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REPLY TO DISCUSSION

REPLY TO "DISCUSSION OF 'ON FINITE PLASTIC FLOWS OF COMPRESSIBLE MATERIALS WITH INTERNAL FRICTION'", *INT. J. SOLIDS STRUCTURES* 16, 495–514 (1980) BY J. W. RUDNICKI

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I wish to thank Dr. Rudnicki for providing me with this opportunity to bring into focus some major points concerning the description of elastoplastic behavior of frictional materials.

While I am flattered by Dr. Rudnicki's generous designation of the constitutive *assumptions* proposed by Shokooh and me as "laws", I wish to dispel any misunderstanding that such designation may cause, because *macroscopic* plasticity theories are, in my view, essentially *empirical* or, at best, stem from empirical inferences, and, therefore, do not qualify as *laws*. They are, at best, approximations with limited applicability.

Since Dr. Rudnicki begins by emphasizing similarities between the Rudnicki and Rice contribution and my work with Shokooh, it may be appropriate and instructive for me to first focus on the differences, and then address some of the fundamental issues which bear on both contributions.

FLOW VERSUS DEFORMATION PLASTICITY

The literature has witnessed considerable discussion on relations between the *proper flow plasticity* theories and the *deformation plasticity* ones, at least beginning with the work of Hencky[1]; see, e.g. Prager[2] and Hill[3], and, more recently, Neale[4], who gives a nice summary. The proper *flow* plasticity theories for metals are motivated from the assumption that slip over crystallographic planes is the major contributor to inelasticity. Macroscopically, a yield function is considered, which may or may not be smooth, and a flow potential is assigned, which may or may not be the same as the yield function, and, again, which may or may not be smooth.

The *deformation* theories of plasticity, on the other hand, are motivated from the *elasticity* theories and may be a proper elasticity type (commonly called *hyperelasticity*) if a strain energy function exists, or they are a *hypoelasticity* type when an objective stress rate (say, Jaumann rate of Cauchy stress) relates to the strain rate by

$$\overset{*}{\sigma}_{ij} = H_{ijkl}D_{kl}, \tag{7}†$$

where H_{ijkl} is a general fourth order tensor with obvious symmetries that depends on the state of stress and on history through some scalar parameters (internal variables), but is independent of rates. Hypoelasticity of this type was proposed by Truesdell[5], and has been discussed extensively by Green[6, 7]. Essentially, all commonly used deformation theories of plasticity are special cases of this type of hypoelasticity. For isotropic relations it can be shown that only twelve material functions are involved and (7) can be given explicit polynomial representation in terms of the Cauchy stress $\boldsymbol{\sigma}$, the deformation rate tensor $\mathbf{D}$, and their relevant combinations; see [8]. The application to frictional geological materials has been discussed by Romano[9] and Davis and Mullenger[10].

Dr. Rudnicki correctly states that the Rudnicki–Rice theory "can be interpreted as a deformation plasticity theory." Since all common deformation plasticity theories are obtained by generalizing the elasticity theory, I do not understand why he finds incorrect the statement

†Equation numbering follows that of the Discussion above.

by Nemat-Nasser and Shokooh, namely that Rudnicki and Rice's deformation theory is "deduced by generalizing linear elasticity equations." Indeed, Stören and Rice [11] start with the total deformation relation

$$\epsilon'_{ij} = \lambda\tau'_{ij}, \tag{8}$$

where prime denotes the deviator, regard λ nonconstant, differentiate (8) with respect to time,

$$\dot{\epsilon}'_{ij} = \lambda\dot{\tau}'_{ij} + \dot{\lambda}\tau'_{ij}, \tag{9}$$

and also consider

$$\gamma^2 = 2\epsilon'_{ij}\epsilon'_{ij} = 2\lambda^2\tau'_{ij}\tau'_{ij} = 4\lambda^2\tau^2, \tag{10}$$

where the definitions of γ and τ are clear. Differentiation of (10) in conjunction with (9) then gives

$$\dot{\epsilon}'_{ij} = \frac{1}{2h_s}\dot{\tau}'_{ij} + \frac{\dot{\tau}}{2\tau}\left[\frac{1}{h_t} - \frac{1}{h_s}\right]\tau'_{ij}, \tag{11}$$

where h_t and h_s are the tangent and secant moduli in the plastic τ vs γ curve. Stören and Rice then identify $\dot{\tau}_{ij}$ with the Jaumann rate of the Cauchy stress, and $\dot{\epsilon}_{ij}$ with the plastic part of the deformation rate tensor. The corresponding equation is clearly motivated by generalization of elasticity, in the manner discussed for small strains by Budiansky [12].

