Miscellaneous works by Francesco dell'Isola (2010-2013)

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Abstract

In this document are gathered: i) all the reviews authored by Francesco dell'Isola appeared in Mathematical Reviews (MathSciNet) in the period 2010-2013 and ii) his three laudationes in honor of Professor Del Piero and Professor Di Carlo.

Review of paper: Iovane G., Nasedkin A.V. Modal analysis of piezoelectric bodies with voids. I. Mathematical approaches. Appl. Math. Model. 34 (2010), no. 1, 60-71

The paper starts by considering a particular case of the equations for piezoelectric bodies with voids quoted in [M. Ciarletta and E. Scarpetta, Mech. Res. Comm. **23** (1996), no. 1, 1–10]. The sense of physical assumptions adopted by the authors is not clear: for example, no explanation is given for the missing terms in the constitutive equations used. Similarly, boundary conditions are introduced without specifying if they were originally deduced (in [op. cit.] or elsewhere) using a variational principle. Subsequently, in Section 3, the eigenvalue problem presented in Section 2 is formulated in a variational form. Proofs of Theorems 3.1 and 3.2 are not presented since they "completely repeat well-known proofs" available in [S. G. Mikhlin, *Variational methods in mathematical physics*, Translated by T. Boddington, Macmillan, NewYork, 1964]. After a technical Section 4, where the results in [A. V. Belokon' and A. V. Nasedkin, Prikl. Mat. Mekh. **60** (1996), no. 1, 151–158] are invoked, in Section 5 some relationships between eigenfrequencies of the eigenproblems considered are found. The paper concludes with a few considerations of a physical nature.

Review of paper: Sobh Ayman Mahmoud. Heat transfer in a slip flow of peristaltic transport of a magneto-Newtonian fluid through a porous medium. Int. J. Biomath. 2 (2009), no. 3, 299-309

From the text: "We study the interaction of peristalsis with heat transfer for the flow of a viscous fluid through a porous medium in uniform and non-uniform channels. The flow is subjected to a constant transverse magnetic field.

"No investigation has been made yet to analyze the peristaltic flow with heat transfer through a porous medium when the no-slip condition is inadequate."

The paper presents some closed-form solutions for well-known evolution equations when no-slip boundary conditions are imposed. Biomechanical implications of the results found are not discussed.

Review of paper: Podio-Guidugli Paolo, Vianello Maurizio. Hypertractions and hyperstresses convey the same mechanical information. Contin. Mech. Thermodyn. 22 (2010), no. 3, 163-176

This article contains a description of the theory of second gradient continua, a theory which is attracting increasing interest. This is accounted for by the large number of researchers who are using it for modelling a variety of phenomena ranging from capillarity or phase transition, to plasticity or flow in porous media. The final recognition of the importance of P. Germain's paper [J. Mécanique 12 (1973), 235–274] — which concludes and systematizes the works by R. A. Toupin [Arch. Rational Mech. Anal. 17 (1964), 85–112], R. D. Mindlin [Internat. J. Solids Structures 1 (1965), no. 4, 417–438] and P. Casal [C. R. Acad. Sci. Paris Sér. A 274 (1972)] — fully justifies its revival. However, such a revival should add some new results to the theory, either clarifying the underlying physical phenomenology or somehow developing the mathematics used. This is not the case in the paper under review: indeed in it, (i) while Paul Germain paid careful attention to introducing a physically meaningful nomenclature for designating the different types of contact actions, here they are all mixed up in a less meaningful "hyper-" designation; (ii) the classification of second gradient materials which do not exert contact edge forces and boundary conditions to be assigned in the framework of second gradient continuum models is claimed to be novel, while instead it has been well known for a long time [see, e.g., F. dell'Isola and P. Seppecher, Meccanica 32 (1997), no. 1, 33–52]; and (iii) instead of using the most suitable, complete and original (multi-index) version of Levi-Civita absolute calculus, the authors try to use a less suitable index-free notation which is well adapted only in contexts where only second-order tensors suffice. In addition, the validity of the Principle of Virtual Powers, not only for the whole continuous body considered but also for all its subbodies, is already explicitly postulated in the work of E. Cosserat and F. Cosserat [Théorie des corps déformables, Hermann, Paris, 1909]. There is no reason for the authors to refer to a strengthened or generalized version of the Principle of Virtual Powers (or Virtual Works) since a word-for-word statement of it can be found, for instance, in the textbook [Mécanique des milieux continus. II, Ellipses, Paris, 1988] of J. Salençon at the École Polytechnique (Paris Tech). On the other hand, the claim that, in the literature, there is no available treatment of second gradient continuum mechanics "à la Cauchy" (i.e., an approach which is starting from the concept of contact actions and is able to deduce the general form of internal powers with a "tetrahedron" argument) is not accurate: such a result has been obtained by dell'Isola and Seppecher [op. cit.], as recognized for instance by G. A. Maugin in his review of that paper, or by S. Forest et al. [Mécanique des milieux continus, Ecole des mines, Paris, 2005–2006; per revr.].

