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ON THE SUBSTANCE OF RIVLIN'S REMARKS ON THE ENDOCHRONIC THEORY

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INTRODUCTION

In a recent manuscript, appearing in the present issue of this journal[1], Rivlin engaged in a lengthy commentary on the endochronic theory. The reader who is familiar with the theory knows that the latter owes its inception to a sequence of two papers by Valanis, published in the Archives of Mechanics in 1971[2, 3]. The physical basis of the endochronic theory lay in the theory of irreversible thermodynamics of internal variables and its purpose was the development of explicit constitutive equations for modelling plastic or viscoplastic behavior of materials subjected to small or large deformation.

The motivation for developing such a theory was two-fold and consisted first of a desire on my part to develop an alternative theory of plasticity which did not require the assumption of yield for its development, and second of an undertaking to bring the constitutive theory of plastic behavior under the aegis of irreversible thermodynamics of internal variables, a task which theretofore had not yet been accomplished.

Since its inception, the theory has undergone significant evolution, the driving force always being the desire to describe as many aspects of material behavior as possible with the simplest possible and most elegant form of a constitutive equation.

It is therefore quite odd, to say the least, that Rivlin's remarks are, in the main, addressed to the earliest versions of theory. His comments on the later developments are scant, inaccurate and amount to no less than a total misrepresentation of the work. Let it be said, however, that his comments on the earlier work are hardly better.

Rivlin's barrage of criticism can be divided, for the purposes of rebuttal, into *five* categories.

(i) Errors of fact.

(ii) Contentions and unsubstantiated criticisms and general charges without foundation.

(iii) Presumed shortcomings of the theory vis-a-vis criteria which are either generally invalid or unsubstantiated.

(iv) Perceived peculiarities of the theory in terms of *mathematical* constraints which have no physical foundation.

(v) Differences in style and general philosophy, regarding the purposes and scope of the field of constitutive equations.

For the purposes of enlightenment of the reader and to set the grounds for the subsequent discussion I would like to point out at the outset that there is no such thing as a *unique* endochronic theory of constitutive behavior.

The term "endochronic theory" encompasses all those theories in which "*the state of stress at the present time is a function of the history of strain with respect to a time scale, which is not the absolute time scale measured by a clock, but a time scale which in itself is a property of the material at hand*"[2].

For instance the classical linear theory of viscoelastic materials is *not* an endochronic theory because the "material memory" therein is defined with respect to the absolute time scale measured by a common clock.

The "arc length" theory originally proposed by Ilyushin[4] and elaborated upon by Rivlin[5] and Pipkin[5, 13] is *not* an endochronic theory since the material memory scale "s" introduced therein was defined for *all* materials by the relation

$$ds^2 = \mathrm{tr}(d\epsilon^2)$$

where ϵ is the strain tensor.

In my original paper[2] I defined an intrinsic time measure $d\zeta$ by the relation

$$d\zeta^2 = d\epsilon \cdot \underset{\sim}{P}(\epsilon) \cdot d\epsilon$$

where $\underset{\sim}{P}$ is a positive definite symmetric tensor. The material time scale z was then defined by the equation:

$$dz = \frac{d\zeta}{f(\zeta)}$$

where $f(\zeta)$ is a positive function. Both $\underset{\sim}{P}$ and f are material functions, rendering the time scale z a material property.

I called the theory endochronic because of this fact. The term "endochronic" was derived from the Greek endos (meaning inner) and chronos (meaning time).

The Notion of Material Invariance. The reader may note that this concept opens possibilities of profound importance in that, as a result, it is possible, at least in principle, to introduce to the field of constitutive theory the concept of constitutive *material invariance*, i.e. a form-invariance of a constitutive equation relative to a *class* of materials. The implication is that *one and the same form of a constitutive equation* applies to a class of materials of different constitution. Different consititutive responses are then brought about by mere changes in the nature of the intrinsic time scale.

Since 1971, when Refs.[2, 3] first appeared in the literature, the theory has undergone a significant evolution[9–11] not in its basic concepts but in the specific forms of constitutive equations chosen to describe specific materials such as metals and soils in specific conditions such as small strain and constant temperature.

It must be noted that the large bulk of Rivlin's comments is addressed to the very original form of the theory. Though he has been apprised by means of letters and copies of reports of recent advances[10, 11], especially as they relate to the application of the theory to metals, he still chose to ignore these and to put on paper comments which are essentially, an attack on certain characteristics of the *early* theory.

It was known to me at an early stage and I reported this to the technical community[6] that certain characteristics of the *early* theory relating to hysteresis loop closure in the first quadrant of the stress strain space made it less suitable for application to metals in that region.

Rivlin knows that more recent advances[10, 11] have eliminated this difficulty yet he has chosen to ignore this information.

ERRORS OF FACT

The title of the Rivlin paper

The title of his paper is grossly misleading. As stated in the introduction, Rivlin *is aware* of recent developments in the endochronic theory, but these he chose to ignore. In light of the contents of his paper, the title should read "Comments on Some *Earlier* Versions of the Endochronic Theory."

Section 1, *paragraph* 2

The separation of the strain increment into elastic and plastic parts was *never* an issue. I have never argued against its use. See Refs.[2, 3], which are my original papers on the endochronic theory.

Section 1, *paragraph* 4

The beginning statement is in error. What was assumed in Ref.[3] was that ". . . . the state of stress in the neighbourhood of a point in a plastic material depends on the set of all previous states of deformation of that neighborhood, but it does not depend on the rapidity at which such deformation states have succeeded one another."

The assumptions of linearity and isotropy are introduced only in the *application* of the theory to specific situations.

Also the end of this paragraph is so worded as to leave open to doubt the published agreement in Ref.[3] between theory and experiment. Rivlin did his own numerical calculations[12] and asked and was supplied with my numerical calculations that corroborated this agreement. The innuendo in his statement has no basis of fact.

