FOCUSED REVIEW OF:

"Conceptual Considerations of the Yucca Mountain Groundwater System with Special Emphasis on the Adequacy of this System to Accommodate a High-Level Nuclear Waste Repository", by Jerry S. Szymanski, U. S. Dept. of Energy, Las Vegas, Nevada, July 26, 1989

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0. FOREWORD:

This letter report outlines the results of our focused review of the report on *"Conceptual Considerations of the Yucca Mountain Groundwater System with Special Emphasis on the Adequacy of this System to Accommodate a High-Level Nuclear Waste Repository"*, by Jerry S. Szymanski (second version, July 1989). This work is Activity 2, Subtask 1.2 of the Geologic Setting Program Element as defined in the Operations Plan of the Center for Nuclear Waste Regulatory Analyses (CNWRA). The present letter report is an intermediate milestone for that activity, under supervision of the Center's Geologic Setting Element Manager, John L. Russell. In addition to David Brooks, manager for NRC's Geologic Setting Program Element to whom this letter report is submitted, individuals contacted were Rex Wescott (NRC Technical Contact for the work), Don Chery (NRC), and Simon Hsiung (Center). Rex Wescott provided technical information and assisted in integrating the work. Simon Hsiung provided technical contributions on several aspects of rock mechanics. Other individuals, covering several disciplines at the Center, have contributed indirectly through informal technical discussions. The author of this letter report is a hydrologist with degrees in Environmental Hydraulics, Fluid Mechanics, and Civil Engineering.

1. INTRODUCTION:

The review presented here focuses on mathematical formulation of conceptual models and quantitative deductions from physical principles. Parts of the report, text and plates, have been reviewed in some detail, including the following sections and sub-sections:

- Section 1.0: INTRODUCTION [all]
- Section 2.0: MATHEMATICAL AND CONCEPTUAL MODELS OF GROUNDWATER SYSTEMS - STATEMENT OF THE PROBLEM [all]
- Section 3.0: CONCEPTUAL FRAMEWORK FOR THE YUCCA MOUNTAIN GROUNDWATER SYSTEM [especially sub-sections 3.1, 3.2, 3.3]
- Section 5.0: SUMMARY, CONCLUSIONS, AND RECOMMENDATIONS [all]

Other parts have not been included for systematic review. In particular, the whole Section 4.0 on *"Characteristics of the Death Valley Groundwater System in Light of Existing Data"*, being focused on field observations rather than modeling, has not been reviewed in detail. Finally, auxiliary material such as Plates and References were, of course, also reviewed along with the main text.

We have found it convenient to organize the remainder of this review as follows. Section 2 provides general comments and an overall view of Szymanski's report (substance, contents, quality). Section 3 analyzes in more detail, section by section, those parts of the reviewed report which have seemed to us to be either flawed, inconsistent, or scientifically uncertain. Section 4 summarizes our evaluation of Szymanski's report and briefly discusses alternative approaches. Section 5 provides a list of references of authors quoted in the present letter report. Our list is only partially redundant with the list of references provided in Szymanski's report.

We have found it necessary to include in this review a (limited) amount of scientific material such as equations and figures in order to provide a sound background for some of the analyses and critiques developed in the technical Section 3. In this report, page numbers, section numbers, and plate numbers from the above-quoted Szymanski's report will be enclosed in square brackets, such as [Sect. 3.34, p.3-38 and Plate 3.3.4-1] for example. All direct quotations from *J. S. Szymanski (1989)* will be clearly indicated using *"italics enclosed in double quotes"*. Double-quote signs without italics will be used by us in the standard way to indicate possible ambiguity or simply to emphasize wording. Equation numbers displayed in parenthesis will indicate equations written in this review, not those from the report being reviewed (which does not have equation numbers). Finally, we will use the acronym JSS to refer to the report being reviewed and/or its author.

2. OVERVIEW AND GENERAL COMMENTS:

This overview section seeks to briefly characterize the substance and scientific quality of the report with regard to mathematical formulations, conceptual models, physical principles, quantitative reasonings and deductions, as well as more general considerations of technical style, clarity of presentation, etc. Detailed justifications of the technical questions raised here are postponed until Section 3.

The JSS report is a heterogeneous assemblage of common-sense facts, specialized empirical relations, basic equations, hypothetical scenarios, field observations, textbook-style material, and recent research findings, borrowed from a vast and eclectic literature ranging from classical works [e.g. Rayleigh (1916); Biot (1941)] to recent unpublished research [e.g. Harper and Last, 1987]. The reference list includes a half dozen textbooks, many research articles including at least two dozens on hydro-thermal and hydro-mechanical processes, a large number of references on the geophysics of the earth mantle and crust, magmatic processes, and tectonophysics, and an even larger number of technical reports describing studies of the Yucca Mountain area or reporting experimental results. A large number of *"Plates"* - on the order of 150 or 200 - is appended to the text.

The main scientific objectives of the JSS report are to study the *"linkage between the behavior of a groundwater system and tectonically generated energy and/or substance in various forms and quantities"* [p.1.1], and addressing the problem by using *"thermodynamical concepts of coupled processes"* rather than more *"traditional hydrogeology"* [p.1-2]. There is a particular emphasis in [Sect. 1] and [Sect. 2] on the fact that *"simple"* groundwater models are of questionable validity for an active tectonic environment like that of Yucca Mountain.

One of the main theses of the report is the existence of tectonic cycles characterized by slow extensional deformation (dilatation of fractures and faults starting from the surface and progressing downwards), and punctuated by seismic events inducing fast closing of the previously dilating fractures and/or faults, and resulting in upwards expulsion of water. The water table rise is enhanced or sustained for larger times due to thermal convection. This process is called *"seismic pumping"* in the JSS report.

The scenario known as "seismic pumping" is not new. The seismic pumping mechanism has been described by *Sibson et al. (1975)* as an explanation for the textures of hydrothermal vein deposits associated with ancient faults. Their model uses the dilatancy/fluid diffusion model of earthquake formation developed by *Scholz et al. (1973)*. More recently, *McCaig (1988)* attempted to use the seismic pumping mechanism to explain massive fluid circulations in ductile shear zones in the Pyrenees. All three works are mentioned in the JSS report, and a figure from *Sibson et al. (1975)* is used to explain the mechanism of seismic pumping. In fact, the first 40 pages of [Section 3] entitled *"Conceptual Framework for the Yucca Mountain Groundwater System"* are almost entirely devoted to rationalizing the stress-deformation processes that may lead to rises of the water table. There is a great deal of use of hypothetical Mohr circle constructions in [Section 3], and many field measurements in [Section 4.2.5] on "*The in situ stress field*" are also reported in the form of Mohr circles.

The overall impression from the report is that of an eclectic, fragmented mass containing many schematic ideas, intentions, and hypothetical scenarios, seldom leading to a fully developed analysis of the coupled processes of interest. We have found the report to be lacking in clarity and concision [see the title of the report for a representative sample], the technical quality of the plates to be below average, and the internal organization of the report too fragmented and somewhat awkward. There is an exaggerated profusion of plates, many of which appear to be mutually redundant, such as the six plates devoted to groundwater flow equations in [Sect. 2]. Others describe repetitive raw data: about 60 plates of Mohr circle diagrams for the in-situ stresses of [Sects. 4.2.5.2, 4.2.5.3, 4.2.5.4], and about 70 plates for the in-situ injection test curves of [Sect. 4.2.5.4].

As will appear more clearly below, we have found a number of technical inconsistencies, incorrect equations, and conceptual inaccuracies, in addition to a frequent lack of rigor in the use of scientific terms and mathematical symbols. These compounded factors result in vagueness, and tend to dilute the author's arguments and conclusions. The general effect is that the partial results and conclusions summarized at the end of each sub-section of the report are difficult to verify or back-up by independent calculations.

A particularly troublesome aspect of the report is the discrepancy between the claim of formulating and using "*mathematical*" and "*conceptual*" models (see title of report and titles of Sections 2.0, 2.2, 2.3, 3.0, 3.3, 3.3.4, 3.4, 3.4.4, and 3.5) and the actual assemblage of empirical models being used. Apart from directly traceable errors and inaccuracies, we have also found in many instances that the models actually used to rationalize hypothetical scenarios were so simplified that they failed to take into account basic physical principles (mass balance, balance of stresses, forces, and momentum, dynamic/inertial effects, dimensionality) and did not represent truly coupled processes with reasonable feed back effects. The need to address coupled hydro-thermo-mechanical processes in a more adequate fashion will be discussed again in the conclusion section.

We will now illustrate these general remarks by analyzing in more detail parts of the material presented in the JSS report.

3. DETAILED TECHNICAL REVIEW:

As explained previously, we cover here only selected portions of the JSS report, excluding in particular the entire [Section 4] as well as several other sub-sections elsewhere. Nevertheless, we will follow the ordering of the JSS report, using [square brackets] to indicate the corresponding sections, pages, and plates, being analyzed, and using *italics* and "*angular brackets*" for quotations from the JSS report. We emphasized that, in order to be completely understood, this detailed review section will be best read with a copy of the JSS report at hand. Nevertheless, we have attempted to develop self-contained discussions whenever possible.

[Section 1.0]: "*Introduction*":

- This short section emphasizes the importance of "*linkage between the behavior of a groundwater system and tectonically generated energy*", and insists that "*difficult mathematical formulations*" are required to study such coupled processes [p.1-2].

- In particular, it is explained that the difficult mathematical formulations "*relate more to thermodynamic concepts of coupled systems than to traditional hydrogeology*", and that "*the majority of assumptions utilized to develop mathematical models to describe 'simple' groundwater systems are of questionable validity*" [p.1-2].

- The opinion of this reviewer is that, although the above-quoted statements appear reasonable, they also indicate a tendency to play down modern advances in the modeling of coupled processes in hydrogeology and geophysics. In order to give a feel for the state-of-the-art and for future comparison purposes, we provide an Appendix on the "Quantitative Modeling of Coupled Hydro-Thermo-Mechanical (HTM) Processes", an admittedly sparse and limited review of the relevant literature in this area.