Review of paper: Madeo A., Gavrilyuk S. Propagation of acoustic waves in porous media and their reflection and transmission at a pure-fluid/porous-medium permeable interface. Eur. J. Mech. A Solids 29 (2010), no.5, 897-910

In this paper the set of balance equations proposed by M. A. Biot [J. Acoust. Soc. Amer. **28** (1956), 168–178; J. Acoust. Soc. Amer. **28** (1956), 179–191] and Coussy [O. Coussy and T. Bourbie, Rev. Inst. Fr. Pétrole **39** (1984), no. 1, 47–66; O. Coussy, *Poromechanics*, Wiley, Chichester, 2004] to model solid deformation and fluid flow in porous media is particularized to get the PDEs governing one-dimensional, quasi-linear compression wave propagation. In the spirit of K. O. Friedrichs and P. D. Lax [Proc. Nat. Acad. Sci. U.S.A. **68** (1971), 1686–1688], S. K. Godunov and E. I. Romenskii [*Elements of continuum mechanics and conservation laws*, translated from the 1998 Russian edition by Tamara Rozhkovskaya, Kluwer/Plenum, New York, 2003] and A. Jeffrey [*Quasilinear hyperbolic systems and waves*, Pitman Publishing, London, 1976], a sufficient condition for hyperbolicity is proven for considered one-dimensional PDEs in terms of the Hessian matrix of the Eulerian deformation energy density, and a range is determined for fluid and solid velocities corresponding to which wave propagation necessarily occurs. This result is of relevance in those applications where a small periodic perturbation is superimposed onto a stationary fluid flow, as e.g. in geotechnical and petrol engineering. Once the linearized equations for these perturbations are formulated, the problem of wave reflection and transmission at a discontinuity surface between a pure fluid phase and a porous fluid saturated solid matrix is addressed. To get transmission and reflection coefficients, the bulk and surface balances of mechanical energy are deduced from considered equations and boundary conditions. A novel expression for these coefficients in terms of the eigenvalues and eigenvectors of the matrices of the coefficients in the governing system of linear PDEs and its judicious non-dimensional formulation allows the authors to reveal a threshold phenomenon: the set of Biot elasticity coefficients can be partitioned into two subsets, in which the greatest amount of wave energy is transported by "fast" Biot wave and by the "slow" Biot wave, respectively. The theoretical results presented seem to give a promising foundation of an efficient indirect method for experimental determination of the Biot coefficients.

Review of paper: Whittaker Robert J., Heil Matthias, Jensen Oliver E., Waters Sarah L. A rational derivation of a tube law from shell theory. Quart. J. Mech. Appl. Math. 63 (2010), no.4, 465-496

This is an interesting paper in which the authors seek to deduce a "tube law", i.e., a relationship between transmural pressure and all relevant geometric and

mechanical parameters of an elastic tube in which a fluid flow occurs. Indeed, when studying fluid flow in a deformable tube one may use a 1D model based on the conservation of mass and on Poiseuille's law (or another friction model) in which the three unknowns are the fluid flow velocity, the internal pressure and the tube area A. Therefore, "A third equation, commonly known as a 'tube law', is required to capture the wall mechanics in order to link A with the internal pressure and thus close the system."

It is clear that more refined theories could be used to describe the phenomena considered. In the paper under review the authors state that "we formally derive a tube law from shell theory that is valid for small-amplitude long-wavelength deformations of a thin-walled elliptical tube and incorporates the leading-order effects of azimuthal bending and axial curvature". The analysis performed and the results obtained are sound and correct. The authors are entitled to claim that "our systematic approach also shows which terms must be neglected to arrive at the simple tube law and thus provides a means of understanding the regime in which such a tube law will hold together with estimates of the likely errors for a given set of parameter values.Within the region of validity, the tube law enables us to capture the predictions of shell theory in a remarkably simple form".

Indeed, the investigation presented supplies the needed criteria for establishing the range of validity of the aforementioned 1D model and for requiring when, instead, the more detailed theory of shells needs to be used. The tube law is deduced by means of a pseudo-spectral method based on a Fourier expansion of the solutions of the field equations obtained by means of shell theory. Subsequently, the theoretical tube law is used to derive solutions for two simple cases (uniform and linearly varying transmural pressure applied to a finite-length tube with rigid supports), and its predictions are compared with some numerical simulations based on shell theory. The results presented will most likely originate some interesting mathematical investigations, from the proof of asymptotic convergence of the solutions arising when using shell theory to the solutions of the 1D problem which includes the tube law. To comment on the relevance of results presented in the applications one can agree with the authors when they state: "The tube law [which we obtain] has a form that has commonly been used in previous work, but our derivation allows the calculation of the coefficients and also provides further insight into when the law might break down and how it could be refined further."

Review of paper: Sharma M.D. