Closure to "Discussion of 'Yield Function for Solder Elastoviscoplastic Modeling'" (2005, ASME J. Electron. Packag., 127, pp. 147–156)

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We respond to the technical content of the discussion as follows:

1. Discussant appears to have erroneously assumed that differences in material response under tensile (here J_1 −ve) and compressive (J_1 +ve) regimes necessitate different parameters and that use of a single set of parameters implies identical responses under these regimes. The yield function we presented is not entirely symmetric about the J_{2D} axis. A single set of parameters can handle different material responses, as also different ultimate stresses, for tensile and for compressive stress paths. We are unsure why discussant wants yield surface discontinuity between compressive and tensile regimes. Von Mises surfaces span both tensile and compressive regions without breaks at $J_1=0$. When loading solder in shear along the J_{2D} axis with J_1 nominally zero, we do not expect vanishingly small perturbations in the sign of J_1 to lead to finite discontinuities in observed behavior. On the contrary, we would expect solder yield behavior to be relatively independent of small hydrostatic stresses. Discussant and coworkers have previously adopted small values for the HISS parameter γ to minimize the impact of J_1 on yielding.
   HISS may have potential issues with solder simulation regardless of the value of γ assumed. The original unloaded state of $J_1=0$, $J_{2D}=0$ does not correspond to a value of zero for the plastic strain trajectory. The yield surface that corresponds to zero plastic strains is a point located at $J_1=-3R$, $J_{2D}=0$, beyond which stress the material is assumed to fail. Furthermore material in the stress free state is on a hardened yield surface that starts at $J_1=-3R$, $J_{2D}=0$ and ends at the stress free state of $J_1=0$, $J_{2D}=0$. The value of the plastic strain trajectory for this surface is calculable iteratively as shown in our paper, and is temperature-dependent. Temperature changes at zero stress then lead to changes in initial plastic strain trajectory, implying spurious plastic straining or spurious hardening under unconstrained heating and cooling. The shape of the surface also implies that vanishingly small compressive stresses cause yielding whereas a finite tensile stress is required to initiate yield. The new function avoids these issues because the yield surface expands from the origin of the stress axes where the stresses are zero, and so are the corresponding plastic strains regardless of the temperature. Thus, it would appear to remove the defects noted for HISS.
   Discussant's premises with regard to (1) do not appear to be correct. As we also explain in our response to (1) above, the HISS function has numerous issues that the new function rectifies. The responses suggested by the discussant are predicated on the assumption that a single set of parameters leads to identical responses under tension and compression loadings for the new yield function. Since this is not true, we do not delve into the matter any further here.
2. The HISS equation provides continuous yielding on the J_1 positive side of the J_{2D} axis, not for J_1 negative. If J_1 is assumed negative for tension as per geotechnical conventions, we get continuous yield behavior for compression. Reversing the sign convention provides continuous yielding for tension, not for compression. If the latter is used to model solder, the maximum shear the material can carry increases with increasing tensile hydrostatic stresses and decreases with compressive hydrostatic stresses. The material would fail if a compressive hydrostatic stress of 3R is applied, while it would withstand an infinite tensile hydrostatic stress and could even carry infinite shear at this state. It may be better to use the compression positive convention, for which we noted in our paper that while tensile yielding will not initiate at zero stress, the region of solder elastic response is small.
   Compression predominant geotechnical materials can be modeled using yield functions defined for the compressive space. Metals are not tensile predominant. A yield function restricted to tensile regimes is insufficient for solder. Under reversed uniaxial cycling, solder is subjected to tension at one time compression at another. In more complex analyses it may not be possible to know beforehand when the hydrostatic stress at a point is tensile, compressive, or zero. If the HISS function can be defined either over the tensile space in which case it is invalid for compression, or over the compressive space in which case it becomes invalid for tension, which version applies here? Or do we switch functions?
   If switching the yield function from tensile to compressive stresses only involved changing parameter values, we could use some iterative procedure to use appropriate values depending on the results of a given calculation step. However, only one sign convention may be used for stresses within a simulation. We cannot say that when material at some point is under tensile hydrostatic stresses we use the tension positive convention, and anytime it goes under compression the convention at that point becomes compression positive. At least two different yield function equations must then be used. One the HISS equation, F, valid for tension and another, some yet unpublished function, F*. The HISS equation by itself would be insufficient. The plasticity formulation may need to account for transitions between F and F* during the course of loading. For metals we would also expect the surfaces for these two functions to remain continuous across $J_1=0$, implying constraints on parameters.[1] We have not noticed simulations employing different sets of parameters for different stress spaces, yield functions complementary to the HISS equation, or provisions for switching the yield function equation itself if stresses changed from tensile to compressive. The stress-transfer approach for geotechnical materials under tensile loads is not applied to solder under compression. Our statement regarding tensile yield characteristics was based on the most reasonable application of HISS to solder, under the normal sign convention where shear capacity does not increase indefinitely with tensile stresses. Restricting the stress space retroactively may make HISS potentially inapplicable for solder, requiring re-evaluation of prior validations.
   We referenced discussant's prior work for "The HISS yield function represents a unification of the above trends, including the various prior functions as special cases [1]." Discussant also states HISS will not be continuous yield, or even