[Section 2.0]: "*Mathematical and Conceptual Models of Groundwater Systems: Statement of the Problem*"

[Sub-Section 2.1]: "*General*":

- The following sentence is representative of the style and tone of this section: "*so-called professional judgment involving long-term behavior of groundwater systems, including their responses to tectonic stimulations, is developed based on mathematical models of 'simple' groundwater systems*" [p.2-1].

- The affirmed goal of [Section 2] is to understand the limitations of models of "*simple*" groundwater systems. More on this below.

[Sub-Section 2.2]: *"Mathematical Models of Natural Groundwater Systems"*

- There are six plates describing groundwater flow equations. They could easily be replaced by a single plate. Only one plate [Plate 2.2-6] shows the classical groundwater flow equations in a sufficiently general form (storage term, recharge term, spatial heterogeneity and anisotropy of hydraulic coefficients).

- Furthermore, a serious confusion is made in three of these plates [Plates 2.2-3, 2.2-4, 2.2-6]. Equations expressed in terms of hydraulic transmissivity T and storage coefficient S are mistakenly written as three-dimensional equations using partial derivatives in x, y, and z. This is inconsistent. The storage coefficient S is correctly defined *"per unit area"* in the text [p.2-3] where the following equation appears:

$$\nabla^2 h = S T^{-1} \frac{\partial h}{\partial t} \tag{1}$$

  The inconsistency stems from the fact that the Laplacian operators (∇^2) shown on the above-mentioned plate are expressed explicitly as three-dimensional rather than two-dimensional operators. For textbook references, see *Freeze and Cherry (1979, Sect. 2.10 and 2.11) and DeMarsily (1986, Sect. 4.1.f and 5.1)*.

- The conclusions of this sub-section appear reasonable but not very informative (e.g.: the use of inappropriate mathematical models can lead to gross misrepresentations, etc.).

[Sub-Section 2.3]: *"Conceptual Models of Natural Groundwater Systems"*

- Again, this sub-section is essentially devoid of substance except for very general considerations on what is required for developing adequate "conceptual" models. In the opinion of this reviewer, [Sect. 1] and [Sect. 2] taken together constitute the truly introductory material of the report, with about 14 plates for 10 pages of text.

[Section 3.0]: *"Conceptual Framework for the Yucca Mountain Groundwater System"*

[Sub-Section 3.1]: *"Regional Tectonic Setting of the Yucca Mountain Groundwater System"*:

- A map given in [Plate 3.1-1] locates Yucca Mountain within the Southern Great Basin, in the west-central part of the Death Valley groundwater system. The Great Basin is thought to be subject to on-going crustal extension; this is debated by JSS, and references are given.

- Based on his geophysics literature review, JSS estimates upper mantle "*viscosity*" (more precisely, the dynamic viscosity μ):

$$\mu \sim 10^{20} - 10^{22} \ \textit{Poise} \tag{2}$$

where 1 Poise = 0.1 Ns/m^2. JSS then utilizes the Rayleigh number, which is given (correctly) on top of [p.3-3] as:

$$Ra = \frac{\alpha \rho g d^3 \Delta T}{\kappa \mu} \tag{3}$$

Based on the critical Rayleigh number (Ra)crit ≈ 1700, he concludes that thermal convection will be triggered if the temperature difference ΔT between the top and bottom of a mantle layer of thickness d=100m and viscosity $\mu = 10^{21}$ Poise exceeds 200°C, a condition that may easily occur given usual thermal gradients in the upper mantle.

- The above arguments seem correct a priori, although perhaps oversimplified. Note that the theoretical critical Rayleigh number is given exactly by:

$$(Ra)_{crit} = 1708 \tag{4}$$

This critical value marks the onset of thermal instability rather than fully turbulent thermal convection; the latter may only be attained at $Ra \geq 10^5 \cdot 10^7$, after *Landahl and Mollo-Christensen (1986)*. Note also that the Rayleigh number can be represented as a combination of time scales:

$$Ra = \frac{t_v t_h}{(t_c)^2} \tag{5}$$

where the t's represent the time scales of *viscous* fluid diffusion ($t_v = \rho d^2/\mu$), *heat* diffusion ($t_h = d^2/\kappa$), and natural *convection* ($t_c^2 = d/\alpha g \Delta T$). The Rayleigh number quantifies how fast local density differences due to heating overcome the stabilizing effects of viscous and heat diffusion processes. The latter stabilizing processes are still present in the convective regime Ra > (Ra)crit; there may be very little convection if Ra remains on the same order of magnitude as (Ra)crit.

- The *"Jeffrey problem"* mentioned on [p.3-4] probably refers to a pioneering study of instability via the Boussinesq equation, by *Jeffreys (1926)*. This should be referenced or explained (we provide the missing reference in the attached list of references). Similarly, the *"familiar power creep equation"*:

$$\dot{\epsilon} = A \exp\left(-\frac{Q}{RT}\right) \sigma^n \tag{6}$$

should be discussed more clearly or referenced [p.3-4]. It seems that alternate creep mechanisms are also proposed on [p.3-4]?

- The conclusion that *"the effective viscosity may depend on the rate of heat transfer and may fluctuate with it"* [p.3-5, top] seems reasonable for a magmatic process, although not very informative as formulated here.

- The only definite conclusion from this sub-section is that "(...) *there is a firm base to believe that there may be a fundamental cause-effect relationship between the upper mantle dynamics and tectonic activities at and near the Earth's surface; such as earthquake fault movement, and volcanism."* [p.3-6].

[Sub-Section 3.2]: *"General Description of Extension Dominated Tectonic Environments and their Relationship to Groundwater Systems"*:

- This sub-section purports to study (or simply discuss) the convective upwelling of material from the upper mantle (asthenosphere) up into the overlying crust (continental lithosphere). It is emphasized that the present tectonic environment of the southern Great Basin is *"dominated by extension and alkalic-tholeiitic volcanism"*. Numerical simulations taken from the tectonophysics literature are used to illustrate mantle upwelling and its effect on stress and deformation of the lithosphere [Plates 3.2.-1 through 3.2-6].

- The indication that groundwater flow occurs *"in a cyclically deforming fractured medium"* [p.3-11, top] is vague and premature perhaps in anticipation of subsequent sub-sections (more on this later). The *"shear stress gradient $\tau_{zx(x)}$"* mentioned on [p.3-11] becomes a *"shear stress"* in [Plate 3.2-7]. This seems inconsistent.

[Sub-Section 3.3]: *"Conceptual Considerations of Groundwater Systems Developed in a Deforming Fractured Medium"*

[Sub-Section 3.3.1]: *"Contemporary Tectonic Environment of the Yucca Mountain Groundwater System"*:

- Contemporary deformation rates in the vicinity of Yucca Mountain are analyzed without discussing hydrologic implications. Note in particular the Northwest extensional strain rate (or strain variation) as measured near Yucca Mountain. This is shown on [Plate 3.3.1-6], borrowed from the literature. We do not attempt here to verify or otherwise evaluate the quality of field data such as the strain rates presented in the above-mentioned plate.

[Sub-Section 3.3.2]: *"The Changing In Situ Stress Field"*

[Sub-Section 3.3.2.1]: *"General"*:

- To clarify [Plate 3.3.2.1-1] and the sequel, it would have helped to recognize more explicitly the tensorial character of stresses and deformations. From Continuum Mechanics textbooks (*Germain and Muller 1986, or Popov 1968*):

$$\textit{STRAIN RATE:}\quad \dot{\epsilon}_{xy} = \frac{1}{2}\left(\frac{\partial U_x}{\partial y} + \frac{\partial U_y}{\partial x}\right)$$

(7)

$$\textit{ROTATION RATE:}\quad \Omega_{xy} = \frac{1}{2}\left(\frac{\partial U_x}{\partial y} - \frac{\partial U_y}{\partial x}\right)$$

Also, this reviewer does not clearly see the reason for presenting the numerical value of the *integrated* shear strain rate. The uniaxial strain rate ($\dot{\epsilon}_{xx}$) is given in the more usual instantaneous form.

[Sub-Section 3.3.2.2]: *"The In Situ Stress Field During a Single Cycle of Tectonic Deformation"*:

- There are several problems with this sub-section; we have found it difficult to comprehend. The key argument revolves around the dilatancy/fluid diffusion model of earthquake nucleation (*Scholz, 1973*) and the seismic pumping scenario developed by *Sibson et al., (1975)*. First, we note that Scholz's model is mentioned [p.3-19, top] but not explained in any explicit way (what comes subsequently is perhaps the author's interpretation of that model?). Second, considerations of structural failure, *"limit equilibrium"*, and so-called *"isotropic"* and *"singular"* points are introduced without much justification, again in anticipation of a later section [Sect. 3.3.3.2] on *"The In Situ Stress Field around a Deforming Fracture"*. Third, the critical mechanism of seismic pumping is explained on [p.3-20, bottom and p.3-21, top] without referring to the main source, *Sibson et al. (1975)*, from which [Plate 3.3.2.2-4] is borrowed. The reference is given in the plate but not in the text; it should have been mentioned from the outset. A more focused 1-2 page summary of seismic pumping in the manner of *Sibson et al. (1975, Section 2)* would perhaps advantageously replace most of the so-called *"conceptualization"* of [Sect. 3.3.2]. More details follow.

- Some of the "equations" shown on [Plate 3.3.2.2-2] are barely comprehensible, e.g. on the left column:

$$\frac{\partial(\sigma_{xx}, \tau_{yx})}{\partial t} \neq 0 \qquad (8)$$

Comparing this "equation" with the discussion on [p.3-19], it appears that a confusion is made between linear time-dependence and slow change. We submit that a variable can undergo slow change in a nonlinear manner, as well as fast change in a linear manner. Whatever the author meant, it is poorly conveyed by "equations" like the one above.

- [top p.3-20]: Admitting for the sake of the argument the existence of decreasing extensional stress from the ground surface downwards, it is not clearly explained whether (and why) the stress field is in "limit equilibrium" in the whole three-dimensional domain above the surface $z = Z(x,y,t)$. It is clear that the condition of "limit equilibrium" is attained in the vicinity of the vertical fault. But is the whole upper region multiply-fractured? Is there not some kind of localization of rock failure?

- [Bottom p.3-20]: In the discussion about the progress of deformation and downward migration of the above-mentioned surface $z = Z(x,y,t)$, there is a vague statement that the corresponding restructuring of the stress field involves: *"(a) spatial migration of 'singular' and 'isotropic' points, and (b) increasing density of occurrence of such points."*. The reviewer believes that arguments like these need to be clarified by involving not only constitutive relations (Mohr circle style) but also basic conservation equations, in particular the conservation of momentum. Dynamic stress redistribution cannot be "modeled" by assertions like the above-quoted phrase in the complex situation described.

- [p.2-21,top]: It is not all clear why and how the *"focus of the subsequent characteristic earthquake"* is situated at a depth *"substantially greater than depth of the surface Z (x,y,t), say at a depth ranging from 10 to 15 km"*, whereas, it appears from the second paragraph of [p.3-20] that not much action is taking place below the surface $Z(x,y,t)$.

- The arguments in the third paragraph of [p.3-22] may be criticized on the basis that, if the blocks model of [Plate 3.3.2.2-5] is true, the blocks in the area of extension must be in low, near-zero stress conditions and not at "limit equilibrium" conditions. The same type of question comes to mind regarding the hypothetical separation of fractures up to apertures of one meter wall-to-wall, as described on [p.3-23]. In fact, on [p.3-24], JSS anticipates that *"At this point in the analyses, (...) some readers may have considerable difficulty in intuitively accepting both the existence and the postulated mechanism of formation of the large-scale separation of wallrocks.*, and urges the reader to consult geological evidence from the Yucca Mountain area [pp.3-24/25]. To be useful, however, the concepts and models developed in the JSS report ought to have internal consistency.

[Sub-Section 3.3.2.3]: *"The In Situ Stress Field in a Cyclically Deforming Fractured Medium"*

- This sub-section is based partly on the previous one regarding "concepts", and partly on field observations (faults, calcite-silica veins, etc.).

[Sub-Section 3.3.2.4]: *"Summary and Conclusions"*

- The most striking feature in this summary and conclusions section is the absence of a summary of the specific conceptual model for the proposed mechanisms. Hence, we learn that the in situ stress field is *"variable in time and space"*, that the "limit equilibrium" conditions are *"occasionally present"*, that "singular" and "isotropic" points are *"present"*, and so on. The conclusion that *"flow occurs in a deforming fractured medium"* is equally vague, since what is important is the magnitude, rate, spatial distribution, localization, and the mechanisms of such deformation. Concerning rock failure criteria and related concepts, see below.

[Section 3.3]: *"Conceptual Considerations of Groundwater Systems Developed in a Deforming Fractured Medium"*

[Sub-Section 3.3.3]: *"Hydrologic Importance of a Changing In Situ Stress Field"*

[Sub-Section 3.3.3.2]: *"The In Situ Stress Field Around a Deforming Fracture"*

- This section attempts to use the Griffith-Coulomb theory of failure for analyzing the progress of deformation and failure in a fractured medium.

- Elements developed here were used more or less explicitly in the previous section on seismic pumping, and are being exploited in the next sections on the hydraulic conductivity structure of a deforming fractured medium. In the section on seismic pumping reviewed earlier, JSS discussed failure subsequent to a single cycle of tectonic deformation using the Coulomb failure criterion *"Coulomb-Navier"*) [see p. 3-20 of Sect. 3.3.2.2]. However, in the present section, JSS develops and advocates failure criteria based on the Griffith-Coulomb theory (*"Griffith/Navier-Coulomb"*). The latter theory is distinct from the former except a certain range of stresses. Moreover, the basic description of the Griffith-Coulomb theory of failure as developed by JSS is not entirely correct and appears confused in a number of details, some of which might be important for understanding the scenarios described in various parts of the report. Due to the fragmented structure of the report, it is difficult to evaluate the consequences of any such errors and misconceptions.

- In the first place, JSS does not clearly justify his use of the *"combined Griffith-Navier-Coulomb envelope of failure"* in the context of tectonics. There is no discussion of the scope and possible limitations of the failure theory, and no references are given. It should be noted that the Griffith theory of failure is based on a model of micro-crack growth in brittle and semi-brittle materials. The overall consequence of micro-crack growth is a failure envelope that is analogous to Coulomb's, but nonlinear, particularly in the region of tensile normal stresses. A decent, succinct summary of the combined Griffith-Coulomnb failure envelope is given by *Shaw (1980)* in a collection of works on magmatic processes (*Hargraves, 1980*). References to Griffith's original work and others are to be found there. The attached Mohr diagrams and enlarged caption from *Shaw (1980, Figure 7)* neatly summarize the Griffith and Griffith-Coulomb criteria; see also the attached text from *Shaw (1980, pp. 217-221)*. Shaw's figures and succinct descriptions of the Mohr circle diagrams and failure envelopes, are much more informative and accurate than those in the JSS report. It is instructive to compare, for instance, the attached [Plate 3.3.3.2-1] from the JSS report, to the attached Figure 7 (diagrams and captions) from Shaw (1980). Using Shaw's diagrams and classical results from continuum mechanics, let us develop a correct interpretation of the Mohr circle and Griffith-Coulomb failure envelope for comparison with the JSS report interpretation of these diagrams:

- (1): The Mohr circles represent the set of all stresses (τ,σ), i.e. shear stress and normal stress, that can be realized given the principal normal stresses σ_1 and σ_3. Rigorously, this interpretation is only valid if $\sigma_2=\sigma_3$; however, the correct failure stresses will be obtained even if $\sigma_2\neq\sigma_3$, provided only that $\sigma_1\geq\sigma_2\geq\sigma_3$ (see textbooks by *Germain and Muller 1986, Popov 1968, and others; in this text we use the convention that $\sigma>0$ in compression*). Accordingly, only the largest (σ_1) and smallest (σ_3) principal stresses are used on the Mohr circle diagram. The equation of the Mohr circle gives directly the shear stress as a function of normal stress:

$$\tau^2 = (\sigma_1-\sigma)(\sigma-\sigma_3) \qquad (9)$$

and it follows immediately that the maximum possible shear stress is:

$$\tau_{max} = \frac{\sigma_1-\sigma_3}{2} \quad \textbf{\textit{(maximum shear stress)}} \qquad (10)$$

and the corresponding normal stress is:

$$\sigma(\tau_{max}) = \frac{\sigma_1+\sigma_3}{2} \quad \textbf{\textit{(mean principal stress)}} \qquad (11)$$

Let (x_1,x_3) be the principle axes corresponding to (σ_1,σ_3) principal stresses. The state of stresses corresponding to τ_{max} occurs in a "plane of maximum shear stress" inclined at 45 degrees with respect to axis x_3. More generally, the plane of stresses (τ,σ) is at an angle α with the axis x_3, given by:

$$\tan\alpha = \sqrt{\frac{\sigma_1 - \sigma}{\sigma - \sigma_3}} \tag{12}$$

From now on, define α as the angle of the failure plane. The angle α is given graphically, as indicated in the centerpiece of the attached *Figure 7* from *Shaw (1980)*.

- (2): Both the Coulomb and the Griffith theories of failure assume failure occurs at a given point on the Mohr circle. As σ_1 and σ_3 vary, the curve described by these points forms the failure envelope, $\tau = f(\sigma)$. In the Griffith model, that envelope is a parabola (see top part of *Shaw's Figure 7*):

$$\tau^2 - 4K\sigma - 4K^2 = 0 \tag{13}$$

where K is the "tensile strength", by definition equal to the value of the normal stress at failure under conditions of zero shear stress. (*Shaw's Figure 7: A,B, and C*). In order to understand the consequences of both the Griffith's failure model and the composite Griffith-Coulomb model, it is necessary to clearly label the (τ,σ) axes in terms of the tensile strength K (*Shaw's Figure 7: A,B, and C*). In the attached [*Plate 3.3.3.2-1*] from the JSS report, the tensile strength is denoted T; it can be seen that the positive σ-axis is not labeled in terms of T, and furthermore, the τ-axis is improperly labeled ($\tau = 2T$ should be the intercept at point B). In addition, although this is difficult to tell from the poorly labeled plate, it seems that the negative σ-axis is also improperly label ($\sigma = -T$ should be the normal stress value at the point labeled A, not at the point labeled σ_3).

- (3): The consequences of the Griffith-Coulomb model regarding the orientation of failure planes is succinctly described in the attached *caption of Figure 7* from *Shaw (1980)*. One may also use equation (12) for obtaining tan α, where α is the angle of the failure plane with the x_3-principal axis. Note in particular that purely extensional failures correspond to Mohr circles tangent to the failure envelope at point A. This mode of failure is correctly described by the following sentence in the JSS report: *"Only if the stress circle touches the envelope at point A will pure tensile failure result"*. In fact, it can also be said that this mode of failure occurs on planes with $\alpha = 90$ degrees, and that the largest compressive principal stress for which this can occur is $\sigma_1 = 3K$, while the smallest principal stress reaches tensile strength: $\sigma_3 = -K$.

- (4): For Mohr circles tangent to the failure envelope between points A and B, failure occurs at angles α decreasing from 90 degrees at A to 67.5 degrees at B (see Shaw's *Figure 7*). This corresponds to a mixed shear-extensional mode of failure, since the stresses in the failure plane satisfy $\tau \neq 0$ and $\sigma<0$ (indicating extension). No quantitative insights are provided in the JSS report regarding the possible orientation of failure planes.

- (5): For Mohr circles tangent to points between B and C of *Shaw's Figure 7*, there is a transition between dominantly tensile and dominantly compressive shear modes of failure (see caption of Fig. 7 from *Shaw, 1980*). To the right of point C of *Shaw's Figure 7* starts the regime of compressive shear failure, and the angle α becomes less than 60 degrees. In comparison, note that JSS places a point C at an arbitrary location to the right of B [*Plate 3.3.3.2-1, attached*], and states without correctly explaining the compressive shear mode of failure: *"If the stress circle touches the failure envelope between B and C, a shear failure result"* [p. 3-31]. This is a poorly worded and partially incorrect description of the shear modes of failure to the right of point B.

