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Reply

Reply to: Dr. Lip Teh's discussion on "Work-conjugacy between rotation-dependent moments and finite rotations" [Vol. 40, No. 11, pp. 2851–2873]

We are grateful to Dr. Lip Teh for his stimulating discussion and also for the praise of our work. Although it is clear that the authors and the discusser do not share exactly the same points of view, it must be said that most of the differences can be attributed to different standpoints: while the discusser focus his attention on the individual beam element (member) and sees its end moments as 'external', the authors look at the structure as a whole and only the applied moments are considered 'external'. Our reply focuses five main points, which are sequentially addressed below.

1. Nodal moment definition

In order to challenge Izzudin's (2001) assertion that any (nodal) end moment definition can be adopted without compromising accuracy, the discusser cites Argyris et al. (1978, 1979b).

To clarify this issue, one needs to realize that the adopted nodal moment definition must be the same for all elements joining at a node. Thus, for a node connecting two beam elements, it is not acceptable, for instance, to view (i) one end moment as quasi-tangential and the other as semi-tangential (Yang and McGuire, 1986a) or (ii) both as quasi-tangential but with different lever orientations (Argyris, 1978). If this aspect is properly handled, then there is nothing wrong with using the energy method, as observed by Krysl (1993) (we thank the discusser for bringing this reference to our attention).

At equilibrium configurations, the total nodal moments (difference between internal and external moments) are null and, in the absence of applied moments, the same applies to the internal nodal moments. So, we are, in fact, arguing about how a null nodal moment changes when it rotates. The discusser is certainly aware of this, since a similar conclusion was reached in Teh and Clarke (1999), albeit from a slightly different perspective.

It seems that the discusser views the out-of-balance nodal moments as external, whereas we see them as internal. Since they vanish when the iterative procedure reaches equilibrium, in the end it all boils down to nothing. Nevertheless, our definition seems preferable because, for conservative loadings, it leads to a symmetric tangent operator at all stages (and not only at equilibrium).

In short, we restate our agreement with Izzuddin's (2001) assertion that *any* nodal moment definition can be adopted, provided that this definition is consistently used for the end moments of all connecting beam elements. Moreover, if applied moments are present, they must be written in terms of the adopted nodal moment definition, as recognized in Ritto-Corrêa and Camotim (2002) and Battini and Pacoste (2002).

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2. Internal moment conservativeness

We cannot agree with the discusser's statements on the conservativeness of follower, or otherwise rotation-dependent, *internal* moments. Only the rotation-dependency of the applied moments is relevant, which is precisely the reason why our paper has focused on them.

If it is true that conservative *applied moments* must be rotation-dependent, the conservativeness of *internal moments* is a totally different issue, related to the existence of a bending/torsional elastic potential energy. To state that conservative internal moments must be rotation-dependent is confusing, to say the least. If some classification is required, it is preferable to call them *configuration-dependent.*

Of course, if the whole structure suffers a rigid-body rotation, all stress resultants rotate accordingly, which probably explains why the discusser is so fond of follower moments. However, from this point of view, the axial and shear forces are also *conservative follower internal forces*. In our opinion, this kind of reasoning is hardly enlightening.

3. Design oriented nodal moments

It is true that the design of steel frames is based on *stress resultants* such as axial forces or bending moments. These internal forces and moments have a precise meaning at each cross-section, since they represent the stress resultant components w.r.t. a local reference frame. Obviously, the design procedure requires their correct evaluation.

On the other hand, in a finite element context, internal *nodal* moments are quantities of a distinct type, which are (i) evaluated on the basis of the stress resultant distribution along the adjacent beam elements (e.g., see first term on the r.h.s. of the last equation on page 1023 in Ritto-Corrêa and Camotim, 2002) and (ii) written in a global reference frame.

Resorting to equilibrium considerations, it is also possible to obtain the stress resultants on the basis of the end forces and moments (plus the deformed geometry). But, in what concerns the end moments, it is only required to know their *current values* (i.e., their components w.r.t. a chosen reference frame) and no assumption needs to be made about their rotation-dependency.