Section 5, paragraph 3

The wording of this paragraph is unfortunate. In the first place it leaves the impression that the theory as presented by Valanis in Ref.[2] is a special case of the theory presented by Rivlin and Pipkin in Refs. [5, 13]. THE THEORY OF VALANIS IS *NOT* A SPECIAL CASE OF THE THEORY OF RIVLIN AND PIPKIN PRESENTED IN REFS.[5, 13].

In fact, in the case where a functional relationship between the stress and the strain history exists, *the theory of Rivlin and Pipkin is a special case of that of Valanis.* Specifically in the notation of Ref.[2] the following is a comparison of the definitions of $d\zeta$ and z by Rivlin and Pipkin on one hand and Valanis on the other:

Rivlin and Pipkin

$$d\zeta^2 = d\epsilon_{ij} d\epsilon_{ij} \tag{1}$$

$$z = \zeta \tag{2}$$

Valanis

$$d\zeta^2 = P_{ijkl}(\epsilon) d\epsilon_{ij} d\epsilon_{kl} \tag{3}$$

$$dz = \frac{d\zeta}{f(\zeta)}. \tag{4}$$

The equations of Rivlin and Pipkin *reduce* to the equations of Valanis under the following conditions:

(i) The material is isotopic, the tensor $\underset{\sim}{P}$ is a constant and has the very restricted form

$$P_{ijkl} = \delta_{ij}\delta_{kl} \tag{5}$$

(ii)
$$f(z) = 1. \tag{6}$$

In the second place the theory proposed by Valanis in Ref.[2] is *NOT* a special case of a "set of [constitutive] relations" generated, in part, by Rivlin's eqn (5.5) of his paper.

In fact Rivlin's eqn (5.5) does *not* include my definition for $d\zeta$ given above. For this to be so his eqn (5.5) should read

$$\zeta = \zeta(l) = \int_0^l \phi[\epsilon, d\epsilon(l')]. \tag{7}$$

Note the dependence of ϕ on ϵ. However even this extended definition is still a particular case of my definition which I quote from Ref.[2]: "The independence of stress of the rapidity of succession of deformation states is achieved by introducing a time scale ζ which is independent of t, the external time measured by a clock, but which is intrinsically related to the deformation of the material."

Section 5, paragraph 5

The statement regarding eqn (5.9) is not true. Furthermore k_1 and k_2 in the context of the theory of Ref.[3] are not constants, in general. Also false is the statement that I give no reason for choosing to define $d\zeta$ by eqn (3). Rivlin appears to *choose* references of my work that suit his purposes. Specifically the following is a quote from Ref. [8]:

> "The statement that the stress is a function of the strain "path" gives rise to the question: "which path"? In particular, what is the appropriate path for materials that are "history dependent" but "strain-rate independent"?
>
> We define a path relative to a Riemannian space. Consider a six dimensional Riemannian space R with metric Gij. Of the six independent components E_i of $\mathbf{E}$ let each be measured along one of the coordinates of R, in a sense of "one-to-one and on-to". Evidently a state of strain is a point and a strain history is a path in this space. The distance $d\zeta$ between two adjacent deformation states is given by the relation
>
> $$d\zeta^2 = G_{ij} d\epsilon_i d\epsilon_j$$
>
> or
>
> $$d\zeta = +\sqrt{(G_{ij} d\epsilon_i d\epsilon_j)}.$$
>
> The intrinsic time ζ is arrived at by the following considerations. If G_{ij} is a material property then $d\zeta$ is a material "time-like interval" between two adjacent strain states: As such $d\zeta$ is a measure of an intrinsic time scale. In tensor form,
>
> $$d\zeta^2 = d\epsilon \cdot \underset{\sim}{P} \cdot d\epsilon$$
>
> where $\underset{\sim}{P}$ is a material tensor, which may conceivably depend on ϵ."

Section 7

Poisson's ratio for metals in the elastic range is very closely equal to 1/3 and not 1/4 as Rivlin claims. If the bulk modulus is K and the Young's modulus E it is well known that

$$K = \frac{E}{3(1-2\nu)} \tag{8}$$

where $\nu = 1/3$ then $K = E$.

Section 9, *paragraph* 6

In this chapter Rivlin dicussses the more recent version of the endochronic theory (his Ref. [5]) in which Valanis defined the deviatoric intrinsic time measure by the relation

$$d\zeta_D{}^2 = (de^p_{ij} de^p_{ij})^{1/2} \tag{9}$$

Regretably Rivlin's summary of the paper *is a gross and irresponsible misrepresentation of my work.* Results which were derived from first principles are reported by Rivlin as having been "assumed". I shall elaborate:

One of the central results of this paper is that the deviatoric stress tensor is related to the history of the plastic strain by an equation which with a slight change in notation is as follows:

$$s = s_y{}^0 \frac{de^p}{dz} + r \tag{10}$$

where

$$r = 2\mu_0 \int_0^z \rho_1(z - z') \frac{de^p}{dz'} dz'. \tag{11}$$

This equation, as I pointed out in the above reference, evinces a yield surface.

However, the above results were *not* assumed, as Rivlin casually and offhandedly remarks.

They were *derived* by applying operator theory to the mathematical treatment of the basic thermodynamics equations as derived by Valanis[14] subject to the following stipulations:

(a) The free energy is quadratic in its arguments.

(b) The rate equations are linear, i.e.

$$\frac{\partial\psi}{\partial q_r}+b_r\frac{dq_r}{dz}=0. \tag{12}$$

It is remarkable that the thermodynamics of internal variables do in fact lead—in a special physical context—to a theory akin to the well known plasticity theory. It is even more remarkable that the yield surface and the normality of the plastic strain increment to the yield surface ARE DERIVED RESULTS.