- (6): On [p.3-31], JSS involves the *"principle of effective stress"* and concludes that *"failure of a fractured medium may be introduced by increasing either the stress difference (σ_1-σ_3) or fluid pressure (P)"*. We agree with the last but not with the first part of the alternative. Assuming (P) fixed, increasing the difference (σ_1-σ_3) will not necessarily induce Griffith-Coulomb failure if, for instance, σ_1 is increased at the same time: just displace the Mohr circle to the right as its diameter is increased. The only case where failure automatically results from increasing (σ_1-σ_3) is if the failure model is assumed to be based on "maximum shear stress" rather than on the Griffith-Coulomb envelope. In addition, we note that JSS's treatment of effective stress is not quite complete; the important notion that pressure is an isotropic tensor (δ_{ij}), unlike the total stress tensor (σ_{ij}), is not even mentioned. And, the total principal stresses (σ_i) in the Griffith-Coulomb/Mohr circle diagrams should be replaced by effective principal stresses, using a distinct notation such as:

$$\overline{\sigma}_i = \sigma_i^{Eff} = \sigma_i - p \qquad (14)$$

This distinction is all the more important since the coupling of tectonics and groundwater flow is considered a key aspect of this work. Assuming the validity of the concept of effective stress, the equation above is necessary, although far from sufficient to adequately represent such coupled processes. Finally, questioning the validity of the concept of effective stress itself should be seriously considered in the case of transient flow with widely open fractures or faults (developing an alternative approach is, of course, out of the scope of this review).

- The remaining remarks below concern other rock mechanics issues, perhaps more intricate than the basic material analyzed above. On [Plate 3.3.3.2-3] and [p.3-32] several new notions concerned with rock joint behavior are introduced without much elaboration. The top part of [Plate 3.3.3.2-3] indicates that rock joint behavior is being modeled by a shear stress versus strain relation of the "smooth joint" type, as can be seen by comparing to *Figure 2.2* from *Scott (1982)*, attached to this review report. However, a bonded joint would show a peak in the shear stress/deformation curve, as also indicated in Scott's Figure 2.2; the peak shear strength may be given by the Coulomb failure model:

$$\tau_{max} = \tau_o + \sigma_n \tan\phi \qquad (15)$$

where τ_o (also denoted C) is the cohesive stress. However, for a non-bonded joint, it is more likely that $\tau_{max} = \sigma_n \tan\Phi$, without cohesion. The term "*peak displacement*" used by JSS is misleading since displacement continues to increase after peak shear stress is reached. Finally, even the above-corrected model based on Coulomb failure may be too ideal. The alternative models of Patton, Jaeger, Barton, and Ladanyi and Archambault (described in *Scott, 1982*) attempt to take into account the processes of dilatation (riding up on asperities) and grinding (shearing through asperities) that may occur at low and high normal stresses.

- On [p. 3-33], there is an assertion related to [Plate 3.3.3.2-4] that seems confused or incorrect: "*Reacting to the variable magnitude of shear displacement* [$u_{s(y)}$], *fracture wallrock is compressed in the upper half and stretched in the lower half, and vice-versa across the fracture plane.*" We recall from [Plate 3.3.3.2-2] that the fracture is assumed to be vertical. Is the fracture mentioned here vertical or horizontal? Why does the process shear produce such different states of stresses on each side, instead of having both sides locally in compression or both sides locally in tension? Relative to the fracture plane, what are the planes on which such "compression" and "stretching" are being exerted? We recall here that stress is a three-dimensional symmetric tensor, each component of which represents a different surficial force (N/m^2).

- The remaining part of this sub-section is equally uncertain and vague. In particular, we note that the moving surface Z(x,y,t) marking the downward "progress of deformation", is not clearly defined in the context of this section. This does not help, given the other uncertainties about this in the previously reviewed [Sect. 3.3.2.2]. Also, it is often unclear whether terms like "limit equilibrium" and "tensile failure" are being applied to existing fractures during wall rock shearing or separation, or to the creation of new fractures and/or to a continuous rock mass. Finally, we point [Plate 3.3.3.2-5] where JSS attempts to explain the state of stress in the zone "*above the surface Z(x,y,t)*". Comparing to the first plate where the Griffith-Coulomb failure envelope was first shown (discussed earlier), we have here a shifted failure envelope that intersects the σ-axis at $\sigma=0$. this means that the tensile strength (denoted T in the first plate) is now equal to zero. No rationale is provided for this change. The remark that tensile strength is now assumed to be null is not even made.

- In summary, given all the above conceptual errors or inaccuracies and the lack of clearly defined premises in [sub-section 3.3.3.2], this reviewer finds it difficult to accept the conclusions of this sub-section. It may well be, however, that more convincing formulations of the problem exist in the literature and would lead to a partially similar scenario, e.g. with downward migrating deformation surface, etc. Such a scenario would then have to be confirmed by various means, including mathematical modeling.

[Sub-Section 3.3.3.3]: *"The Hydraulic Conductivity Structure in a Deforming Fractured Medium"*:

- In this subsection is proposed a simple model of the stress-dependent hydraulic aperture (*"conducting aperture"*) of a fracture. The total hydraulic aperture (a) is assumed to be a piecewise linear function of normal effective stress (total stress minus fluid pressure), plus an additional aperture (a_d) to account for shear dilatation effects [Plate 3.3.3.3-1]. It appears that this model is borrowed from *Harper and Last (1987)*, an unpublished paper not mentioned in the main text. The annotations in the plate raise a few questions. Does not the "dilatational" aperture (a_d) depend on shear displacement ("u_s-u_{sp}") and is not the latter related to the applied normal stress? How is the dependence of (a_d) on normal stress integrated into the model? If there are good answers to these questions, they are not given here.

- The first equation appearing on [p.3-34] gives correctly the *"hydraulic conductivity of a fracture"*:

$$K_f = a^2 \frac{\rho g}{12\mu} \tag{16}$$

To be more precise, it should be specified that this conductivity gives the average fluid velocity through a single fracture using a Darcy-type equation:

$$\vec{V} = K_f \, \vec{J} \tag{17}$$

in terms of the prescribed hydraulic gradient:

$$\vec{J} = -\frac{1}{\rho g} \nabla (p + \rho g z) \tag{18}$$

In case the vector $\vec{J}$ does not lie in the plane of the fracture, its projection onto the plane of the fracture must be used instead. The resulting fluid velocity is therefore assumed to be parallel to the fracture plane.

- Darcy-type equations, like the first equation on [p.3-34] of JSS, assume implicitly that the flow is slow, non-turbulent, dominantly planar, and satisfies a no-slip condition at the fracture walls. These assumptions are necessary to approximate the complex Navier-Stokes equations. To clarify this, let us write the Navier-Stokes equation for an incompressible viscous fluid:

$$\rho \frac{D\vec{V}}{Dt} = \rho\left(\frac{\partial \vec{V}}{\partial t} + (\vec{V}\cdot\nabla)(\vec{V})\right) = \vec{J} + \frac{\mu}{\rho g}\Delta(\vec{V}) \tag{19}$$

where the operator D•/Dt denotes the point-material derivative. In particular, it is generally recognized that the laminar, Darcy-type flow regime will not hold for Reynolds number larger than roughly 1-10. Taking the most optimistic view, we require:

$$(Re)_f = \frac{V_f a}{\nu} \leq 10 \tag{20}$$

For water at room temperature (T ≈ 15.5 °C) the kinematic viscosity is $\gamma \approx 1.124\ 10^{-2}$ cm^2/sec, which gives roughly $V_f \leq 0.1/a$ if V_f is given in cm/sec, and a in cm. This shows a possible limit to the range of validity of the fracture flow model being used here, particularly in the context of seismic pumping.

- The "*storativity*" of a single fracture is improperly defined in [p.3-34] as:

$$S_f = a \tag{21}$$

In groundwater hydraulics, the terms "storativity", "storage coefficient", and "specific yield", are dimensionless parameters characterizing the change in stored volume of water per unit area per unit change of pressure, or hydraulic head, or drawdown. For a confined aquifer of thickness (a) and porosity (Φ), the storativity is due to the compressibilities of the porous matrix (α) and water (β):

$$\textit{CONFINED}: \quad S = \rho g(\alpha + \phi\beta)a \tag{22}$$

Note that we purposedly use the same notation for aquifer thickness and fracture aperture. On the other hand, the specific yield of an unconfined aquifer is roughly a measure of its effective porosity. The specific yield of an array of vertical fractures being desaturated by drawdown will therefore be:

$$\textit{UNCONFINED}: \quad S = \textit{Effective Porosity} \tag{23}$$

This is just S = a/L if the fractures are parallel with spacing (L) and aperture (a). The specific yield of a single fracture, by the same token, is simply one. The storativity of a single fracture due to compressibility effects is another matter, which would have to be investigated carefully. In all that follows, this reviewer will assume that the so-called "storativity" (S_f) really means hydraulic aperture (a) in the JSS report.

- Plate [3.3.3.3-2] crudely depicts the consequences of aperture-stress relations on the hydraulic conductivity: the latter increases as the normal effective stress decreases in time. There is a minor misrepresentation of the curve $K_f(t)$: since aperture a(t) is shown to increase linearly in time, and since $K_f \sim a^2$ from eq. (16), or the first equation of [p.3-34], the curve $K_f(t)$ should have the appearance of a parabola starting smoothly at $t=t_1$ but ending with a slope discontinuity at its upper limit. The vague S-shaped curve shown on the third graph from top is therefore inaccurate.

- On Plates [3.3.3.3-2] through [3.3.3.3-6], there is an average of three graphs per plate, many of which seem redundant, or easily replaceable by simple explanations in the text. The graph on the lowest part of [Plate 3.3.3.3-3] should have been shown on the first plate [Plate 3.3.3.3-1]. It indicates that the "dilatational" aperture (a_d) is assumed to be a linearly increasing function of the shear displacement (u_s) from $u_s = u_{sp}$ (so-called "*peak*" displacement) up to an unspecified upper value where the maximum possible "dilatational" aperture is assumed to be reached. The text does not discuss this upper limit of the dilatational aperture, except to say that there is an upper limit [first paragraph of p.3-35]. Finally, the analogy with the buckling of slender bodies in the last paragraph of [p.3-35] does not help much and/or is not sufficiently developed.