[1] In reference [19] of our paper we suggested a modified HISS equation, replacing J_1 in HISS by $|J_1|$. This mirrors the positive J_1 side to the negative side, implying continuous yielding on both sides. However the problem of increasing shear capacity with increasing tensile hydrostatic stress and the problem of spurious initial plastic strain trajectory increments under unconstrained heating or cooling remain. Furthermore continuity of yield surfaces would require the same parameters under tension and compression and in this case, unlike for the new yield function, tensile and compressive behavior must then be identical.

valid, either for tensile or for compressive loads, depending on the stress space on which it is defined.

We never raised the issue of "unconstrained thermal loading in the tensile zone." By definition, unconstrained means the material is under zero load, neither tensile nor compressive. Even if HISS were to be defined only for the tensile region for solder, as long as the state of zero stress is permitted, the initial plastic strain trajectory would still depend on the temperature and the problem would appear to remain.

With regard to the responses suggested by the discussant, as we explain above in our response to (2), for metals we cannot use a function that is defined only for tensile loads, or only for compressive loads, but not for both. HISS being continuous yield only for the stress-space it is defined for, i.e., for tensile loading only for metallic materials, would appear to imply HISS is not applicable for metals. Issues of continuous versus discontinues yield with regard to solder could then be moot.

3. We would need to apply a continuous yield function to solder only if solder were to demonstrate continuous yield behavior, which discussant has defined elsewhere as significant plastic deformation below the conventional yield stress. Therefore, we need to cite experimental data that suggests such behavior, to justify any increased complexity in simulation. For data where the 0.2% conventional yield stress is at about 5000 psi, Rafanelli (1992) noted the behavior to be "predominantly plastic … some elastic behavior is present at approximately 1000 to 3000 psi" and noted that elastic modulus and yield stress were insufficient to describe the behavior. This is why we referenced Rafanelli, for demonstrating solder behavior that would constitute continuous yielding. The model Rafanelli used to simulate the experimental data is irrelevant to our citation. Discussant mentions two works by discussant and coworkers as the ones we should have cited instead of Rafanelli, for this purpose. These works are subsequent to Rafanelli. They did not generate any experimental data that showed solder needed to be modeled as continuous yield. The six curves provided by Reimer they primarily used to determine parameters are not experimental data, but are curves generated from an equation similar to what Rafanelli used. As per the discussant these would be curves from a nonlinear elastic equation. Even if their backpredictions matched the curves exactly, the best this could have shown is that HISS can accurately replicate nonlinear elastic behavior. There are also issues with the formulation and with parameter determination, some of which we discussed in our paper. These curves are for uniaxial tension for which discussant states a tension predominant HISS, not continuous and not valid under compression, is used. We feel this makes HISS inapplicable to solder. Indeed discussant and coworkers have been applying HISS to solder for several years and in our paper we referenced the works the discussant mentions, and also many other works by the discussant, extensively.