It is in the theories of classical plasticity were these results (or their equivalents) are initial *ASSUMPTIONS*.

It is a sad day in the field of mechanics when a preeminent worker presents the work of a peer in such careless fashion.

In paragraph 10 of the same chapter the misrepresentation is even more blatant. I shall reply to this paragraph in great detail. Rivlin claims that in the aforesaid paper I made 5 assumptions.

(i) I assumed that the total strain can be expressed in terms of its plastic and elastic parts.

(ii) I assumed that the moduli associated with changes in elastic strain are constants, independent of strain history.

In fact I *did* make assumption (ii). For metals under conditions of small strain this assumption is atomistically and physically sound. On the other hand once (ii) is true, then (i) is no longer an assumption but a *definition*. For instance, with reference to the deviatoric strain tensor $\mathbf{e}$

$$\mathbf{e}=\mathbf{e}-\frac{\mathbf{s}}{2\mu_0}+\frac{\mathbf{s}}{2\mu_0} \tag{13}$$

identically. Then by *definition*

$$\mathbf{e}^p\stackrel{\text{def}}{=}\mathbf{e}-\frac{\mathbf{s}}{2\mu_0} \tag{14}$$

$$\mathbf{e}^e=\frac{\mathbf{s}}{2\mu_0}. \tag{15}$$

There is *no* assumption involved here.

Digression: More generally

$$d\mathbf{e}=\left(d\mathbf{e}-\frac{d\mathbf{s}}{2\mu_0}\right)+\frac{d\mathbf{s}}{2\mu} \tag{16}$$

or,

$$d\mathbf{e}=d\mathbf{e}^p+d\mathbf{e}^e \tag{17}$$

by definition. In this case μ need not be a constant but may in fact depend on the history strain. This is treated in a more recent work by Valanis[10, 11] and Read[11].

(iii) Rivlin states that I assumed that a spherical yield surface exists.

This statement is absolutely *false*. The existence as well as the shape of the yield surface are a derived result.

(iv) He further asserts that I *assumed* that the radius r of the yield surface and the position of its center depend on the history of plastic strain.

This is also *false*. Rivlin continues to misrepresent the results of the paper. The conclusions in (iv) were *derived* results.

(v) He then proceeds to state that I made the assumption that if the deviatoric stress lies at a point on the yield surface (corresponding to some specified plastic strain history) and is then changed to a point lying inside, or on, this yield surface, the corresponding deformation is purely elastic.

This statement is falso on two counts:

(a) Statement (v) without the bracket is a result which *I derived*, not assumed as claimed by Rivlin.

(b) The bracket is an insertion by Rivlin. The insertion is simply wrong for the following reason: The plastic strain history does *not* determine the stress point on the yield surface. There is an infinity of points on the yield surface which correspond to one and the same plastic strain history.

Rivlin has missed the whole point of the paper, i.e. that: *Linear Theory* of irreversible thermodynamics with internal variables leads by *analysis*, NOT ASSUMPTION, to a plasticity theory with a yield surface, in the deviatoric stress space where $d\zeta_D$ is defined as

$$d\zeta_D{}^2 = \mathrm{tr}\|de^p\|^2. \tag{18}$$

This is a new and *original* result which shows that *classical plasticity lies within the domain of irreversible thermodynamics with internal variables.* In a similar fashion it is shown that a yield "surface"—the two end points of a region—exists in the one dimensional hydrostatic stress space where

$$d\zeta_H = |d\epsilon^p_{kk}|. \tag{19}$$

This particular result does not appear in the theories of plasticity where only *one* yield surface is talked about.

CONTENTIONS AND UNSUBSTANTIATED CRITICISMS

Introduction—paragraph 3

In the third paragraph of his introduction Rivlin states: "Since the thermodynamic content of Valanis's argument is questionalbe and is, in any case, replete with *ad hoc* (my italics) assumptions, discussion of it is relegated to Appendix A".

This paragraph is, I feel, the determinant of the general level of scientific merit of Rivlin's comments. For, in fact, *nowhere*, let alone in Appendix A, has Rivlin ever substantiated these personal criticisms of irreversible thermodynamics. Regrettably, his Appendix A, to which I shall return, is replete with repetitious technical slurs of this type. (In this regard see also, his Introduction, paragraph 11.)

I believe it behooves Rivlin to substantiate his criticisms of irreversible thermodynamics in the open literature, by rational technical arguments.

Introduction—paragraph 11

In this paragraph one sees a total misrepresentation of the most recent and *telling* development of the endochronic theory. The summary of the paper by Valanis referred to, therein, is in fact *wrong* (again).

In that paper[9] (Rivlin's Ref.[5]) a deviatoric intrinsic time $d\zeta_D$ was introduced by the following relation

$$d\zeta_D{}^2 = \left\|de - \frac{k_1}{2\mu}ds\right\|^2. \tag{20}$$

A similar expression was introduced to define a hydrostatic intrinsic time scale $d\zeta_H$ by a similar relation:

$$d\zeta_H{}^2 = \left|d\epsilon_{kk} - \frac{k_0}{3K_0}d\sigma_{kk}\right|^2. \tag{21}$$

In the above, $0 \le k_0 \le 1$, $0 \le k_1 \le 1$.

Of importance are th following facts:

(a) If $k_0 = 0$, $k_1 = 0$, then this version of the theory reduces to the previous one. *But* if $k_0 = k_1 = 1$, then the intrinsic times $\mathrm{d}\zeta_D$ and $\mathrm{d}\zeta_H$ are defined in terms of the plastic components of strain.

(b) Intermediate values of k_0 and k_1 are obviously possible and are governed by the inequalities on k_0 and k_1.

These intermediate values *control* the degree of closure of hysteresis loops in the first quadrant of the stress strain space insofar as one dimensional (sheer, bulk, axial, etc.) responses are concerned.