The secand modulus h_s, however, must remain positive if the above interpretation holds. As I shall explain below, a parameter of this kind (noncoaxiality parameter) emerges in a natural way in modeling geological materials. It, however, does not have the interpretation of secant modulus and, in fact, may be *negative*.

From the above remarks, therefore, it is clear that a major difference between Nemat-Nasser and Shokooh's work and Rudnicki and Rice's contribution (eqns (3) above) is that the former is a flow plasticity theory, whereas the latter is a deformation plasticity theory.

It has been now generally accepted that the deformation plasticity theories may be good approximations to flow plasticity theories, if proportional or nearly proportional loadings are involved. For unstable deformations or close to the tip of cracks, e.g. one expects considerable deviation from proportional loading. Therefore, for these problems the deformation type plasticity seems physically less suitable, although, as Dr. Rudnicki points out, theories of this kind have been extensively used for such cases with often better results, possibly because they include additional parameters. Indeed, in the case of nonlinear fracture, deformation theories have been used extensively for small strains, but as pointed out by Rice [13], they are in such cases essentially nonlinear elasticity, and as further stressed by Hutchinson and Paris [14] more recently, they do not apply to regimes (close to the crack tip) that deviate substantially from proportional loading.

THE DILATANCY FACTOR

Rudnicki and Rice introduce two parameters, μ and β, which represent the overall frictional coefficient and the dilatancy, respectively. Both are regarded as *material* parameters which must be either measured experimentally or defined otherwise.

In contrast, Nemat-Nasser and Shokooh regard the dilatancy factor as a *response* parameter which must be calculated as part of the solution, like other strain rate measures. Indeed, for continued plastic flow, since a rate independent material is involved, one may use the effective plastic strain

$$\bar{\gamma} = \int_0^\theta (2D^{p'}_{ij}D^{p'}_{ij})^{1/2}\,\mathrm{d}\theta \tag{12}$$

as the time parameter, and observe that

$$\beta = \frac{D^p_{kk}}{\bar{\dot{\gamma}}} = D^p_{kk} \tag{13}$$

which is the rate of plastic volume change per unit current volume.

In general, there is no reason to believe that β is a material parameter different from other components of the strain rate.

Dr. Rudnicki states that both μ and β are material parameters that can vary with the state. However, he provides no recipe to estimate such variations. Surely, constitutive relations which include material parameters that can vary from point to point in a given test and from test to test in an unspecified manner, are not very useful, and I don't think Dr. Rudnicki means to say this. Therefore, these parameters, if not specified, can rightly be regarded as material constants similar to Young's modulus and Poisson's ratio, with minor (experimental) variation in magnitude. Indeed, lacking an explicit recipe, Rudnicki and Rice assign *constant* values to dilatancy factor β, as well as to μ in all their calculations of localized deformations contained in their Tables 1–3.

In contrast, Nemat-Nasser and Shokooh consider energetic balance, estimate the frictional loss, and obtain the following dilatancy equation for the conventional triaxial test:

$$\sqrt{3}\frac{\partial G}{\partial p} = 3\frac{\partial F}{\partial p} - \frac{q}{p}, \tag{14}$$

where $\partial G/\partial p = -\beta$,

$$f \equiv \tau - F(\rho,\Delta,\xi) \quad \text{and} \quad g \equiv \tau + G(\rho,\Delta,\xi), \tag{15}$$

where p is the pressure, $q = \sigma_1 - \sigma_2$ is the stress difference in triaxial tests with $\sigma_1 > \sigma_2 = \sigma_3$, and ξ and Δ are the total distortional plastic work and the total plastic volume change measured per unit reference volume. Since $\partial F/\partial p$ is the coefficient of overall friction, it is always positive. Then, for small q, eqn (14) shows that one always starts with initial densification ($\partial G/\partial p > 0$ for compaction). As q increases, the rate of volume contraction decreases, until the r.h.s. of (14) is zero, and then, as q increases further, dilatancy begins. The dilatancy attains its maximum value at peak q, and then decreases as q decreases (post failure), becoming zero at the critical state. These are all observed experimentally, and, in fact, eqn (14) is in good quantitative accord with experimental data, as is shown by Nemat-Nasser and Shokooh.