   With regard to the responses suggested by the discussant, as we explain above in our response to (3), the model Rafanelli used, or whether or not Rafanelli used any model at all, is not the issue here because we cite the experimental observations for solder that suggest continuous yield behavior and not the model used to backpredict these. The discussant appears to have misunderstood why we needed to reference Rafanelli and therefore we do not delve into the matter any further here.

4. Higher level validations can be applied meaningfully only after reasonable confidence is attained for lower levels. We would add a level 0, ensuring basic formulations and models are reasonably consistent. In attempting to apply HISS/DSC to solder we realized the elastic equations used were inconsistent with $\sigma_{\mathrm{ij}} = \mathrm{C^e}_{\mathrm{ijkl}}\varepsilon^{\mathrm{e}}_{\mathrm{kl}}$. Wang had changed the plasticity formulation employed by the discussant but even the new scheme violated $F \equiv 0$ for rate-dependent parameters. These discrepancies are significant enough that for typical loading conditions, the entire elastoplastic formulation could be inapplicable to solder unless strain-rate were held constant. For the viscoplastic formulation, the procedure for parameter determination used $F = 0$ for steady-state creep while the underlying formulation $\overset{\bullet}{\varepsilon}{}^{vp}_{ij} = \Gamma \langle F \rangle^N \partial F / \partial \sigma_{ij}$ requires $\overset{\bullet}{\varepsilon}{}^{vp}_{ij} = 0$ when $F = 0$. There are issues with the HISS yield function, described earlier. Additionally the procedure for determination of hardening parameters was internally inconsistent. Level 1 validations also seemed problematic. The discussant refers to Wang wherein reported values of 3R and γ grossly violate the basic relationship $3\mathrm{R} = \tau_{\mathrm{ult}}/\sqrt{\gamma}$ that is used to compute one parameter given the other. Hardening parameters are computed based on a simplified equation assuming atmospheric pressure to be 1 MPa. It is 0.1 MPa. At higher levels, the finite element simulation provided for backprediction of the test data is a single element under plane stress, although the joint height is extremely small compared to its length and width. It shows reasonable results do not mean that parameters are correct or the loading is realistic. The level 3 TSOP simulation which discussant says validates HISS/DSC uses parameters that are not the parameters computed in the work. What is the point?

   Thus, our first focus was levels 0 and 1. We corrected basic schemes such as the elastic formulation and developed formulations ameliorating the prior schemes to the extent feasible, such as the iterative parameter determination scheme for HISS. Where this was not possible, we developed other formulations such as the new yield function, and the corrected procedure for viscoplastic parameter determination. The next task was to carefully compare and contrast the new schemes with the prior schemes at these basic levels, for which we used Wang's data as referenced in the paper. Our current validations are sufficient for these purposes and at this level itself we appear to have obtained significant results vis-à-vis prior level 3 analyses. Whereas prior level 3 simulations have crossed into both regimes, HISS now appears to have been restricted to either tensile space or the compressive space at one time, potentially making it inapplicable to solder. Prior level 3 analyses validated the viscoplastic formulation using Γ values of the order of 1 to 10, when our analyses showed the value should be in the range 10^{-11} to -10^{-7}, depending on the temperature.[2] We would note that of late discussant has adopted 0.24×10^{-10} for level 3 analyses elsewhere indicating our range is reasonable. HISS/DSC has been level 3 validated for the elastoplastic scheme. It is unclear how complex numerical approximations to solve equations violating $\sigma_{\mathrm{ij}} = \mathrm{C^e}_{\mathrm{ijkl}}\varepsilon^{\mathrm{e}}_{\mathrm{kl}}$ and $\mathrm{F} \equiv 0$ is useful, especially with inconsistent plasticity parameters. Without corrections Wang's parameters would not even allow for use of the alternative, more appropriate, viscoplastic scheme. A new model by definition is something that is recent and not validated over more than fifteen years as is the case for solder with HISS. Our results indicate significant care is needed in subsequent levels, which may also be a joint test of the constitutive formulations, the model for degradation and the numerical schemes employed to handle potentially negative definite matrices at the structural level. Potential issues with these latter formulations are not our focus here, but ones that exist need to be resolved before such formulations can be applied.