Hence, we think that the discusser's argument—that nodal moments must be design oriented—does not hold.

4. Tangent operator symmetry

Our discussion on tangent operator symmetry took place in the context of finite elements having work-conjugate nodal moment and rotation definitions. Our main point is that symmetric tangent operators are obtained when the finite rotation update scheme is either additive or multiplicative semi-tangential. Incidently, we do not see any apparent inconsistency between Eq. (24), based on the additive rotation vector, and Section 5.3, which addresses multiplicative updates based on spin-like variables.

Co-rotational formulations, in which the configuration of each beam element is written in terms of a local reference frame that continuously rotates with the element (Crisfield, 1991, 1997), can be achieved in several ways and, as pointed out by the discusser, the formulation framework can affect the tangent operator symmetry. For instance, the co-rotational formulation due to Spillers (1990) relies on an additive rotation update but has an asymmetric tangent operator. This is explained by the fact that the moment equilibrium equations, written directly in the deformed configuration, are not work-conjugate to the additive rotations.

Furthermore, while most co-rotational formulations have asymmetric tangent operators, it is possible to obtain symmetric ones. For instance, see Oran (1973), Kassimali and Abbasnia (1991), Sokol (1996) and Crisfield's comments (1997, p. 247).

We also point out that Cardona and Géradin (1988) did not develop any co-rotational formulation, if one abides by the above generally accepted definition. The asymmetric version alluded by the discusser is, in fact, a geometrically exact formulation, in which material spins are used to perform multiplicative updates. If such formulation is termed co-rotational (because the material strains and stress resultants are expressed in a reference frame attached to each cross-section), then all formulations based on Reissner-Simo beam theory—including the three versions presented by Cardona and Géradin (1988) and the ones of Simo Vu-Quoc (1986) and Ritto-Corrêa and Camotim (2002)—would also qualify as co-rotational.

Moreover, although the multiplicative update used by Cardona and Géradin (1988) does not fit exactly the format of our Eq. (97), Eqs. (98)–(103) still apply and, since the material spin is not semi-tangential, it is not surprising that such an update leads to an asymmetric tangent operator.

The bottom line is: while there are many paths to tangent operator asymmetry, most of them are indeed due to the non-vectorial character of 3D rotations and/or the lack of work-conjugacy between the adopted nodal moments and nodal rotations.

5. Quasi-tangential and fourth-kind moments

The discusser suggests that we also address the better known quasi-tangential moment. By the way, our opinion is that bending moments, like all internal moments, should not be viewed as rotation-dependent (hence, they are not quasi-tangential).

Unfortunately, we are unable to follow this suggestion precisely by the same reason that we cannot analyze the discusser's fourth-kind 'conservative' moment: we lack a satisfactory moment-rotation law in the finite rotation domain, due to difficulties in establishing a general kinematical condition between the strings and disks.

In addition, as hinted in the introduction of our paper, we strongly suspect that the string–disk kinematical condition is non-holonomic (even for rotations smaller than 90°). If this is the case, the 'transparent and physically meaningful way' used by the discusser to determine the conservativeness of Teh and Clarke (1997) moment mechanism is fatally flawed. In contrast to the implicit claim of simplicity, we find such (follower-like) mechanism very difficult to analyze when the disk rotation is (i) not infinitesimal and (ii) not aligned with its axis. The assumption of small rotations is appropriate in many occasions, but certainly cannot be trusted to analyze the conservativeness of moments (for instance, an axial moment would also be deemed conservative in the infinitesimal range).

In any event, on the basis on the work carried out in the paper, what we may state beyond any doubt is that both a follower moment and its first order approximation are not conservative.

6. Concluding remark

Finally, after thanking once more the discusser for this fruitful and pleasant exchange of ideas, we would like to reply to his allusion to the warnings of Biot (1965) by recalling Truesdell and Noll (1965, p. 237) observation that 'even the giants of mechanics have stumbled when [...] they leaned upon the infinitesimal theory as a conceptual guide rather than just a particularly simple approximation to elastic response'.

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