Only when $k_1(k_0)$ is equal to unity does a yield surface in deviatoric (hydrostatic) space appear.

In this sense plasticity is a *limiting* case of the endochronic theory.

In a more recent paper[10] it was *further* shown that if one retains the definition

$$\mathrm{d}\zeta_D{}^2 = \|\mathrm{d}\mathbf{e}^p\|^2 \tag{22}$$

$$\begin{gathered} \mathrm{d}\zeta_H{}^2 = |\mathrm{d}\epsilon^p_{kk}|^2 \\ (k_0 = k_1 = 1). \end{gathered} \tag{23}$$

One may still eliminate the yield surface, whereupon the theory applies to materials which exhibit plastic response immediately upon initiation of deformation.

Total loop closure in the first quadrant of the stress-strain space is still retained.

In this version of the theory the yield surface has shrunk to *a point.*

Though Rivlin is aware of these results, he does not choose to discuss them in his paper.

PRESUMED SHORTCOMINGS OF THE THEORY VIS-A-VIS CRITERIA WHICH ARE EITHER GENERALLY INVALID OR UNSUBSTANTIATED

The Rivlin inequality

In his Section 4 Rivlin considers the question of dissipation in cyclic deformation. Specifically he considers the case where a material undergoes increasing axial strain to a strain ϵ_1, whereupon the strain is decreased to ϵ_2 and then increased again to ϵ_1. He then asserts that the work done during this cycle—he calls it the dissipation D—must be either positive or zero.

This cycle is shown in Fig. 1. In terms of the notation of Fig. 1 the Rivlin inequality may be

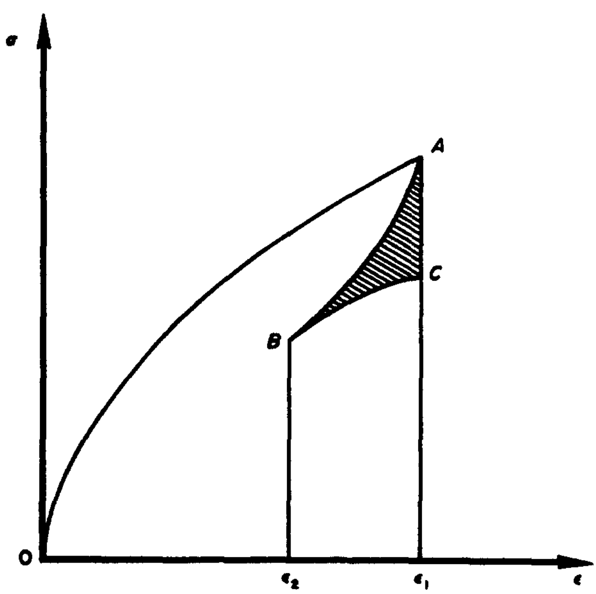

Fig. 1. Cycle of deformation.

stated as follows:

$$D = \int_A^B \sigma \mathrm{d}\epsilon + \int_B^C \sigma \mathrm{d}\epsilon \geq 0. \tag{24}$$

Rivlin contends that because the constitutive equation of the early theory[2] violates this inequality, then the theory must be deficient in some sense. Specifically he states that the endochronic model "must be unstable".

I shall show in the following that the Rivlin Inequality has no general validity *first* on the basis of physical grounds and second by demonstrating that *linear viscoelastic* materials as well as *frictional* materials *violate* the inequality.

The physical argument

When a material is strained to the point A it contains, while at A, free energy (stored, or potential energy) which is *recoverable* by an affine deformation back to the unstressed state. See Fig. 2.

Specifically, the amount of recoverable energy at A is equal to the area $B'A\epsilon_1$. The irrecoverable energy in state A is $0AB'$ and, of course, the total work done in reaching point A is equal to $0A\epsilon_1$.

When the material is partially unloaded to point B a partial amount of stored energy is *recovered*—I repeat *recovered* not *dissipated*. This amount is the area $\epsilon_2 BA\epsilon_1$. Note that while at B the material contains a residual amount of recoverable energy given by the area $B'B\epsilon_2$.

When the material is now reloaded to point C an amount of work is done which is equal to the area $\epsilon_2 BC\epsilon_1$. Note that *part* of this work is stored as free energy in the material, as I shall show below.

Rivlin adds the *work* $\epsilon_2 BC\epsilon_1$ to the *recovered* energy $\epsilon_2 BA\epsilon_1$ and calls the sum "dissipation"(?!). That one does that, shows a lack of awareness of the thermodynamic principles involved. There is no reason why the work $\epsilon_2 CB\epsilon_1$ should be greater than the recovered energy $\epsilon_2 BA\epsilon_1$.

To understand the principles involved it is instructive to regard the material as an "engine". When the material is at A, it contains a pool of recoverable energy the maximum value of which is the area $B'A\epsilon_1$. Any other path to the unstressed state will yield less energy.

The question is: "Can the material acting as an engine extract energy from the pool through a cycle ABC?" The answer is *yes, but at a cost*. This amount of extracted energy is equal to the energy released in going form A to B minus the work done in going from B to C. This is shown as the shaded area in Fig. 2.