- On [p.3-36, first paragraph]: the thread that ultimately leads to "*tensile failure*" via shear failure of a pre-existing "*segment of deforming fracture*" is rather tenuous. There is the assumption that both the mean principal stress $(\sigma_1+\sigma_3)/2$ and the difference $(\sigma_1-\sigma_3)/2$ decrease with time. This can be shown to imply that σ_1 must decrease with time while σ_3 may either increase or decrease, at a rate smaller than σ_1 in absolute value; this assumption seems a little contrived and requires to be properly justified by invoking some basic physics. Finally, the scope of the conclusion that "*the values of hydraulic conductivity and storativity are dramatically increased*" is not clear: does this apply to the overall system or just to a localized segments of those deforming fractures that happen to behave as described above?

- More generally there is a lack of detail regarding the geometry and localization (or spatial distribution) of tectonic stresses, fractures, and deformations. Boundary conditions and domain geometry remain vague or unspecified, even where they may play important roles. The geometry and interconnectivity (or lack of interconnectivity) of fractures is not explicitly discussed from a large scale perspective. Furthermore, none of the technical arguments ensures that the balance of tectonic stresses, fluid pressures, overburden, and momentum will be satisfied given the assumptions and simplified scenarios being developed. There is not enough "physics" there for modeling even roughly, the large scale propagation of rock failure from "*singular points*".

[Sub-Section 3.3.3.4]: *"Summary and Conclusions"*

- Upon reading this section, the reader is reminded that the previous developments were an attempt to elaborate from an existing theory (*Scholz 1973, Sibson et al. 1975*). Using and adapting an existing theory is a fair and valid approach. Unfortunately, this approach has resulted here in a garbled discussion. Indeed, where the discussion coincides with the original (*Sibson et al. 1975 for example*), it appears to be of lesser clarity and quality than the original.

[Section 3.3.4]: *"Conceptual Model of a Groundwater System in a Deforming Fractured Medium"*

- [p.3-38 and Plate 3.3.4-1]: On the first paragraph of [p.3-38], the boundary conditions of the (initially steady) GW flow system are described as follows: *"In this model, both vertical boundaries are considered as 'no fluid-flux' or 'const. head' boundaries. Hydraulic pressures acting at both vertical boundaries differ by a constant amount, which is represented by the term cx ·"*. However, the so-called *"no fluid-flux"* condition imposed on the vertical boundaries is hardly compatible with the *"const. head"* boundaries under the conditions described above [Plate 3.3.4-1]. Other problems with the plate itself are as follows. There is a meaningless label indicating the "POSITION OF WATER TABLE AT TIME t_2 WHEN $Z(x,t_2) >> 0$"; it is the opinion of this reviewer that no quantity can be meaningfully described as being "much greater than zero" without some additional element of comparison (some non-zero scale). In addition, the position of the water table as indicated in the top part of the plate looks arbitrary (is it?). Finally, the location Z=0 is not defined on the graph; could that be where the label Z_0 appears?

- The discussion on [p.3-38] also misses some points, such as the importance of the time scales of the dynamic processes being studied, and basic principles such as the continuity of fluid pressure and normal flux at the moving tectonic interface $z=Z(x,y,t)$. A sometimes annoying confusion is frequently made: the depth of the tectonic interface, $Z(x,y,t)$, should be distinguished from plain depth z, and the equation of the interface is:

$$F(x,y,z,t) = z - Z(x,y,t) = 0 \qquad (24)$$

and not simply *"Z(x,yt)"*. Finally, note that the rate of change of pressure and other quantities at the moving interface can be separated in two parts:

$$\frac{Dp}{Dt} = \frac{\partial p}{\partial t} + \vec{V}_s \cdot \nabla p \qquad (25)$$

where $\vec{V}_s$ is the velocity of the moving interface.

- Plates [3.3.4-2] through [3.3.4-5] present an *"idealized history"* of changes in a groundwater flow system, in response to tectonically induced stresses during a *"cycle of tectonic deformation"*. There is a minor glitch: contrary to what is said in the text, the history is not *"presented in the form of a depth versus hydraulic potential relationship"* but rather in the form of a pressure profile $p(z)$. Plate [3.3.4-4] explains that the downward movement of the tectonic interface $z=Z(x,y,t)$ entrains the water table downward: this is not quantified, although there is a profusion of symbols on that plate. The so-called *"overpressure"* Δp_{max} should be related to water table changes, perhaps like $\Delta p_{max} = \rho g \Delta d_w$. Finally, Plate [3.3.4-5] depicts how the water table rises above its initial position at the end of the tectonic cycle due to closure of the previously dilated fracture, ultimately "decaying" to the *"equilibrium"* position, i.e. the initial steady state. Apparently, the tectonic deformation cycle is assumed periodic (or say ergodic) with same end-state as initial-state. The higher than initial water table level is explained by the existence of an overpressure at depth, but this seems insufficient or incomplete. To justify this scenario would require some basic physics, including mass balance and conservation of momentum for the fluid, possibly with inertial effects. To elaborate further would be out of the scope of this review (see, however, the attached Appendix).

- On Plate [3.3.4-6] there is a cautionary note that *"The drawing is diagrammatic"*, which we presume means schematic and even sketchy. The same remark applies quite well to many of the plates reviewed above. The last plate of this sub-section [Plate 3.3.4-7] depicts a steep water table slope due to inhomogeneous hydraulic properties caused by inhomogeneous strain. There is virtually no physics to back-up the figure, and so we must assume that the plate is only the schematic representation of a possible explanation for high water table gradients.

4. SUMMARY AND CONCLUSIONS:

Our detailed review (section 3) appears to confirm the overall impression as stated earlier in the overview section of this letter report (section 2). In the reviewed report, we have found quite often that discussions concerning conceptual models of physical processes lacked consistency and/or failed to make use of basic physical principles. Technical discussions elaborating scenarios from models of physical processes are often incomplete, imbalanced, and/or flawed. These apparent defects can be traced back to several causes, among them: (i) a lack of detail regarding the spatial distribution of tectonic stresses, deformations, and fractures; (ii) arbitrary, unspecified, or vague context (boundary-initial conditions); (iii) attempts to predict complex coupled processes without the proper tools, e.g. without a physically-based model that explicitly takes into account the balance of forces for example; (iv) accumulated ambiguities and/or flawed representations of certain processes and/or of existing models of these processes (joint behavior; storage coefficient and hydraulic transmissivity; Mohr circle diagrams and failure envelopes). Some of the conclusions are astonishingly vague given the intricate technical discussions that precede them. A case in point is the *summary-conclusion* [section 3.3.2.4] where we learn that the stress field must be *"variable in time and space"*, that limit equilibrium conditions are *"occasionally present"* and that *"flow occurs in a deforming fractured medium"*. In this and several other sub-sections, a clear summary of the specific premises and of the proposed mechanisms or scenarios is often lacking. For technical details, we refer to the previous section 3.

This relatively long report has, however, the merit of pointing out the relevance of large-scale coupled geoprocesses to the safety of an underground geologic repository. Processes like those discussed in the report (and others) may be potentially relevant to the prospective Yucca Mountain site, and their magnitude deserves to be seriously investigated, including by way of mathematical modeling. The weaknesses of the JSS report, as identified by this review, illustrate the difficulty of developing tractable quantitative models that integrate the observed characteristics of real geoprocesses while being compatible with first physical principles. It is the opinion of this reviewer that a model that attempts to merely fit empirical observations without satisfying basic physical principles, such as conservation of mass and momentum, cannot be realistic and is essentially useless as a predictive tool. Consistent mathematical models of coupled hydro-thermo-mechanical geoprocesses have and are being developed based on detailed balance equations and specific constitutive relations (see attached *Appendix*). Although precise quantitative predictions are hampered by model uncertainties, such as the poorly understood constitutive relations of stress-strain-slippage in a deforming fractured rock, at least approximate or bounding calculations may be feasible. In addition, sensitivity analyses can be conducted to identify the most critical assumptions in the models, as pointed out in some of the papers reviewed in the *Appendix*.

In closing, we note the following statements from the conclusions section of the JSS report: *"The most important licensing concern is a potential rise of the water table"* [Sect. 5.0, p.5-8]; and: *"Most of the above licensing concerns are not new concerns. They were raised previously, in one form or another by various parties, most* *notably by the U. S. Nuclear Regulatory Commission and the State of Nevada. The proposed conceptual understanding of the Yucca Mountain groundwater system reinforces those concerns and provides a uniform theoretical background for them"* [p.5-9]. It is the opinion of this reviewer that, considering the weaknesses pointed out by this review, the JSS report fails to provide a uniform theoretical background, contrary to author's intentions.

5. APPENDIX TO LETTER REPORT:

QUANTITATIVE MODELING OF COUPLED HYDRO-THERMO-MECHANICAL (HTM) PROCESSES: A SPARSE LITERATURE REVIEW

- *Bethke (1985)* and *Bethke et al. (1988)* developed quantitative simulation models of compaction driven groundwater flow and heat transfer in sedimentary basins over geologic time scales. For example, *Bethke et al. (1988)* solved a coupled system of two-dimensional, transient partial differential equations to simulate the development of Gulf Coast geopressures over 156 million years of subsidence and filling of the Gulf of Mexico Basin. The subsurface movements of pore fluids were modeled by:

$$\phi\beta\frac{\partial P}{\partial t} = \nabla\cdot\left(\frac{\overline{\overline{k}}}{\mu}\nabla P-\rho g\hat{z}\right)-\frac{1}{1-\phi}\frac{\partial\phi}{\partial t}+\alpha\phi\frac{\partial T}{\partial t} \tag{A1}$$

  where:

  - (P) is pore fluid pressure,
  - (Φ) is porosity,
  - (T) is temperature,
  - ($\overline{\overline{k}}$) is the permeability tensor,
  - (β) is fluid compressibility,
  - (α) is fluid thermal expansivity,
  - (ρ) is fluid density,
  - (μ) is dynamic viscosity, and
  - (g) is acceleration of gravity.

  The porosity is coupled to effective stress, and the temperature gradient is coupled to the rate of sediment burial via equations not shown here.