Even for rocks, initial densification induced by shearing may occur, depending on the nature of the rock and the state of stress. Indeed, in certain sandstones and for certain loading regimes, no dilatancy may occur, as has been discussed by Schock *et al.*[15] for Graywackes sandstones.

DILATANCY AND WORK-HARDENING

Another major difference between the two contributions in question is the fact that Nemat-Nasser and Shokooh obtain strong coupling between the work-hardening parameter H, and the dilatancy factor $\beta \equiv -\partial G/\partial p$, whereas no coupling is transparent from eqns (1) and (3) of Discussion. This is essentially because Nemat-Nasser and Shokooh deal with the explicitly stated yield function and flow potential, eqns (15).

Dr. Rudnicki states that "Nemat-Nasser and Shokooh also introduce a second plastic modulus....." This statement is incorrect, as these authors do not *introduce* a second modulus, but rather, a density-hardening modulus *emerges* from the basic assumptions given by (15) above. In fact, as discussed by Nemat-Nasser and Shokooh, the density-hardening parameter,

$$h_1 = \frac{\rho_0}{\rho}\frac{\partial G}{\partial p}\frac{\partial F}{\partial \Delta} \tag{16}$$

may be positive, negative, or zero, depending on the sign of $\beta = -\partial G/\partial p$; for dilatancy, $\partial G/\partial p$ is regarded negative. *This coupling, therefore, leads in a natural way to a stress–strain relation which admits a peak stress.*

In contrast, as Dr. Rudnicki states, the hardening parameter H in (3) or in (1) of Discussion "is related to the slope of the shear stress τ versus shear strain γ curve at constant hydrostatic stress....", suggesting that quantitatively and qualitatively the variation of H in (1) and (3) must be obtained empirically.

VERTEX MODULUS, NONCOAXIALITY, AND CORNER

A major component in the Rudnicki and Rice paper relates to a second modulus identified by H_1 in eqn (3) of Dr. Rudnicki's Discussion. The associated term in (3) makes the plastic deformation rate tensor to be *noncoaxial* with the stress tensor. No such term is obtained if the plastic strain rate is derived from a smooth flow potential that depends on stress invariants; as observed by Dr. Rudnicki, this does not seem to have any significant bearing on the problem considered by Nemat-Nasser and Shokooh. This is an important difference between the Rudnicki–Rice contribution which does include noncoaxiality, and the Nemat-Nasser and Shokooh one, which does not.

For geological materials, the noncoaxiality is an important factor. Indeed, precisely the same expression (but with a negative sign) is contained in the works of Mandel[16] and Spencer[17], and (with the positive sign) in the work of Mandl and Férnandez Luque[18]; these authors consider two-dimensional problems.

In fact, one of the most important results in Spencer's double sliding theory of the flow of geotechnical materials is this noncoaxiality term; see Spencer[17], eqn (3.28). This equation can be written as (using the present notation)

$$D_{ij}^{p'} = \lambda \frac{\sigma'_{ij}}{\tau} - \frac{1}{2}\sin\phi \left(\frac{\sigma'_{ij}}{\tau}\right)^{*}, \tag{17}$$

where ϕ is the angle of overall friction, τ is $\sqrt{J}$, as before, and superposed * denotes the Jaumann rate; see Spencer[19], eqns (6.13) and (6.15), who gives a historical account and a detailed derivation.

Since (σ'_{ij}/τ) is a tensor of constant magnitude, its rate is "normal" to itself so that

$$\left(\frac{\sigma'_{ij}}{\tau}\right)^{*} \sigma'_{ij} = 0, \tag{18}$$

and hence the corresponding contribution to the rate of plastic distortion is "workless." Indeed, since

$$-\frac{1}{2}\sin\phi \left(\frac{\sigma'_{ij}}{\tau}\right)^{*} = -\frac{\sin\phi}{2\tau}\left[\overset{*}{\sigma}'_{ij} - \frac{1}{2\tau^2}\sigma'_{ij}\sigma'_{kl}\overset{*}{\sigma}'_{kl}\right], \tag{19}$$

we see that the Mandel-Spencer noncoaxiality parameter is given by

$$H_1 = -\frac{\tau}{\sin\phi} < 0. \tag{20}$$

In a later work, Mandl and Férnandez Luque[18] obtain the same noncoaxiality (but with a positive sign) by a different consideration. Recently, Christoffersen *et al.*[20] have given a micromechanical description of the plastic flow of granular materials, and have obtained a noncoaxiality equation which has the positive sign.