The "cost" is the loss of recoverable energy of the pool, resulting from the fact the material is now in *state C*. The maximum recoverable energy at C is found by unloading from C to the

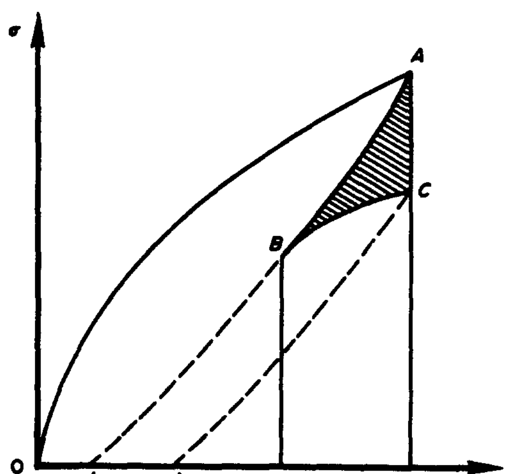

Fig. 2. Stored energy in cycle of deformation.

unstressed state. This amount is the area $C'C\epsilon_1$. So the *loss* in stored energy in the energy pool is the area $B'ACC'$. Hence the *efficiency* η of the engine is

$$\eta = \frac{ABC}{AB'C'C} \tag{25}$$

which is positive and considerably less than unity. Furthermore it follows that the *correct* dissipation $\mathcal{D}$ during the cycle is the *stored energy loss* minus the work done by the engine, i.e.

$$AB'C'C - ABC = \mathcal{D} > 0 \tag{26}$$

which is of course positive.

Rivlin in calculating his dissipation $\mathcal{D}$ completely omitted the *loss* in stored energy brought about by the fact that the material has changed *its state from A* to *C*.

In fact if one were to continue the cycles ad infinitum one would arrive at a situation depicted in Fig. 3 where the shaded areas represent the energy extracted by the engine. Of course their sum is much less than the stored energy at A, which is $AB'\epsilon_1$, and which is finally reduced to *zero*, when the stress becomes zero at the point ϵ_1.

DEMONSTRATION OF THE VIOLATION OF RIVLIN'S INEQUALITY BY LINEAR VISCOELASTIC MATERIALS

Remark 1

A linear Maxwell model *violates* the Rivlin inequality.

Let a linear Maxwell model, where

$$\sigma = E_0 \int_0^t e^{-\alpha(t-\tau)} \frac{\partial \epsilon}{\partial \tau} d\tau, \tag{27}$$

undergo the cycle ABC of Fig. 1 at *constant* absolute value of the *strain rate k*. It can then be shown that for an infinitesimally small cycle, where terms of order $(\epsilon_2 - \epsilon_1)^2$ are negligible, that

$$D = E_0 \frac{(\epsilon_1 - \epsilon_2)}{\alpha} k e^{-\alpha\epsilon_1/k}(1 - e^{-\alpha(\epsilon_1 - \epsilon_2)/k})(1 - 1\, e^{\alpha\epsilon_1/k}). \tag{28}$$

If $\epsilon_1 > 0$ and $\epsilon_1 > \epsilon_2$ then since k and α are both positive then D is negative, in violation of Rivlin's inequality.

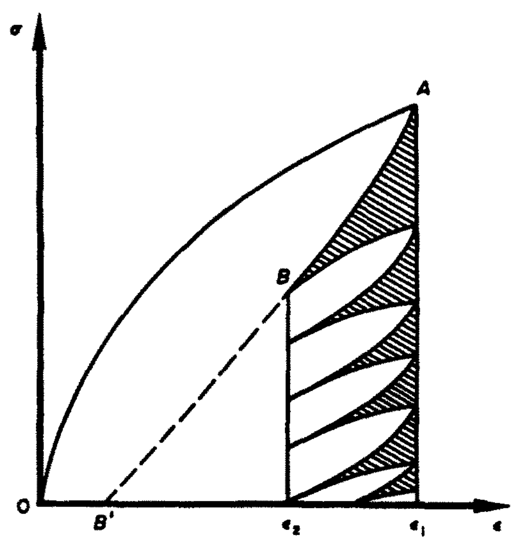

Fig. 3. Repetitive cycles.

Remark 2

All linear viscoelastic materials violate the Rivlin inequality under conditions of constant absolute value of the uniaxial strain rate. The proof is left to the reader.

DEMONSTRATION OF VIOLATION OF RIVLIN'S INEQUALITY BY A FRICTIONAL MODEL

In this section of the paper I shall demonstrate that there exist frictional models that also violate Rivlin's Inequality. The one below is a case in point.

Properties of the model

Shear block. Has unit thickness and is purely elastic. If it is rigidly held at the friction surface then the deformation u at A due to the force vector F is

$$u = \frac{F \cos \theta}{G} \tag{29}$$

where F is the absolute value of the force $\mathbf{F}$ (corresponding to a stress $\mathbf{F}$) and G is the shear modulus.

Resistive surface. The surface offers to the block a resistance (to motion) which is proportional to the normal stress. The resistive force R exerted by the surface is equal to μW where W is the normal force—tending to press the block against tne surface—and μ is the friction coefficient. The friction angle ϕ is defined by the relation

$$\tan \phi = \mu. \tag{30}$$

Spring. The spring BC is linear and has a stiffness k.

HISTORY OF DEFORMATION AND CONSTITUTIVE PROPERTIES OF THE MODEL

With reference to Fig. 4, initially $\Delta \mathbf{P} = 0$. A monotonically increasing force $\mathbf{F}$ is applied at $\theta = 0$. So long as $F \leq \mu W$ there is no slip and the displacement u at A is

$$u = \frac{F}{G}$$

when $F > \mu W$ slip will occur and the displacement at A will be given by equation

$$u = \frac{F}{G} + (F - \mu W)\frac{1}{k}. \tag{31}$$

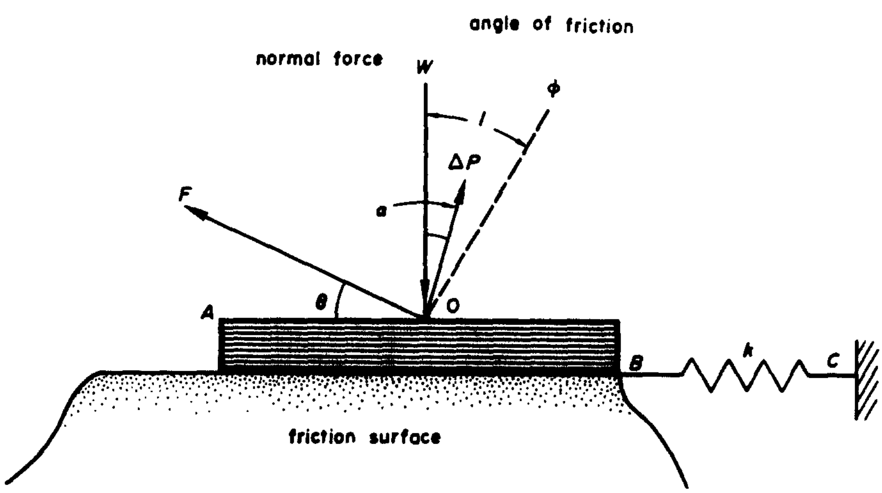

Fig. 4. Friction model.

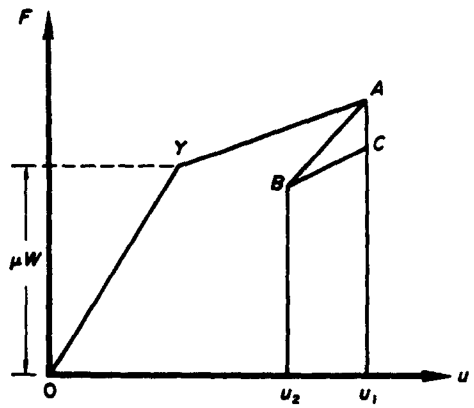

Fig. 5. Stress–strain history.

This process is terminated when $u = u_1$. The corresponding stress–strain history is shown in Fig. 5. In this figure I show that slip occurs at point Y and the monotonically increasing strain is terminated at A.

At this point an "external agency" applies a force $\Delta \mathbf{P}$ at an angle α to the vertical such that $\alpha < \phi$. We wish to examine the effect of this force on the displacement u. One can easily see that:

(i) The applied force is diminished by an amount $|\Delta F|$ where

$$\Delta F = -\Delta P \sin \alpha. \tag{32}$$

(ii) The resistance R between the block and the surface has also diminished by an amount $|\Delta R|$ where,

$$\Delta R = -\mu \Delta P \cos \alpha. \tag{33}$$

We note that $\Delta \mathbf{P}$ will cause further slip if the decrement in stress is smaller than the decrement in resistance, i.e. if

$$\Delta P \sin \alpha < \mu \Delta P \cos \alpha \tag{34}$$

or

$$\tan \alpha < \tan \phi, \tag{35}$$

i.e.

$$\alpha < \phi. \tag{36}$$

Since this is indeed the case by *choice* (see Fig. 4), further slip will indeed take place Thus, now,

$$u + \Delta u = \frac{F - \Delta P \sin \alpha}{G} + \frac{(F - \Delta P \sin \alpha - \mu w + \mu \Delta P \cos \alpha)}{k}$$

oe,

$$\Delta u = -\Delta P \frac{\sin \alpha}{G} + \frac{\Delta P \mu \cos \alpha - \sin \alpha}{k} \tag{37}$$

or, making use of eqn (30),

$$\Delta u^- = \Delta P^- \left(\frac{\sin(\phi - \alpha)}{k \cos \phi} - \frac{\sin \alpha}{G} \right) \tag{38}$$

where we called *this* change in displacement Δu^- and the corresponding *magnitude* of the force ΔP^-.

Let the external agency now reverse the direction of $\Delta \mathbf{P}$. We wish again to examine the effect on Δu. In this case the change ΔF in the resultant force is

$$\Delta F = \Delta P \sin \alpha \tag{39}$$

and the change ΔR in the resistance is

$$\Delta R = \Delta P \cos \alpha. \tag{40}$$

Evidently on the basis of the previous analysis in eqn (41) still holds:

$$\mu \cos \alpha > \sin \alpha. \tag{41}$$

It follows that $\Delta R > \Delta F$ and therefore no slip will take place; hence the increment in deformation is

$$\Delta u^+ = \frac{\Delta P^+ \sin \alpha}{G}$$

since only the elastic sheer of the spring will now contribute to u. We call this increment in displacement Δu^+ and the corresponding *magnitude* of the force ΔP^+.

Consider the specific case where

$$\phi = 2\alpha, \; k \cos \phi = (n+1)G \tag{42a, b}$$

where n is an integer greater than 2.
Then,

$$\Delta u^- = \Delta P^- \left(\frac{\sin \alpha}{(n+1)G} - \frac{\sin \alpha}{G} \right) = -\Delta P^- \frac{n}{(n+1)G} \sin \alpha. \tag{43}$$

It is worthy of note that the corresponding change ΔF^- in the resultant force is negative (see eqn 44)

$$\Delta F^- = -\Delta P \sin \alpha \tag{44}$$

so that a decrease in the horizontal resultant force brings about a decrease in displacement.

Thus

$$\left(\frac{\Delta F}{\Delta u} \right)^- = \frac{(n+1)}{n} \frac{G}{\sin \alpha}. \tag{45}$$

But it also follows from the foregoing that

$$\left(\frac{\Delta F}{\Delta u} \right)^+ = \frac{G}{\sin \alpha}. \tag{46}$$

The following inequality, therefore, holds.

$$\left(\frac{\Delta F}{\Delta u} \right)^- > \left(\frac{\Delta F}{\Delta u} \right)^+. \tag{47}$$

The resulting history of deformation is given in Fig. 5. Note that the cycle ABC violates the Rivlin inequality.

PERCEIVED PECULIARITIES OF THE THEORY IN TERMS OF MATHEMATICAL CONSTRAINTS THAT HAVE NO PHYSICAL FOUNDATION

Rivlin's continuity condition—Section 8

It will be shown presently that mechanical systems, with dissipation mechanisms of the frictional type, violate Rivlin's continuity condition. We address the problem in terms of the simple mechanical system shown in Fig. 6.