- *Verma and Pruess (1988)* solved a system of coupled, multiphase-multicomponent equations for fluid flow, heat flow, and the transport of reacting chemical species (silica). In one case, they numerically simulated the flow-transport processes in initially saturated rocks over scales of 7 km x 2.5 km. The canister was treated as a heat source and the geothermal gradient was also taken into account. Although rock deformations were not modeled, there was an additional coupling between silica transport-dissolution-precipitation and fluid flow, through changes in rock permeability. A semi-empirical model was used to relate changes in porosity to changes in permeability. In comparing with earlier work by *Wyble (1958)* on sandstones, they note that porosity reductions due to mineral precipitation more strongly reduce pore throat pathways than porosity reductions due to applied stresses (in his sandstone compression experiments, *Wyble* finds "irreducible permeability" values about 30-60% the initial permeability value).

- The proceedings book entitled "Coupled Processes Associated with Nuclear Waste Repositories" (*C.F. Tsang editor, 1987*) contains a number of contributions to the subject of quantitative modeling of HTM processes, in particular on "Coupled Processes in Geomechanics" *(Cook, Chap. 7)*, "Numerical Modeling of Coupled Fluid, Heat, and Solute Transport in Deformable Fractured Rock" *(Chan et al. Chap. 44)*, "A Coupled Model for Fluid Driven Fractures" *(Heuze et al. Chap 47)*, "Simulation of Coupled Thermal-Hydraulic-Mechanical Interactions in Fluid Injection into Fractured Rocks" *(Noorishad and Tsang, Chap. 49)*, and on the "Development of Finite Element Code for the Analysis of Coupled Thermo-Hydro-Mechanical Behaviors of a Saturated-Unsaturated Medium" *(Ohnishi et al., Chap. 50)*, among others. Some notable aspects from these works are listed below:

  - In *Chap. 44, Chan et al.* use the 3D finite element code "MOTIF" for simulating coupled flow of groundwater, brine, and heat in porous-fractured medium. They discuss the difficulties of coupling these flow processes with mechanical effects, due to poorly understood constitutive relations for stress-strain-slippage in a deforming fractured rocks. The discussion is based on field observations during shaft excavation in a granite at Canada's Underground Research Laboratory (Lac du Bonnet, Manitoba).

  - In *Chap. 47, Heuze et al.* combined a (1D) model of fluid flow through rock joints with an elastic finite element code and a nonlinear model for the movement of the joint elements. Joint apertures and fluid flow are mutually dependent. The goal was to simulate hydro-fracturing for gas recovery in a jointed sandstone-shale formation.

  - In *Chap. 49, Noorishad and Tsang* use the numerical code "ROCMAS" (*Noorishad, 1984*) to analyze cold water hydro-fracturing in warm rock reservoirs. Their conclusion is that hydrofracturing occurs earlier when thermal effects are taken into account. They note that the general scarcity of data precludes detailed simulations of coupled hydro-thermo-mechanical processes but still allows for a scoping analysis. Their paper is an example of such a scoping analysis using numerical modeling.

  - In *Chap. 50, Ohnishi et al.* address the problem of modeling coupled HTM processes in a purely elastic mode (no fractures or joints), but for unsaturated rather than saturated geologic media. They point out the lack of a consensus concerning the generalization of Terzaghi's effective stress to the case of unsaturated media. For the stress-strain-temperature behavior of the medium, they use the "Duhamel-Neuman" constitutive relation, which leads to:

$$\sigma_{ij}^{eff} = [\lambda e_{kk} - \beta (T - T_o)] \delta_{ij} + 2\mu e_{ij} \tag{A2}$$

with α the thermal expansion coefficient, $\beta = (3\lambda + 2\mu)\alpha$, and γ and μ are the elasticity coefficients of Lamé; in terms of Young's modulus, E and Poisson's modulus γ, we have $\lambda = vE/(1-2v)(1+v)$ and $\mu = 0.5\ E/(1+v)$. The interesting thing to notice is that thermal expansion can cancel out the effects of mechanical compression. This can be seen more clearly by considering the spherical part of the stress tensor σ_{kk} from eq. (E2). The result is:

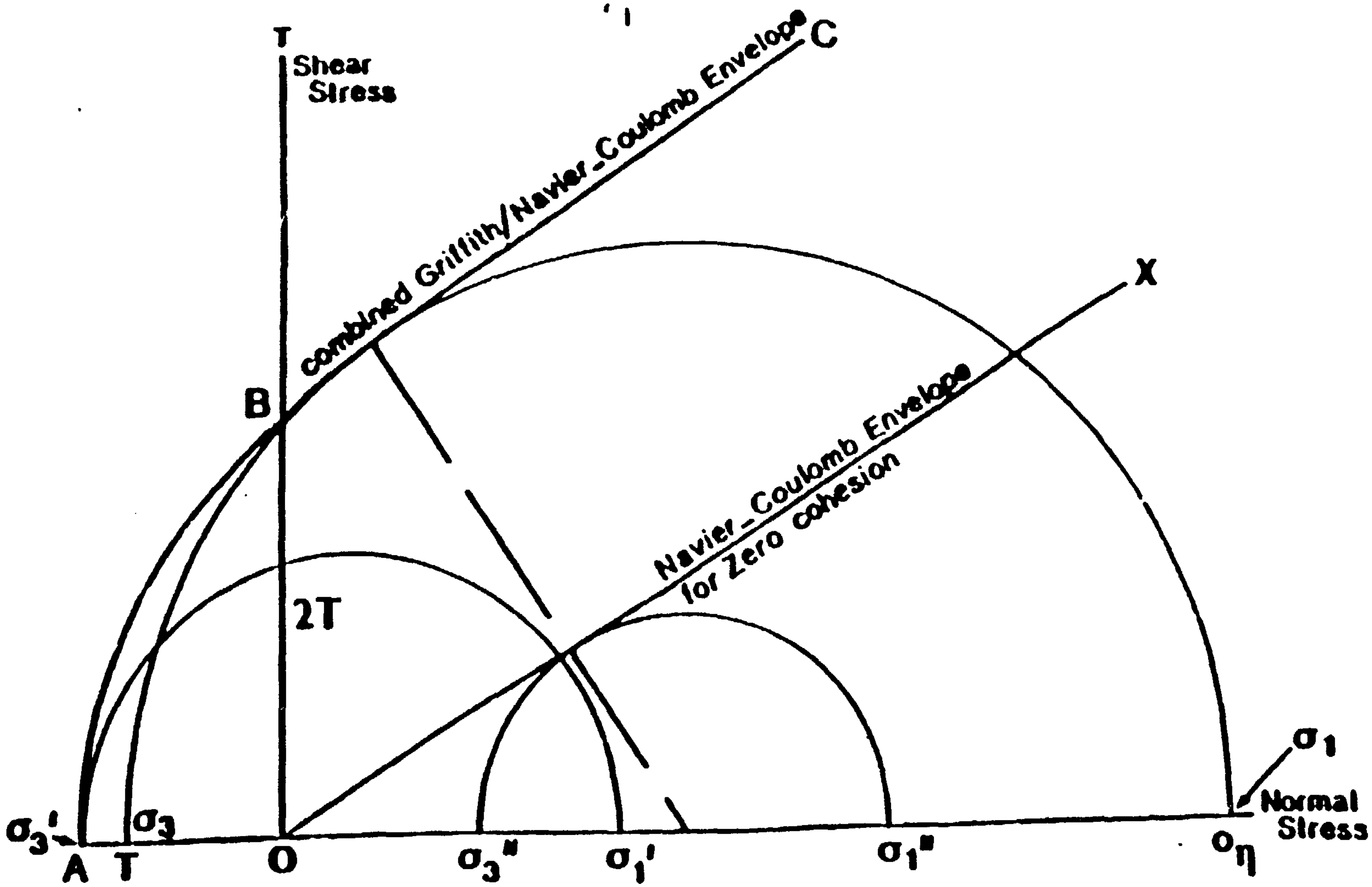
$$\lambda e_{kk} = \sigma_{kk} + \beta (T - T_0) \tag{A3}$$

and it should be kept in mind that the convention used here is $\sigma<0$ in compression, in contrast with the convention used by us in the main text ($\sigma>0$ in compression).

6. ATTACHED FIGURES AND DOCUMENTS

- Plate 3.3.3.2-1 from *Szymanski, 1989*
- Figure 7 from *Shaw, 1980*
- Caption of Figure 7 from *Shaw, 1980*
- Mohr diagram, Coulomb, and Griffith failure models (pp. 217-221 from *Shaw, 1980)*
- Figure 2.2 from *Scott, 1982*

PLATE 3.3.3.2.-1
(Szymanski, 1989)



Mohr's representation of stresses in two dimensions and combined Griffith/Navier Coulomb failure envelope

MOHR DIAGRAMS
(Figure 7 from Shaw, 1980)

