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AUTHORS' CLOSURE

The authors thank the readers for pointing out several important issues. The central point seems to be the question of static equilibrium in the moving hinge method (MHM). The readers' comments on this stems from a misunderstanding of the moving hinge concept. The MHM is one form of the discretization process to approximate the actually continuous curvature change, resulting in a closed-form solution. At the moving hinge, the equilibrium condition is not satisfied, due to moment discontinuity. This is true for a rigid–strain hardening (RSH) material [Fig. 3(c) in the original paper] as well as for a rigid–perfectly plastic (RPP) material [Fig. 2(c) in the original paper]. One must not confuse the *moving hinge* in the present context with the *moving plastic hinge* in the usual sense [see Hopkins (1955) on dynamic loading of beams]. A moving hinge is simply a *curvature discontinuity* and therefore, *moment discontinuity*, whereas a real plastic hinge has an *infinite curvature* in the neighbourhood. Strain hardening renders that infinite curvature finite, resulting in a finite deformation zone. Since a "hinge" with a finite length is no longer a hinge, a plastic hinge exists only for a non-hardening material. The mistaken equating of the conventional travelling plastic hinge with the now specially meaning moving hinge can also be detected in the work of Wierzbicki and Bhat (1986), who are considered to be important advocates of the MHM.

The moving hinge technique is used as a tool in assessing the global, or macro, structural behaviour, rather than the micro behaviour in the continuum sense, since the total energy balance is maintained. The use of a constant curvature or moment value for a segment is analogous to a constant stress over an element in the finite element method. There are indeed some extra complexities when a RSH material is considered. This has been appropriately addressed in the second last part of the original paper (see Fig. 15). The readers are referred to Lu and Sherbourne (1992, 1993) for more discussions on the essence of the moving hinge.

The authors acknowledge the dependence of the ring shape on the material behaviour (and other factors such as contact friction), which is precisely why certain parameters are included in eqn (22) of the original paper to allow for modelling flexibility. One may agree that the use of such methodology is not uncommon. Such an approach is also a component of the so-called inverse approach. The readers point out that "in rings of materials showing upper and lower yield phenomenon such as mild steel, a dumb bell or peanut shape is observed". However, in the model proposed by two of the readers (Reid and Reddy, 1978), a simple linear hardening law and the deformation shape with a flat top and bottom [see Fig. 5(c) in Reid and Reddy (1978)] are used. The violation of the actual behaviour is no less than in the present proposal.

The accuracy of the measurements from the imprints of the ring shape are considered satisfactory since the experiments involved rings with a length of only 0.5 in. In longer rings, the anticlastic curvature effect becomes significant. It is noted that such an effect was observed by Reddy and Reid (1980) but not dealt with in their model (Reid and Reddy, 1978). The results of the work by Reid and Reddy (1978) do satisfy the equilibrium and geometrical compatibility. Unfortunately, the load–deflection curve was obtained on a point-by-point basis (i.e. the deflection is calculated by assuming an applied load at a certain deformation state), disregarding the loading history. This shaky approach defeats the effort to "ensure that both slope and curvature are continuous...". Reid and Reddy (1978) admitted that the growth and shrinking of the hardening zone (*BH* in Fig. 3 of the Letter) cannot be the case, which fundamentally deviates from experimental observation or simple intuition. The only theoretically well-founded work, in the opinion of the authors, is that of DeRuntz and Hodge (1963), who are among early pioneers in dealing with this

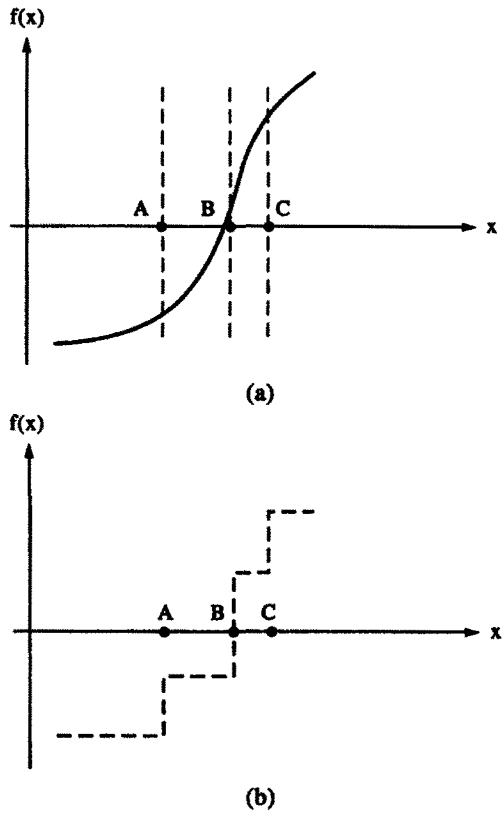

Fig. 1. Approximation of a continuous curve into a step curve.

subject. Equation (36) in the original paper can be derived by tracing the exact loading history in the context of plasticity theory. The results of DeRuntz and Hodge (1963) are qualitatively elegant but quantitatively underpredicting. The use of a single plasticity theory fails to capture the complexities of the crushing behaviour.

The so-called inverse method was not criticised by the authors. On the contrary, it is advocated. The adoption of the MHM manifests itself. A deformation mode based on experiments, or any *a priori* knowledge is relatively more reliable and meaningful. A large number of problems, particularly large displacement crushing and post-buckling problems, are solved in this manner, no matter what the basis on which the mode is chosen. This is the essence of the kinematic approach.

In summary, the MHM expounded in the original paper is well founded. The key word here is "jump" at the moving hinge. As shown here in Fig. 1, the continuous curve in Fig. 1(a) can be approximated as a step function in Fig. 1(b). If the function f happens to be a bending moment, there is bound to be a violation of the static equilibrium at locations A, B and C. The global effect is nevertheless minimal.

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