   With regard to the responses suggested by the discussant, as we explain above in our response to (4), the analyses we presented are sufficient for our purposes in the paper under discussion, and are significant because such issues appear to

[2]The exponent for Γ, in the header of Table 2 of our paper, should read -9 instead of -6.

(a) Parameter determination (Chia [50], Desai et al. [48])

I. Elastoplastic parameters from data of Reimer				
Reimer's curves are not experimental data, but are from a nonlinear elastic equation for 63/37 solder. Strain range of 0.01 may also not provide peak stress				
Parameter	**Value**	**Rate ($\dot{\varepsilon}$)**	**Computed**	**Notes**
E	$E(\theta) = 23.45(\theta/300)^{-0.292}$ GPa	-	No	Based on various sources for 60/40 solder
ν	Fit to $\nu(\theta) = 0.4(\theta/300)^{0.14}$	-	No	From 63/37 values of Pan [58], where corresponding E values are significantly higher
β	0	-	No	Assumed so that octahedral space shape is like vonMises
n	2.1	-	No	Assumed to approximate hydrostatic incompressibility
3R and γ	Fit to $R(\theta) = 217.473(\theta/300)^{-2.95}$ MPa Fit to $\gamma(\theta) = 0.00082(\theta/300)^{-0.072}$ Fit to $R(\theta) = 240.67(\theta/300)^{-1.91}$ MPa Fit to $\gamma(\theta) = 0.00082(\theta/300)^{-0.034}$	$0.0002/s$ $0.002/s$	Yes,?	Determination of 3R and γ is interrelated. Researchers assume shear strength under zero hydrostatic stress to be 95% of the shear strength under uniaxial loading. This fixes γ to a constant value of 0.00083. It is unclear how a temperature-dependent γ was obtained.
a1 and η1	a1: 0.298×10^{-4} (208 K) -0.11×10^{-4} (373 K) η1: 0.394 (average) a1: 8.3×10^{-6} (208 K) $-$ 0.195×10^{-4} (373K) η1: 0.615 (average)	$0.0002/s$ $0.002/s$	Yes,?	When computing a1 and η1 from data, the plastic strain trajectory, ξ, has to be assumed to be zero at zero stress. This implies for HISS $\alpha = a1/\xi^{\eta 1} \to \infty$ at zero stress. However at zero stress we also have for HISS $\alpha = \gamma(3R/p_a)^{2-n}$. Substituting parameter values, say at 300 K, 0.0002/s gives $\alpha = 3.4 \times 10^{-4}$. This is an inconsistency in the parameter determination procedure.
II. Elastoplastic parameters from data of Skipor				
Chia takes the data as being for 60/40 solder. The title of Skipor's cited work is "Constitutive Characterization of 63/37 solder…" Elastic modulus values reported are different from those adopted above. It is unclear how these were obtained since procedures are not reported. Issues noted for γ, a1 and η1, persist.				
III. Viscoplastic parameters from creep data of Pan [58]				
ln(Γ) [1,50]/Γ[48] Γ[48, 50]/ln(Γ)[1] N	0.5784 (293K), 2.058 (313), 3.475 (333), 4.61 (373), 6.96 (393) $1.8(\theta/300)^{6.185}$ 2.67 (average)			Γ versus ln(Γ) confusion; static yield surface parameters not reported; Γ and N are computed assuming $F = 0$ at steady state creep, but formulation $\dot{\varepsilon}_{ij}^{vp} = \Gamma\langle F\rangle^N \partial F/\partial\sigma_{ij}$ requires $\dot{\varepsilon}_{ij}^{vp} = 0$ when $F = 0$.

