



Letter to the Editor

We write to answer a criticism made by Simon et al. in their paper "A Poroelastic Finite Element Formulation Including Transport and Swelling in Soft Tissue Structure," JOURNAL OF BIOMECHANICAL ENGINEERING, Vol. 118, 1996, pp. 1–9. Specifically, these authors state that the relation between the fixed charge density c^F and the dilatation e of the tissue is:

$$c^F = c_R^F [1 - n_R^{-1} e]^{-1}. \quad (\text{Eq. (46); Simon et al.})$$

Here n_R is the porosity at the reference configuration and c_R^F is the reference fixed charge density (i.e., the fixed charge per initial tissue water volume), and e is the dilatation, which is equal to $\mathbf{m}^T \mathbf{e}$ in Simon et al.'s notation. Based on Eq. (46), Simon et al. state that "Lai et al.'s equation appears to be missing the negative exponent found in the above equation." Now, using the notation in their paper, the equation found in the Lai et al. paper [JOURNAL OF BIOMECHANICAL ENGINEERING, Vol. 113, 1991, pp. 245–258] is:

$$c^F = c_R^F [1 - n_R^{-1} e]. \quad (\text{Eq. (66); Lai et al.})$$

Clearly, when the volume of the tissue increases (i.e., a positive dilatation e), the fixed charge density should decrease. But, according to Simon et al.'s Eq. (46), a positive dilatation e leads to an increase in fixed charge density. This is obviously incorrect. We note that the error in Simon et al.'s Eq. (46) follows from the error in their Eq. (5b), where $(J - 1)$ should have been $(1 - J)$.

We note also that the right side of Lai et al.'s Eq. (66) is the first two terms of the binomial series expansion of $[1 + n_R^{-1} e]^{-1}$, where the exponent (-1) yielded the -1 coefficient of the second term of the expression. Incidentally, in a similar derivation, the well-known equation of mass density of a solid with dilatation e is given by $\rho = \rho_R [1 - e]$, not $\rho = \rho_R [1 - e]^{-1}$.

We wish to take this opportunity to make a few additional comments regarding this paper:

- 1 We applaud the authors for developing a finite element model to study the swelling behaviors of charge hydrated tissue.
- 2 With regard to the development of a constitutive theory for the swelling behaviors of charged-hydrated soft biological tissues, in our opinion, the present paper presents few new ideas over our triphasic theory [indeed, they stated in their paper that these two "formulations are equivalent"]. The following examples will illustrate this point:

(i) *Time Derivatives:* When more than one phase is present, in order to avoid confusion, the material time derivative for each phase (α) should be clearly defined, i.e., $D^\alpha(\cdot)/Dt = \partial(\cdot)/\partial t + \mathbf{v}^\alpha \cdot \nabla(\cdot)$. But, in this, and other Simon et al.'s papers, no distinction is made for the material time derivatives, i.e., both the time derivative for solid displacement

\mathbf{u} and the fluid displacement \mathbf{u}^f are denoted by a single super-dot without a clear definition.

- (ii) *The Chemical Potential:* In our triphasic theory, we followed the well-known approach used in the mixture theory formulation to obtain the equations of motion for each phase. In this approach, the driving force for the movement of each of the fluid phases (the water phase and the ion phases) was shown to be the gradient of their chemical potentials, $-\nabla \mu^i$ ($i = w, +, -$). In their paper, without derivation, the concentration-dependent driving forces in the momentum equations for the solid and the fluid are given as $-\nabla \mu^i/T$ ($i = s, f$), where μ^s and μ^f were stated as the chemical potential for the solid and the fluid and T is the absolute temperature [Eq. (11); Simon et al.]. Now, the term "chemical potential" has the dimension of energy per unit volume (or per unit mass) and is a well-established classical concept, used in physical chemistry and in mechanics. Thus, $\nabla \mu^i$ has the dimension of force per unit volume (or mass) whereas $\nabla \mu^i/T$ has the dimension of force per unit volume (or mass) per degree Kelvin. Clearly, Simon et al.'s Eq. (11) is dimensionally inconsistent, and therefore incorrect.

Indeed, in the terminology of irreversible thermodynamics, the entropy production is expressed as a sum of products of flows (J_i) with their conjugated forces; in that context, the *conjugated forces* are $\nabla(\mu^i/T)$ [e.g., see Katchalsky and Curran, *Nonequilibrium Thermodynamics*, p. 79, 1975]. However, this kind of "conjugated force" does not have the dimension of force, and therefore is not to be confused with the force in the momentum equation. We should point out that while in Eq. (11), Simon et al. stated that μ^s and μ^f were the chemical potentials, in fact, the authors later *redefined* their (μ^i/T) in their Eq. (40c) to be related to the chemical potentials of our triphasic theory. Therefore, their "chemical potentials" μ^s and μ^f in authors' Eq. (11) are really not chemical potentials.

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Authors' Response

Following is our response to a Letter to the Editor written by Drs. Lai, Mow, and Gu associated with our paper "A Poroelastic Finite Element Formulation Including Transport and Swelling in Soft Tissue Structures" by B. R. Simon, J. P. Liabale, D. Pflaster, Y. Yuan, and M. H. Krag, ASME JOURNAL OF BIOMECHANICAL ENGINEERING, Vol. 118, 1996, pp. 1–9.