External forces $\sigma_i (i = 1, 2)$ are applied to the system at 0. The point F is a friction point in the sense that F will not move unless the resultant force at F exceeds a critical value R_0 which is the absolute value of the resistive force experienced by the point F.

The elastic spring $0F$ is hinged at F and aligns itself with the external force σ_i. This being the case the external force σ_i can be considered to be applied at F, as a simple free body diagram of the spring $0F$ will readily domonstrate. In so far as the response of the system in terms of the motion of the point F is concerned, the case under study is summarized in Fig. 7.

We assume that the resisitive action of the motion of the point F is isotropic in the sense that the resistive force $\bar{R}$ is always in the direction of motion q_i of the point F and always opposes the motion of F. While motion takes place

$$\|\vec{R}\| = R_0$$

where $\|\vec{R}\| = (R_iR_i)^{1/2}$ and R_0 is a constant.

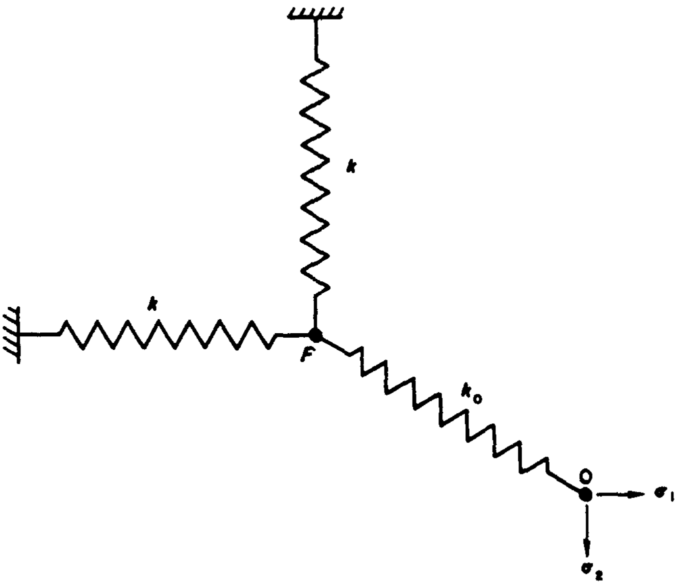

Fig. 6. Mechanical system of the simple friction type.

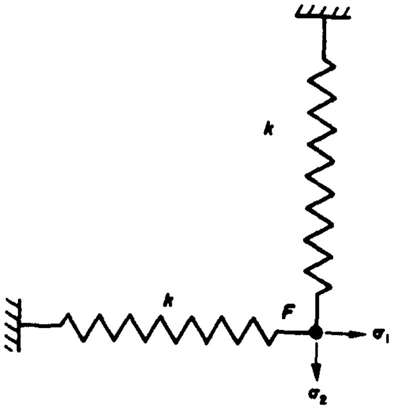

Fig. 7. Point F under the action of external force σ_i.

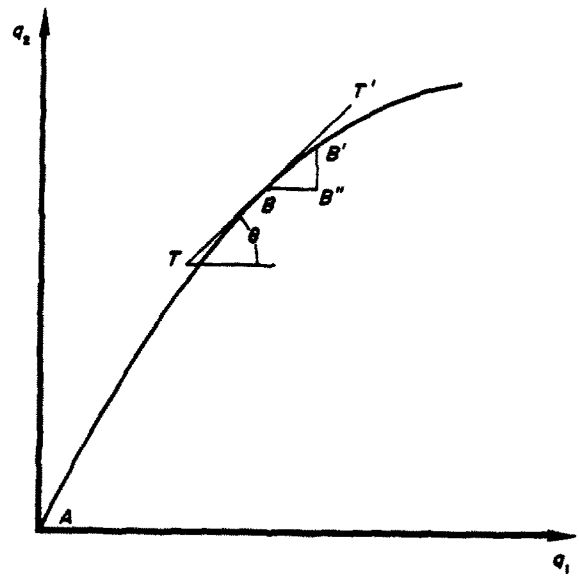

Fig. 8. History in plastic "strain" space q_i.

Let us presume that under a certain external force history the path of the point F in q-space is as shown in Fig. 8.

We wish to construct the equation for the constitutive response of the mechanical system in question. We do this in terms of a generic point B along the path in q-space as shown above. The resistive force at B is the direction of the tangent at B, i.e. $T'T$ since, by assumption, it is always in the direction of motion and it opposes the motion.

If l_i are the direction cosines at B, i.e.

$$l_i = \frac{dq_i}{d\zeta} \tag{48}$$

and $d\zeta$ is an element of length of the path where

$$d\zeta^2 = dq_i dq_i \tag{49}$$

then,

$$R_i = R_0 l_i. \tag{50}$$

The balance of forces at F, with reference to point B, is given by the following equation

$$kq_i + R_0 \frac{dq_i}{d\zeta} = \sigma_i. \tag{51}$$

This is of course the equation for the constitutive response of the system. Anyone who is familiar with the endochronic theory will recognize the above as an "endochronic constitutive equation", but more will be said about this later. Note, however, that if ϵ_i is the "overall strain" of the system, i.e. its displacement at 0, then it follows from Fig. 8 that

$$\epsilon_i = q_i + \frac{\sigma_i}{k_0} \tag{52}$$

or

$$q_i = \epsilon_i - \frac{\sigma_i}{k_0}, \tag{53}$$

i.e. q_i is the "plastic strain" of the system. In this event $d\zeta$ is the norm of the plastic strain, a definition introduced by Valanis in Ref. [9].