Mandel[21] in 1966 rejects his own contribution of 1947 and the noncoaxiality of Spencer on the grounds that they do not fall within the classical plasticity with associative flow rule, see Mandel[21], eqns (10) and (11), and the discussion that follows. (If we exclude the elastic part displayed by the shear modulus G in eqn (5) of Dr. Rudnicki's Discussion, then this equation becomes identical with eqn (11) of Mandel for $i = 1$ and $j = 2$, with H_1 given by (20) above; note that eqn (11) of Mandel is equivalent to eqn (3.28) of Spencer[17].)

Hence, I agree with Dr. Rudnicki that the noncoaxiality parameter, H_1, is of some fundamental importance to frictional materials. However, I find the interpretation of H_1 as a secant modulus unnecessarily restrictive. It seems to me that the Mandel–Spencer work and my work with Christoffersen and Mehrabadi support these remarks. I also agree with Dr. Rudnicki that this term only "approximately" accounts for the response at a yield surface vertex. Therefore, it is necessary and useful to keep the noncoaxiality and the response at the yield surface vertex as two separate phenomena.

FURTHER COMMENTS

From Dr. Rudnicki's comments, one may infer that there should be some advantage in presenting constitutive relations which contain unspecified parameters. This would be incorrect, otherwise eqns (7), which are far more general than (1), (3), and certainly those of Nemat-Nasser and Shokooh, should be preferred. However, eqns (7) do not add much to our knowledge of the plastic flow of frictional materials, whereas the less general equations (1), (3), and especially those of Nemat-Nasser and Shokooh, do. Furthermore, these more specific results serve as a guidance for further (specific) generalizations for application to other classes of problems.

For example, eqns (1), (3), and those obtained from (15) are essentially for *two-dimensional stress states.* It is well-known that the *stress triaxiality* (i.e. when the principal stresses are such that $\sigma_1 \neq \sigma_2 \neq \sigma_3$) plays an important role in affecting the response of frictional materials; see, for example, Mogi[22], Lade and Duncan[23], and Nemat-Nasser[24] who gives additional references. It is clear that plasticity theories based on the J_2-flow potential, including the pressure effect, are not adequate for problems of this kind. On the other hand, eqns (15) do provide a framework from which one may obtain more general yield functions and flow potentials for application to the true triaxial stress states. This has been briefly discussed in [24] and extensively illustrated in [25]. The simple but effective modification suggested in [24, 25] is to replace eqns (15) by

$$
\begin{aligned}
f &\equiv (1 + c(\eta))\tau - F(p, \Delta, \xi),\\
g &\equiv (1 + c(\eta))\tau + G(p, \Delta, \xi),
\end{aligned}
\tag{21}
$$

where $\eta = J_3/\tau^3$, $J_3 = \frac{1}{3}\sigma'_{ij}\sigma'_{jk}\sigma'_{ki}$, and c is a function of η. All previously obtained results by Nemat-Nasser and Shokooh are now deduced as special cases. In [25] the form of the function c is fixed by comparison with experimental results. It is then shown that, for cohesionless sands, the form and magnitude of major parameters can be fixed for application to general *true* triaxial stress states with the aid of the results obtained from only two *conventional* triaxial tests: (1) a compression test where $\sigma_1 > \sigma_2 = \sigma_3$; and (2) a "tension" test where $\sigma_1 = \sigma_2 > \sigma_3$ (principal stresses are regarded positive when compressive, and even in the tension test all principal stresses remain compressive).

Since the noncoaxiality term in eqn (3) of Discussion, or in eqn (17) above, is really tailored for two-dimensional stress states, as is clear from the Mandel–Spencer double sliding theory, it would be useful to see how it should be generalized for application to *true* triaxial stress states, i.e. when $\sigma_1 \neq \sigma_2 \neq \sigma_3$. This, however, will take us far away from the subject of the present "reply".

Before closing, I wish to correct a misprint which occurs in eqn (4.30) of Nemat-Nasser and Shokooh; this equation should read

$$
\frac{dq}{d\epsilon} - \frac{\sqrt{3}}{3} M \frac{dp}{d\epsilon} = a\left(M - \frac{q}{p}\right) + \hat{h}.
$$

Again, I wish to express my appreciation to Dr. Rudnicki for initiating this exchange which, I am sure, will be useful to the scientific community and should provide stimulus for further scientific exchange and progress in this rather difficult area of material behavior.

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