(b): Constitutive validations

Chia [50] and Desai et al. [48]

Level 1	1. There appear to be no validations with respect to experimental data for viscoplastic parameters. 2. Elastoplastic backpredictions for parameter determination curves appear reasonable.
Level 2	1. There appear to be no validations with respect to experimental data for viscoplastic parameters. 2. For elastoplastic parameters, backpredict Skipor's curves at strain rate 0.0001/s using parameters from Reimer's curves at rate 0.0002/s. One set of elastoplastic parameters, such as the values of the elastic and plasticity parameters from Reimer's curve at 298K and at strain rate 0.0002, will backpredict only one stress-strain curve for uniaxial loading. For comparison with Reimer's data itself, the backprediction shows a stress of around 30MPa is attained at 0.2% strain (Fig. 7.1(c) of [50]). For the backpredicted curve for comparison with Skipor's data at 293K, the stress does not reach 30 MPa even at strains higher than 8%, while at 0.2% strain the stress appears to be around 12 MPa (Fig. 7.4(d), *ibid*).
Level 3	There appear to be no level 3 finite element validations

The Perzyna viscoplastic formulation simulates delayed plasticity under high strain rates. Material loaded statically in the limit of zero strain rate is assumed to respond as elastoplastic. This static stress-strain curve follows the elastoplastic formulation with $F = 0$. Plasticity parameters are computed for the static yield surface. Under faster loading the irreversible strain is delayed, so the yield surface does not harden sufficiently for the state of stress to satisfy $F = 0$. Instead we get $F > 0$, and viscoplastic strain is induced at the rate $\dot{\varepsilon}_{ij}^{vp} = \Gamma\langle F\rangle^N \partial F/\partial\sigma_{ij}$. $F = 0$ implies $\dot{\varepsilon}_{ij}^{vp} = 0$, and viscoplastic strains arise only when stresses exceed the static yield surface, i.e., $F > 0$. So (1) plasticity parameters are only computed for the static surface; (2) fluidity parameters are not determined by assuming $F = 0$ at the steady state of a constant creep rate; (3) if a stress-increment is applied over several time increments, F will not be zero at the end of each time increment unless the viscoplastic strain rate is zero at that instant of time. The convergence criterion would be F tending to a constant value as finer sub-increments are used, with the converged value of F corresponding to the viscoplastic strain rate at that instant.

Basaran et al., JEP 120, March 1998, pp. 48–53 (viscoplastic validation).

Level 1/3	Finite Element viscoplastic backprediction of Reimer's curve at 300K, strain rate 0.0002/s, using viscoplastic parameters of [48,50].The strain-range for the data is 1%, the backprediction is up to 0.3% (Fig. 2). Starting approximately 0.25% strain the predicted stress appears to decrease, whereas for the data the stress rises monotonically up to 1% strain. The yield function parameters used for the simulation are those computed by [48,50] for the elastoplastic simulation, i.e. assuming that the curve being backpredicted is the static yield surface itself. A stress drop could indicate material going into the elastic zone. Furthermore static surface simulations cannot test the validity of Γ and N. If Γ is made larger and larger, results for a given strain rate would be closer and closer to the static surface. As $\Gamma \to \infty$, the material will be perfectly elastoplastic and, regardless of the strain rate, the backpredicted curve will always be the assumed static yield surface. However as Γ becomes larger, numerical issues may also arise. For instance if a viscoplastic strain increment becomes too large, the stress may appear to drop. The way to test whether these parameters and the procedure $F = 0$ provide level 1 backpredictions, taking the curve at 0.0002/s to be the static yield surface, is to keep all parameters the same and run the simulation for the data at 0.002/s. Results are not available, but it would appear that using $F = 0$ for convergence will try to force the solution to the static yield surface, which is the only surface on which F would be 0.