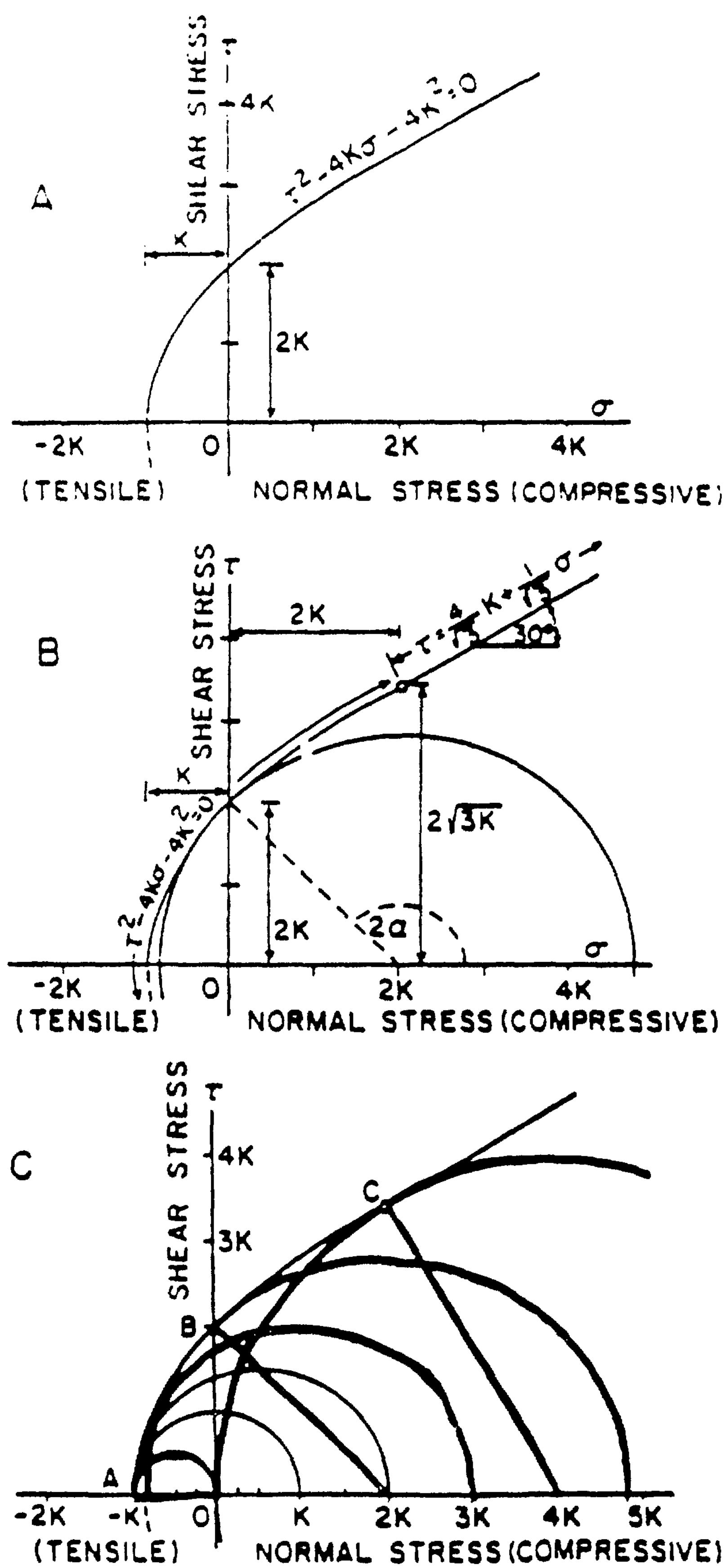


FIGURE 7 CAPTION
(from Shaw, 1980)

Figure 7. Mohr diagrams illustrating the Griffith and composite Griffith plus Coulomb failure laws (redrawn from Secor, 1965, figures 1, 2 and 3). *Diagram A* shows the parabolic form of the Griffith failure envelope and the relationship of intercepts on the σ and τ axes. *Diagram B* shows the composite envelope and the equations of the parabolic and linear portions. *Diagram C* shows a family of Mohr circles tangent at different points along the composite envelope: circles tangent at A theoretically give purely extensional failure along planes 90° to the σ_3 axis; circles tangent at points between A and B give failure involving both extension and shear and failure angles between those of pure extension and pure shear (theoretically, at angles between 90° and 70° to the σ_3 axis). The circle tangent at point C theoretically involves only compressive shear (with no effectively tensile components anywhere in the material) acting along a plane at a theoretical angle of 60° to σ_3. In actual materials, this limiting angle for shear failure may be closer to 70° (Secor, 1965, p. 639). The regime corresponding to points between A and C in diagram C is termed in this paper *extensional shear failure*. Strictly, this regime terminates at B relative to potential opening across the theoretical failure plane, but failure phenomena between B and C are considered to represent important transitions between the fields of dominantly compressive and dominantly tensile failures.

(x)

pp. 217-221 from Shaw (1980)

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... because the
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... criteria of extensional
... of magma ... in
... ostatic pressure ...
... re the pressure head is

... Shaw and Swanson
Columbia River basalt
escribed the results of
and generally confirm

the melting rate is so low
e surface, because there
effects during intrusion
rate, or extremely long
gma reservoir to provide
ise to reach the surface

transport rates, magma
than in cases (a-b) and
mblance to the problems
uch as those exemplified
canic necks, kimberlite
of course, a gas phase
ed to discussion of two-
al phases. Anderson and
n some detail for systems
corrosion theories. They
e pipes.
pes combining features
re is one combination,
si-steady model. In this
urce are rapid enough to
a intrusion to occur at
tion, the generation and
ing on long enough, that
it rates that also approx-
except for some fraction
pinnings of the volcanic

In model (c) there is no opportunity for accumulation of large magma reservoirs near the source, and individual magma pulses are not large enough to reach from the source to the surface during one extensional failure event. The number of events, however, is sufficiently large and frequent that a sequence of pulses moves upward in a discontinuous progression of interacting diking events. The frequency of these events is controlled by the local pressure fluctuations (up or down depending on the cooling-heating, addition-subtraction effects of magma increments) and the stress criteria of extensional failure relating to the local tectonic stress field and the effects of liquid-crystal proportions on rock strengths.

In this model, the fluid pressure of magma oscillates around a mean value approximating the average lithostatic stress. The magnitudes of pressure deviations from the mean are roughly defined by the range of yield strengths of the rock. These stress ranges, in turn, are determined in part by the local proportion of liquid magma; estimates are given later for tensile strength versus crystal-liquid ratios of basaltic magma.

If the balances were precisely steady in example (c), the result would be a continuous magma conduit with a continuous "artesian" volcanic extrusion at the surface that exactly matched the melting rate in the mantle. The steady state would also imply that fluid pressure gradients in the conduit were precisely balanced against rock stress gradients in the walls. The unlikelihood of maintaining such delicate balances is apparent in relation to even the simplest petrological models of the lithosphere and of plate tectonics, particularly when it is realized that, simultaneously, the volcanic edifice is growing, and probably propagating laterally, and that both the lithosphere and asthenosphere are in relative motion. Problems related to gradients of pressure and stress are addressed by Secor and Pollard (1975) and by Pollard (1976).

It is probably evident at this point that, even though model (c) is a hypothetical description of one combination of possible limiting factors influencing the pressure distribution, it is thought to describe some of the effects analogous to those operating in the Hawaiian system as portrayed in Figure 5.

The Mohr Diagram and Coulomb Failure Criterion

For the purpose of outlining the types of stress regime relevant to extensional fracture, the simplest graphical approach is given by the Mohr diagram of normal and shear stresses. Figure 6 shows the relationships of greatest and least principal effective stresses to normal and shear stresses, and illustrates one form of failure criterion, "Coulomb's law of failure," which is roughly applicable to some types of rocks (Hubbert

(2)

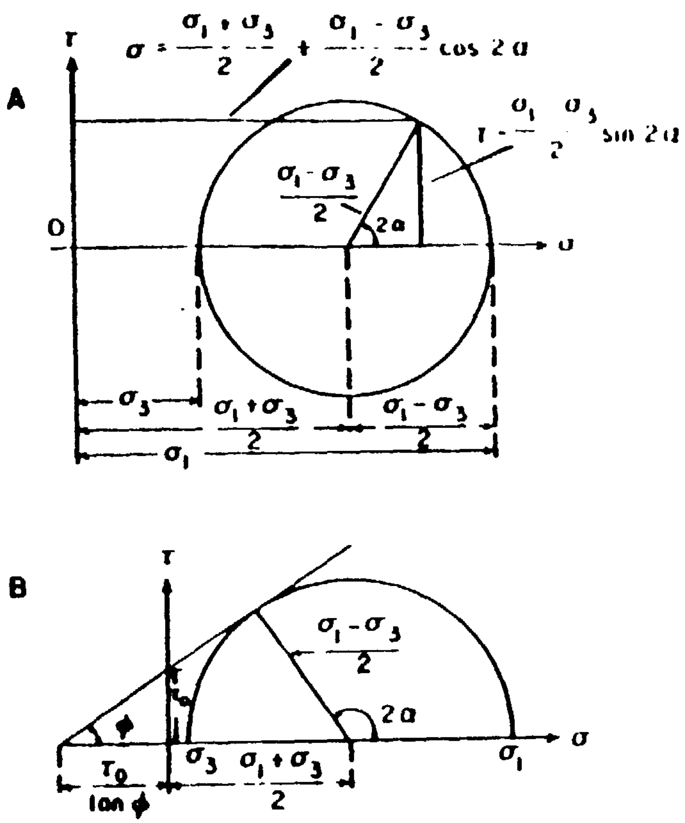

Figure 6 Trigonometric relations among normal stresses, shear stresses and maximum and minimum principal stresses represented by Mohr diagrams (redrawn from Hubbert and Rubey, 1959, Figures 1 and 3; see this reference for mathematical discussion of decomposition of stresses). *Diagram A* shows a complete Mohr circle and the angular relationships of points on the circle to maximum and minimum principal stresses. *Diagram B* illustrates these relations in terms of points on the circle tangent to a locus of values (σ, τ) representing conditions of shear failure; in this case the locus is a straight line, called the "failure envelope," representing Coulomb's law of failure (see Voight, 1976). The angle 2α is twice the angle between the direction of the minimum principal stress and the failure plane; α in this example is 60°. The angle ϕ is called the angle of internal friction. In this example ϕ is 30° and is equal to the angle between the failure plane and the direction of the maximum principal stress.

and Rubey, 1959, p. 124). This law is stated in terms of *effective normal stress*, σ, in the form

$$\tau = \tau_0 + \sigma \tan\phi \qquad (2)$$

where the angle ϕ is called the *angle of internal friction* and $\tan\phi$ is called the "*coefficient of internal friction*." The intercept τ_0 is in the nature of a *cohesive strength* when $\sigma = 0$, that is, when the fluid pressure p equals the normal stress, S. Brace (1969) has shown experimentally that the law of *effective stress* holds for the critical conditions and form of failure of a variety of rocks as long as the strain rate is not too high (e.g., less than 10^{-7} per second for Westerly granite with water as the fluid).

In Figure 6B the intercept of the *failure envelope* with the σ axis identifies the limit of *cohesive strength* as a critical value of purely tensile stress. This condition occurs in terms of *effective stress* only if $p > S$. Hubbert and Rubey (1959, p. 142) argued that this situation is unlikely for highly jointed rock and assumed that $\tau_0 = 0$, that is, the failure envelope for this case starts at the origin. Hsu (1969) took exception to that assumption and emphasized the importance of τ_0 in overthrust faulting.

