more developed than I had realized when preparing [2], but the errors in the PragTic analyses should be evident even in the form used in [2] or in his own paper [23].
7. I find Prof. Susmel's comments and his conclusion quite damaging, and challenge him either to support his arguments by some sound scientific facts or to offer an apology.

8. Final comments to the external referee, and a challenge to researchers

Firstly, it is obvious that the level of detail concerning specific items in my paper [2] is not sufficient. Particular phenomena, e.g. those highlighted by the referee, need further work, but this was beyond the scope of my small team of one researcher. The paper was intended as a starting point for further analyses. It was thus conceived as a big picture, details of which should be evaluated in years to come.

At first, I also doubted whether such an overview has some sense, due to the numerous effects affecting the external sources of experiments. However, the difference between my PCr method and the methods of other researchers is so pronounced that these secondary uncertainties seem not to be the crucial point at the present time.

Secondly, I really do not understand Prof. Susmel's accusations (though the referee seems basically to accept them). Thanks to FatLim and the link to it provided in [2], Prof Susmel had a complete description of the inputs and outputs for every single experimental point that I use in the analyses summed up in [2]. However, even after being invited to react to my comments here, he has not provided a single scientific argument to show that I made some wrong calculations. He has only pointed out that I have omitted some particular peculiarities of his method. Nevertheless, he persists in his accusations.

By contrast, I have tried to explain each of my objections to his method, and support my arguments with exact data. I have used the method of scientific disputation (the referee's use of the expression ***"accusation"*** fails to recognize the detailed evidence that I have provided). Scientific dispute is surely the right and traditional way for researchers to settle the differences in their findings.

It is true, and the referee notes it as well, that careful selection of experiments can lead to obtaining nice prediction results. The FatLim database is open, so anybody can add new experimental results. Unfortunately, this option has not been used by any website visitors. I myself have discarded only those experimental results which I explicitly described in [2].

I would like to suggest a challenge to provide a framework for finding scientific answers to the matter of scientific disagreement between myself and Prof. Susmel:

(1) Let any researcher who has published at least one paper on multiaxial fatigue either in International Journal of Fatigue or in Fatigue & Fracture of Engineering Materials and Structures participate, and have one vote.
(2) Let the voters elect five people among this community to form a committee, which will have a right to accept or discard any potential experimental input.
(3) Anybody can propose further experimental data that should be used for validation of fatigue limit evaluation methods. The FatLim database can be used for presenting the data, or some independent database system can be built up.
(4) It is quite a widely accepted assumption that no criteria can correctly describe all conditions. Let the committee announce if there are any categories involving e.g. specific load combinations, material groups, etc.

Whoever describes a method that provides better results than those existing before and proves its functionality in the specified category, will receive an small prize, something like a yellow jersey (à la Tour de France), whatever is proposed (are there any sponsors?). Originally, I intended PragTic to be used for this goal and I offered to provide its source code to interested people, but any other tool can be used – the committee will decide.

(I would like the referee to explain why he ***"was a little disgusted by (hided) promotion of software PragTic."*** PragTic is opensource freeware, and I see nothing unethical or disgusting about bringing it to the attention of researchers in the field, in an attempt to provide an accessible platform for collaboration. Can the referee suggest a better method for speeding up development?)

The question arises, Which institution is able to manage a challenge of the type that I suggest here? My own attempts to provoke other researchers to collaborate (PragTic in Use contest – http://www.pragtic.com/PiU.php) has failed, perhaps because it has not been possible to advertise it adequately.

If no challenge of this type is organized or accepted, I would like to invite other researchers to let me know if they find any errors in the results I provide in FatLim. Anybody can make mistakes, but a better future depends on accepting that mistakes may have been made, and then fixing them.

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