For space sake we omit simulations where, the formulation was later modified; a subset of parameters were modified, as that would require re-computation of subsequent parameters and data for 60/40 and 63/37 is mixed anyway in parameter determination; results were not compared to test data; or DSC was included, the last as potential issues with DSC and its numerical simulation may commingle with constitutive modeling. We would however note that for later level 3 viscoplastic simulations, Nickerson (2002) has adopted a value of 0.24×10^{-10}, several orders of magnitude smaller than the values reported by [48,50], for Γ.

have been missed in prior work using complex finite element analyses. The results of the paper, in terms of the rationale for the work, are discussed in the Conclusions section of the paper.

5. In Section 2.1 we discuss the strain-rate terms omitted in prior DSC analyses, with reference to [19], and that "the stress would fall at the initiation of hold, and would rise at the end of the hold period, for a trapezoidal displacement cycle." For an elastic analysis if $\tau = G\gamma$ and G increases with strain rate like for Wang's parameters, a decrease in strain rate at the beginning of hold in a trapezoidal cycle would decrease the stress. Similar logic, for end of hold. Why develop an overly complex elastoplastic formulation, including the additional rate-dependence term for plasticity in Eq. (3), to study trapezoidal cycles in more detail when the modulus change is an artifact of the elastoplastic approximation which cannot simulate creep directly?
   With regard to the responses suggested by the discussant, as we note above, a simple elastic analysis is sufficient to demonstrate the issue. Adding rate-dependent plasticity will not resolve the problem with the elastic stress–strain relationship. Instead, such an endeavor would only serve to complicate the formulation making it more difficult to discern the problem with the elastic stress-strain formulation. As we pointed out the prior formulation with rate dependent plasticity parameters itself violated $F \equiv 0$, and would only have served to contaminate results further. This example would appear to confirm our view, that it is important to avoid extraneous complexity that can potentially mask significant issues, when analyzing the characteristics of various formulations. This has also been a motivation for our paper.

The reviewer asked for a table to show how prior parameters were determined and how they were validated. Discussant provides elastoplastic parameter values for one loading condition for Chia. We present the temperature-dependent relationships Chia fitted, from parameter values at various temperatures, to summarize all such values. Where such relationships are unavailable, we report values at the extremes of temperature. Issues noted in the table for parameter determination pertain to the individual sets of parameters obtained at specific loading conditions, like the values provided by discussant, upon which the relationships are based. We also present Chia's viscous parameters as the elastoplastic formulation is not reasonable for solder simulation.

Our table omits Wang's parameters. Here β is assumed zero as for most metals. Wang estimates n for each test, including $n = 2.09$ for the test presented by discussant, and suggests the average value of 2.15 could be used for all tests because of minimal impact on results. Wang's results are described in Desai et al. (2004, JEP 126, pp. 227–236), where Table 1 suggests n is assumed as 2.01 for most solders. It is not clear which values are finally adopted. We do not delve into potential issues with the procedures and assumptions used for computation of n for these tests, such as for the value of 2.09 discussant reports, because (1) the resulting values are all essentially close to 2, and so should be reasonable for solder (2) other issues lead to significant inconsistencies in these parameters anyway. The assumed $\gamma = 0.00112$ along with the observed ultimate shear, $\tau_{ult} = 22.7047$ MPa for the test, gives R as 226 MPa. Wang reports 3R to be 2050.9 MPa for this test, implying $R = 683.63$ MPa instead, as reported by discussant. R and γ are inconsistent for other loading conditions as well, which makes viscoplastic simulations under the Perzyna formulation infeasible. a1 and η1 values reported by discussant, as well as those obtained for other loading conditions, appear to have been computed assuming atmospheric pressure to be 1 MPa instead of 0.1 MPa. Validations for these parameters are discussed elsewhere in our response.