I believe that this issue depends essentially on the nature of the rocks and dynamics of the pore fluid conditions, and cannot be generalized. The Hubbert-Rubey analysis is one limit in a family of critical conditions for sliding. In the present discussion τ_0 plays an important role because it is related to tensile strength and hence to melt fraction in partially molten rock, as will be shown. In terms of the Coulomb criterion (Figure 6B) the *tensile strength*, symbolized K following Secor (1965), is defined as

$$K = \frac{\tau_0}{\tan\phi} \qquad (3)$$

The Griffith Theory of Fracture

Many other studies have shown that the failure envelope often is not a straight line. Another form of failure criterion is given by the *Griffith theory of fracture* (Griffith, 1921, 1925), which is more applicable than the Coulomb theory to many materials. It is the criterion used by Secor (1965) to discuss *extension fracture* in crustal rocks (see this reference for fracture terminology); it is also assumed here to be more representative for partly molten rocks.

The Griffith theory actually refers to a method of analysis based on the energy balances involved in the growth of an existing crack. The fact that most rocks and minerals contain microcracks and flaws of various kinds makes this theory a logical starting point for discussion of crack propagation, particularly for rock containing a melt phase in the form of intergranular films or as a plexus of fluid-filled fractures.

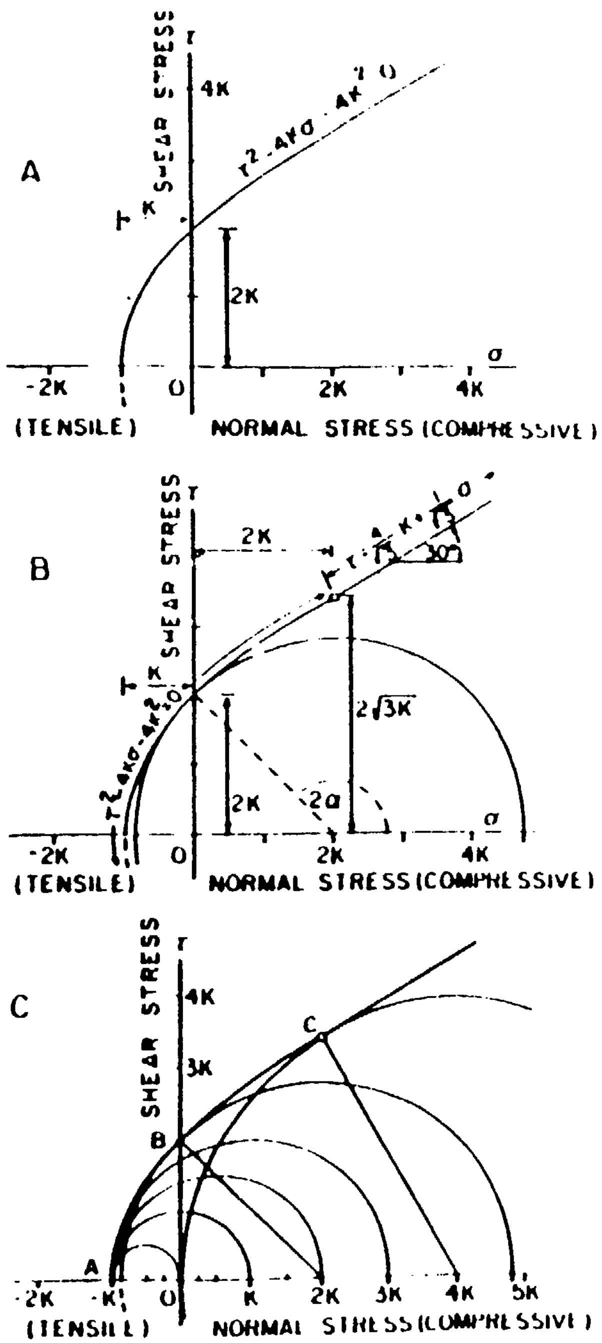

Figure 7. Mohr diagrams illustrating the Griffith and composite Griffith plus Coulomb failure laws (redrawn from Secor, 1965, figures 1, 2 and 3). *Diagram A* shows the parabolic form of the Griffith failure envelope and the relationship of intercepts on the σ and τ axes. *Diagram B* shows the composite envelope and the equations of the parabolic and linear portions. *Diagram C* shows a family of Mohr circles tangent at different points along the composite envelope. Circles tangent at A theoretically give purely extensional failure along planes 90° to the σ_3 axis; circles tangent at points between A and B give failure involving both extension and shear and failure angles between those of pure extension and pure shear (theoretically at angles between 90° and 70° to the σ_3 axis). The circle tangent at point C theoretically involves only compressive shear with no effectively tensile component anywhere in the material, acting along a plane at a theoretical angle of 60° [illegible]. In actual materials, the limiting angle for shear failure may be closer to 70° (Secor, 1965, p. 639). The regime corresponding to points between A and C on the envelope is termed in this paper *extensional shear failure*. Strictly, this regime [illegible] B relative to [illegible] the theoretical failure plane. [illegible] phenomena between B and C [illegible] between the fields of dominantly compressive and dominantly tensile failure.

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Many references discuss the nature of Griffith cracks and the application of Griffith's analysis of fracture; a detailed mathematical derivation is given by Lawn and Wilshaw (1975, p. 5-11), and a comparison of the relationship between the Griffith and Coulomb criteria of failure is given by Obert and Duvall (1967, p. 307-310) in terms of the Mohr representation.

The Griffith theory form of the failure envelope is defined by (Secor, 1965, p. 636)

$$\tau^2 - 4K\sigma - 4K^2 = 0 \tag{1}$$

The relationships of normal and shear stresses to this form of the failure envelope and to tensile strength, K, are shown in Figure 7, taken from the detailed graphical constructions in Secor's paper. From Figure 7A it is seen that the shear stress for $\sigma = 0$ is given by

$$\tau = 2K \tag{2}$$

and the tensile strength is therefore K [illegible] instead of the more negative value given by Equation (1). At values of normal stress above about $2K$ the failure envelope is assumed to be linear in conformance with Coulomb's law. Therefore, it is referred to as a composite failure envelope. Figure 7B illustrates the Mohr relations for the composite diagram. Figure 7C identifies the corresponding regimes of failure from pure extension to pure shear failures.

Tensile Strength of Partly Molten Rock

The relationship in Figure 7, together with Equation (2), provides a simple way to estimate tensile strengths from viscometric studies of

SHEAR STRESS/DEFORMATION CURVES FOR JOINTS
(Figure 2.2 from Scott, 1982)

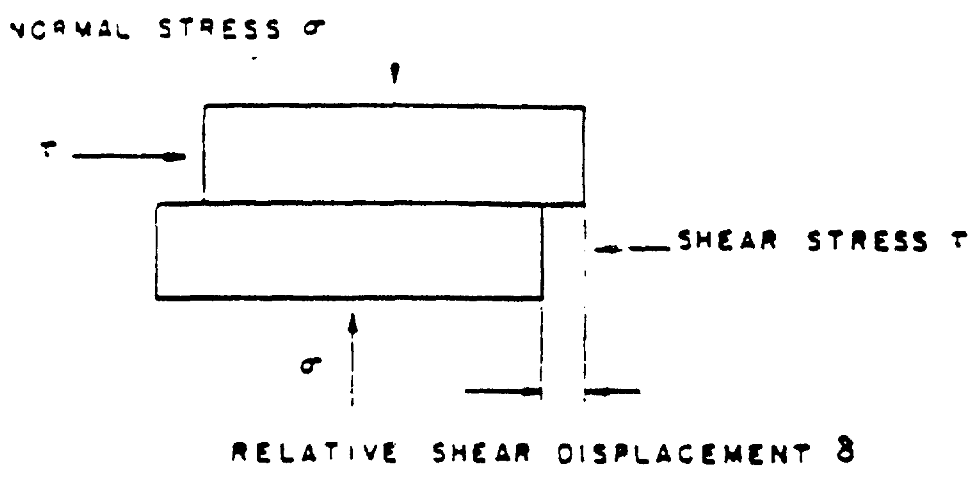

Figure 2.1 Smooth Joint Under Direct Shear Conditions.

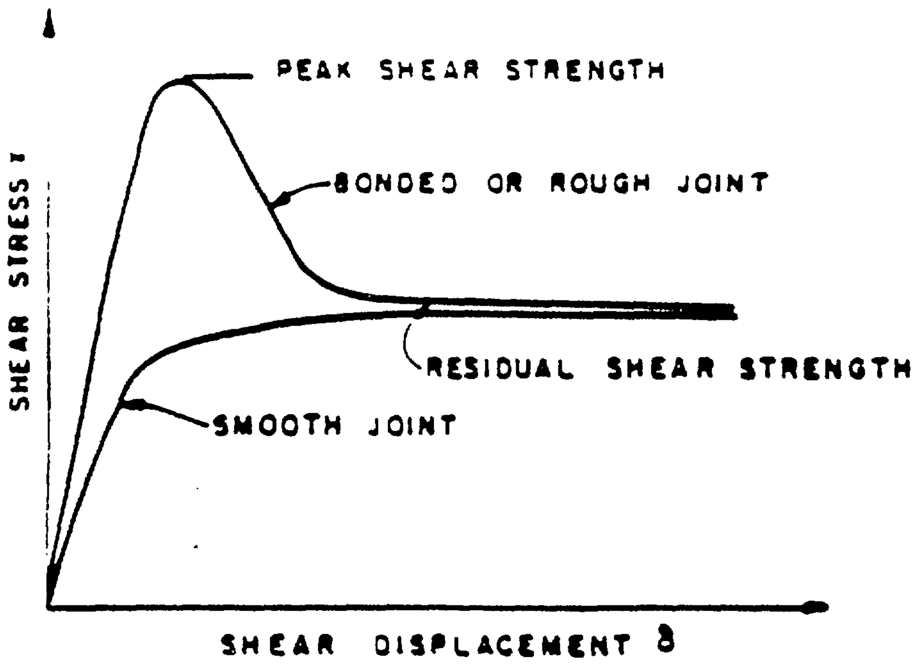

Figure 2.2 Typical Shear Stress vs. Shear Deformation Curves.

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