# Linear Elasticity and Potential Theory:
# a Comment on Gurtin (1972)

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**Abstract**

In an exhaustive presentation of the linear theory of elasticity by Gurtin (1972) the author included a chapter on the relation of the theory of elasticity to the theory of potentials. Potential theory distinguishes two fundamental physical categories: divergence-free and divergence-involving problems. From the criteria given in the source quoted by the author it is evident that elastic deformation of solids falls into the latter category. It is documented in this short note that the author presented volume-constant elastic deformation as a divergence-free physical process, systematically ignoring all the information that was available to him that this is not so.

**Introduction**

There is little mention of potential theory in the literature on the mechanics of solids. This author has shown before that in fact there are profound incompatibilities between the theory of stress and deformation on the one hand, and the potential theory on the other (Koenemann 2001a, b). Recently it has come to my attention that I cannot have been the first one to notice this incompatibility.

In 1972 Gurtin published a 300 page presentation of the linear theory of elasticity. One of the chapters in his article is entitled *Elements of Potential Theory* (p.12-24). Several times Gurtin refers to the classical text on potential theory by Kellogg (1929), e.g. in a footnote on p.13 he explicitly mentions chapter IV which is entitled *The Divergence Theorem*. He also quotes Kellogg on p.19 when he explains Newtonian and logarithmic potentials. It appears therefore that that he must have studied Kellogg's book carefully. The unsuspecting reader must come to the belief that there is complete compatibility between Kellogg's source and Gurtin's presentation of it. However, to anyone who is familiar with both texts there is a profound disparity between the two which I document here.

**Contrasts**

Kellogg mentions many details that cannot possibly have escaped Gurtin's attention, such as:

- Kellogg gives the physical interpretation of divergence as a measure of "the work done on/by a field" on a system of mass, e.g. p.52.

- Kellogg writes on p.41, "Newtonian fields are solenoidal [i.e. divergence-free] in free space, ceasing to be so only at points where masses are situated".

- Kellogg repeatedly remarks that the surface $A$ in the divergence theorem must not run through mass for the divergence theorem to be universally valid, i.e. for the expression for divergence to be physically meaningful.

- Kellogg explains volume distributions in chapter I, §8 and §9.

- Kellogg explains the Poisson equation $\nabla^2 U = -4\pi\kappa$ in the case that there is mass in the system initially on p.43, in great detail on p.156, and already prepares for this in the exercises for chapter III, §3 on p.57, #9 and p.58, #11.

- Kellogg explains the conditions under which bodies may be reduced to point sources, and when this is not possible, i.e. when one deals with a *distributed source* (e.g. p.45), the latter in their proper context of the Poisson equation.

- Kellogg mentions the zero potential distance which may be infinite or finite; in the latter case it is by convention set to be unity, but it cannot be zero (p.53 & 63).

None of these points are mentioned by Gurtin. Besides, I must assume that he has knowledge of the fact that quite commonly the condition *div x* = 0 (*x* = some variable) is used both as a condition of orthogonality *and* a condition of equilibrium. This is always

true, of course, for problems involving conservative physics, but it cannot be true for problems for which $div\,x \neq 0$, i.e. for Poisson problems. Physically, the divergence-absent condition is the definition of a conservative process, implying that there is no exchange of energy between a system and its surrounding, neither in form of heat nor in form of work; in contrast, the condition of nonzero divergence implies that the process under study is nonconservative. Such processes may be reversible or irreversible. That is, the conditions $div\,x = 0$ and $div\,x \neq 0$ refer to entirely different energy conservation laws.

As a specialist in continuum mechanics, Gurtin surely knows that Euler used the Laplace condition $div\,\mathbf{v} = 0$ ($\mathbf{v}$ = velocity vector) in his continuity equation as a mass conservation law. Any mass conservation law in conservative physics is also an energy conservation law in the sense of Bernoulli, $E_{kin} + E_{pot} = const$, because of the equivalence of mass and energy in Newtonian physics. But this equivalence does not hold, of course, in thermodynamics where mass and energy are independent variables: there one usually keeps mass constant in order to understand changes of the energetic state, and the respective energy conservation law is the First Law, $dU = dw + dq$. By nature, an elastic deformation requires work to be done by a surrounding upon a system such that an elastic potential builds up.

Despite Kellogg's painstaking effort to distinguish divergence-free cases from non-solenoidal cases, Gurtin never mentions the condition $div\,\mathbf{f} = \varphi$ ($\mathbf{f}$ = some vector field). Instead, on p.24 he writes: "The following well-known proposition on divergence-free and curl-free vector fields will be extremely useful: let $\mathbf{v}$ be a smooth vector field that satisfies $div\,\mathbf{v} = 0$, $curl\,\mathbf{v} = 0$; then $\mathbf{v}$ is harmonic." This sentence appears as a general statement that does not need further justification, inducing in the reader the readiness to rely on the author's authority. In the light of Kellogg's careful systematics and indeed against the intent of the better part of his book, however, Gurtin's seemingly innocuous remark is grossly misleading. Kellogg explains the Laplace condition $\nabla^2 U = 0$ ($U$ = some potential) in great detail in chapter V entitled *Properties of Newtonian Potentials at Points of Free Space*. Then Kellogg devotes chapter VI to the *Properties of Newtonian Potentials at Points Occupied by Masses*, discussing the Poisson condition $\nabla^2 U = \varphi$. Even if a reader has no prior knowledge of potential theory one would assume – as it happened to me – that such a chapter must exert irresistible attraction to anyone's attention with a particular interest in the mechanics and physics of solids. Gurtin manages to ignore it completely. Such consistent and apparently systematic omissions of substantial and important parts of Kellogg's text make it likely that this was done with intent. Gurtin writes

on p.45: $\mathbf{S}_n = -\mathbf{S}_{-n}$ ($\mathbf{S}$ = stress vector),

on p.49: $div\,\mathbf{S} + \mathbf{b} = 0$ ($\mathbf{S}$ = stress vector field, $\mathbf{b}$ = body force vector)

on p.53: In the absence of body forces the equations of equilibrium take the form $div\,\mathbf{S} = 0$.

