

## LETTER TO THE EDITOR

Discussion of “A variational theory for finite-step elasto-plastic problems”,  
*Int. J. Solids Structures*, Vol. 30, No. 17, pp. 2317–2334 (1993).

The subject paper presents a general sub-stationarity property of the solution to the finite-step boundary value problem which arises when an implicit, backward-difference scheme is adopted for the time integration of the relation set governing the small deformation quasi-static evolution of an elastic–plastic solid. In the writers’ opinion, the authors should be commended for this result and for the systematic study, documented by several papers, of its mathematical foundations.

From their general stationarity principle, the authors derive, by specialization, a minimum principle in terms of displacements, plastic strains and internal variables and expound a fairly detailed “critical comparison with an analogous minimum principle” proposed by Comi and Maier (1991) with reference to boundary element discretization in space, and by Comi *et al.* (1992b) with reference to semi-discretization in finite elements.

On page 2331 of the original article, the authors state that the latter principle “does in fact hold true”. However, they also add that “its statement, being *non-optimal*, belongs to the category of *variational crimes*”. The statement is called non-optimal, inasmuch as the feasible set involved in it “turns out to be a proper subset of the variationally consistent one derived in the present paper”.

This occurrence of “constraint conditions in surplus to the minimal set” or “clandestine” conditions (page 2332) is called, by the authors, an *ill-constraining* phenomenon and is regarded as a possible source of “coarser” and “poorer results”, and of “unduly complicated algorithms” (pages 2318 and 2332).

In the writers’ opinion, the expression *variational crimes*, employed first by G. Strang with reference to completely different situations, is rather misleading and hardly appropriate in the present context. Among different variational properties that hold true, which one is optimal appears to be a rather subjective, purpose-dependent judgement.

As for the solution algorithm, the authors do not point out any algorithm related to the extremum characterization of the finite-step solution they arrive at. Likewise, solution algorithms were never intended by the writers to be “generated” through the earlier extremum theorem referred to by the authors, nor through other extremum properties proposed in papers related to those quoted by the authors. Instead, extremum theorems presented by the writers (Comi and Maier, 1991; Comi *et al.*, 1991a) were used in order to investigate computational features of popular predictor–corrector algorithms (primarily to derive sufficient conditions for convergence). By means of a similar *ad hoc* path of reasoning, an extremum theorem and some computational corollaries have been proposed by Comi *et al.* (1991b; 1992a) in structural dynamics with *softening* associative elastic–plastic constitutive laws (unstable in Drucker sense, with nonconvex locked-in strain energy associated to internal variables), i.e. beyond the range of validity of the author’s results. In fact, the elastic–plastic material behaviour assumed by the authors, and described by a quite general internal variable constitutive law, complies with Drucker’s stability postulate (i.e. exhibits associativity, convexity and no softening).

The subject paper also points out a comparison between, on one hand, approaches resting on “classical variational theory requiring differentiability of the involved functionals” and, on the other hand, approaches which “must be exploited following the general guidelines provided by concepts and methods of convex analysis and potential theory for monotone multi-valued operators” (page 2318). Such a general comparative assessment of

merits may be timely, after so many recent developments in “non-smooth” mathematics, convex and nonconvex functional analysis and mathematical optimization theory due to Clarke, Moreau, Panagiotopoulos, Rockafellar and others. The authors have vigorously contributed to these developments, especially to the “brand-new non-smooth potential theory” (page 2332), by which the results presented in the subject paper have been derived. The subject paper gives evidence that this theory is able to provide “general guidelines for a direct variational formulation of problems initially put forth in terms of multi-valued operators”. It may also be true that “up to now travellers without non-smooth potential tools in their luggage were compelled to discover variational principles” by “skillful intuition” and “*ad hoc* procedures” (page 2332).

The approach leading to variational principles on the basis of merely classical “smooth” mathematics and mechanical arguments, may actually have the above drawbacks. However, as for elasto-plastic step problems in engineering mechanics and their computation-oriented theory, the usual differentiability assumption of yield functions (which obviously rules out the origin except in piecewise-linear plasticity) and possible “clandestine” (page 2332) constraint conditions represent minor disadvantages, in the writers’ opinion (of course, provided solutions are characterized by necessary and sufficient variational conditions). Moreover, the following circumstances should not be overlooked as possible advantages of an approach with a serendipity flavour. Generally, it precedes in time, stimulates and sometimes advocates the construction of a unifying and abstract theoretical framework. Using the authors’ metaphor, it permits travel with lighter luggage to provide an easier insight into the role and consequences of constitutive features and to avoid digressions on so far unfamiliar mathematical preliminaries.

As a conclusion, we fully acknowledge the beauty and inherent potentialities of approaches resorting to non-smooth mathematics in order to systematically generate variational characterizations of solutions in plasticity and other areas of nonlinear mechanics. The writers agree that results, such as those claimed by the authors in the subject paper, are valuable contributions to re-shape plasticity theory for more possible general systematizations. However, a *per se* beautiful and powerful branch of mathematics, if regarded as a tool to which physicists and engineers resort according to the objectives pursued, is unlikely to be exclusive or optimal in absolute terms. In the writers’ opinion classical approaches may still be useful, with comparable dignity, in engineering mechanics where, borrowing J. W. Gibbs’ words (1881), “the principal objective of theoretical research is to find the point of view from which the subject appears in its greatest simplicity”. R. Bellman’s recommendation to wend the path “between the pitfalls of oversimplification and the morass of overcomplication” is appropriate and well received; but that path seems to us not so neatly defined and not necessarily unique in engineering mechanics and plasticity.

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