If θ is the angle that the tangent TT' makes with the axis q_1, as shown in Fig. 8, then as a result of eqn (51)

$$kq_1 + R_0 \cos \theta = \sigma_1 \tag{54}$$

$$kq_2 + R_0 \sin \theta = \sigma_2. \tag{55}$$

Rivlin's condition of continuity requires that a motion to B' should give rise to the same stresses at B', whether the path followed is BB' directly or $BB''B$ instead, in the limit of $|BB'| \to 0$. Or formally:

$$\mathrm{Lim}(|\sigma_i(BB') - \sigma_i(BB''B'))=0 \quad |BB'| \to 0. \tag{56}$$

It can be verified readily that the present mechanical system *violates* Rivlin's condition (51). For, along the path BB'

$$l_1 = \cos \theta,\ l_2 = \sin \theta \tag{57}$$

whereas along the $B''B'$, which is the latter part of the path $BB''B'$,

$$l_1 = 0,\ l_2 = 1.$$

Hence using eqn (51)

$$\sigma_1(BB') = kq_1(B') + R_0 \cos \theta \tag{58a}$$

$$\sigma_2(BB') = kq_2(B') + R_0 \sin \theta \tag{58b}$$

$$\sigma_1(BB''B') = kq_1(B') \tag{59a}$$

$$\sigma_2(BB''B') = kq_2(b') + r_0. \tag{59b}$$

It follows that

$$\mathrm{Lim}|(\sigma_1(BB') - \sigma_1(BB''B')| = R_0 \quad |BB'| \to 0 \tag{60}$$

$$\mathrm{Lim}|(\sigma_2(BB') - \sigma_2(BB''B'))| = R_0(1 - \sin \theta) \quad |BB'| \to 0 \tag{61}$$

in violation of Rivlin's continuity condition.

The reason for the above is, of course, the fact that the *tangent* at B' is different for the *two* paths. For a more general treatment of this model the reader is referred to Ref. [7].

RIVLIN'S COMMENTS IN APPENDIX A ON THE "RATE EQUATIONS" OF THE INTERNAL VARIABLES

In paragraph 1 of his Appendix A, Rivlin challenges the "validity" of my thermodynamic "considerations". He then proceeds to make this challenge explicit in paragraph 12 of Appendix A, immediately following his eqn (A19), by attacking the rate equations (eqn A19) as possessing a perceived "peculiarity".

I will proceed to show that Rivlin's preceived "peculiarity" of these equations, is not the fault of the equations but the result of his own erroneous perceptions of thermodynamics.

In the aforementioned paragraph Rivlin makes the statement that "...when internal variables are introduced along with the current strain, as independent variables in a constitutive equation, it is in order to provide a full description of the *current state* (my italics) in terms of the current values of the independent variables of the theory".

Of course the above statement is *true*. He then proceeds, however, to complete the paragraph with the following statement:

"If the internal variables and the strain provide a complete description of the state, then the infinitessimal change in the internal variables due to a specified infinitesimal change in the strain *should depend only on their current values*" (my italics).

The above statement is patently *false* and is due to Rivlin's misunderstanding of the meaning of thermodynamic equation of state—though this has been explained, ad nauseam, in the literature. However, let me repeat.

The thermodynamic state of a system (internal energy density, free energy density, stress) is defined by the strain tensor $\boldsymbol{\epsilon}$ the temperature θ, and n (where n is possibly infinite) internal variables $\mathbf{q}_r$. The latter are descriptors of the internal structural configuration of the system.

If ψ is the free energy density then the analytical expression of the above statement is, simply,

$$\psi = \psi(\boldsymbol{\epsilon}, \theta, \mathbf{q}_r). \tag{62}$$

Knowledge of ψ suffices to determine the internal energy density ε and the stress σ since

$$\boldsymbol{\sigma} = \frac{\partial \psi}{\partial \boldsymbol{\epsilon}} \tag{63}$$

$$\varepsilon = \psi + \theta \frac{\partial \psi}{\partial \theta}. \tag{64}$$

Consider the functional dependence of ψ on $\boldsymbol{\epsilon}$, θ and $\mathbf{q}_r$ as known. Then knowledge of the *current* values $\mathbf{e}$, θ and $\mathbf{q}_r$ determines ψ, i.e. determines thermodynamic state.

How the internal variables $\mathbf{q}_r$ are determined is, of course, important but totally irrelevant to the above issue. The rate equations, to which Rivlin objects on false grounds, merely determine $\mathbf{q}_r$ in terms of the history of strain (under isothermal conditions).

The fact that q_r are functionals of the strain history in no way negates the fact that ψ depends only on the CURRENT values of $\boldsymbol{\epsilon}$, q_r and θ, and is therefore a STATE FUNCTION.

DIFFERENCES IN STYLE AND GENERAL PHILOSOPHY

I shall address this issue in terms of the field of constitutive equations. The field of mechanics in its broadest context is forever torn between generality and specificity. The field of constitutive equations is not exempt. I believe the crux of the matter lies in the fact that materials are specific. Metals are specific and so are polymers, ceramics and soils. Each one, being such, obeys necessarily a specific law encapsuled in a specific constitutive equation.

Often one witnesses ambitious attempts to construct a "master" constitutive law for *all* materials. Such grand designs envision specific laws as derivatives of the general one, obtained by judicious choice of functions and/or parameters. I believe that such attempts are futile because they lie outside the realm of human intellectual capability.

Progress in the sciences has been made through faltering steps, "accidents", humble attempts at a specific task, *not* through grandiose experiments designed to solve all the riddles of the universe with one fell swoop.

Historically, men and women have formulated laws on the basis of the *available* experimental evidence.

Further experiments are then conducted to test such laws outside their established realm of validity. The results of such experiments then serve to investigate further the domain of validity of such laws and, where appropriate, to construct others which are more encompassing.

Einstein formulated his Theory of Relativity in terms of Riemannian geometry. I, living by example, and guided by a need for simplicity, used Riemannian geometry to define the intrinsic time ζ. It is obviously not the "most general" definition. But its domain of validity must be explored experimentally before one embarks on general and, perhaps, more complex possibilities.

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