Gurtin must have realized – in the light of Kellogg's physical interpretation of divergence as a measure of the work done upon/by a system – that the condition $div\,\mathbf{S} = 0$ is a mathematically exact form of the assertion that during a volume-constant elastic deformation no work is done upon the system, implying either that deformation

of a solid is a conservative physical process like celestial mechanics, or else that the deformation can be achieved without doing work.

I cannot believe that Gurtin has missed the significance of the zero potential distance, and he must have noticed that no such distance is to be found in continuum mechanics – thanks to Cauchy's flawed derivation of the stress tensor (Koenemann 2001a). Continuum mechanics is incompatible with thermodynamics because this distance, missing in continuum mechanics, is in thermodynamics the radius of the thermodynamic system.

Gurtin cannot have missed Lemma 1 in Kellogg, p.147 which states that "the integral

$$\iiint f(Q)dV$$

is convergent if and only if it approaches 0 with the maximum chord of $V$". (The lemma already appears on p.21 in an exercise to chapter I, §9.) Under the condition stated in the headline the integral has a nonzero value. To anyone with standard skills in this field it must be clear that this most fundamental existence theorem is squarely at variance with the assumptions made for the derivation of the Cauchy stress tensor (Koenemann 2001a). $\int f(Q)\,dV = \kappa$ is known as the charge (see, for example, Feynman's lectures). Of necessity, the charge is zero only in the unloaded state, which is not of much interest in elasticity.

Gurtin must have realized that deformation of solids involves homogeneously distributed mass, that a solid represents a distributed source, and such problems are always Poisson problems; that is: the Laplace condition does not apply, but any differential approach to continuum mechanics must be based on the Poisson condition $div\,\mathbf{f} = \varphi$. It follows from Kellogg that distributed masses cannot be dissolved into a continuum of point sources – this is precisely the intent of Cauchy's flawed continuity approach that is used to derive this tensor – but that energy and work must be considered *per unit volume or unit mass*, i.e. in the sense of a thermodynamic system. And when Gurtin explains the properties of logarithmic potentials directly referring to Kellogg, it is inconceivable that he did not recall the logarithmic nature of thermodynamic potentials, or did not notice the nature of the examples given by Kellogg to illustrate where such potentials are encountered.

Gurtin must have noticed that the one and only expression in Kellogg referring to work is the divergence. The expression he gives for elastic work, i.e. the elastic potential (Gurtin 1972:85),

$$\varepsilon(\mathbf{E}) = \mu|\mathbf{E}|^2 + \frac{k}{3}(tr\mathbf{E})^2$$

is squarely at odds with potential theory: the only term that is compatible with the latter is $tr\,\mathbf{E}$, but it is by definition zero for a volume-constant deformation. The entire derivation of this expression (which Gurtin does not show) presupposes the existence of the Cauchy stress tensor – which is unsuited to derive a work term; in its derivation the condition $tr\,\mathbf{E} = 0$ is of critical importance (it is used as an equilibrium condition, but the wrong one), and the zero potential distance was allowed to vanish identically (Koenemann 2001a, b); but without this zero potential distance it is impossible to define work. The term $tr\,\mathbf{E}$ is fortuitous – spurious by derivation, correct by coincidence, and

by itself insufficient for consideration of a general deformation because, as can be learned from Kellogg, but has been ignored by Gurtin, any surface of a thermodynamic system within a solid must necessarily run through mass, thereby limiting the generality of the Gauss divergence theorem. The term $tr\,\mathbf{E}$ therefore can *not* be taken as a measure of the *complete* work done on/by the system, i.e. by itself it is physically meaningless.

Gurtin must have seen the method how to derive a vector field from a potential: if $U$ is some scalar field, $\partial U/\partial x_i = v_i$ is a vector field, and $\partial^2 U/\partial x_i \partial x_j = \mathbf{F}$ is the field property tensor; but he never refers to it. He must be aware that $\mathbf{f} = m\mathbf{a}$ is not a field force, but a single force vector, it cannot form a field, and it cannot be derived; Sokolnikoff (1964) calls $\mathbf{f} = m\mathbf{a}$ an *underived force*. Instead, all force vector fields must be derived from a potential, $\mathbf{f} = \mathbf{e}_i \partial U/\partial x_i$, but this has never been done in continuum mechanics. In fact, upon careful reading it becomes apparent that all of vector fields in Gurtin (1972) are asserted, never defined. In his textbook (Gurtin 1981) he is more circumspect, he only writes about *systems of forces*, studiously avoiding the treacherous term *field*. Technically, this is only correct, because due to all its shortcomings, first of all the absence of the zero potential distance, current continuum mechanics is not a field theory in the way this term is commonly understood.


**Conclusion**

I cannot believe that Gurtin failed to recognize the relevance of the divergence theorem to the stress theory and continuum mechanics. He must have noticed the profound incompatibility of the theory of elasticity with the theory of potentials around 1970 when he wrote the article for the *Handbuch der Physik*. Given the wealth of information that can be found in Kellogg on distributed mass problems, conscious effort must have been invested in order not to refer to it.

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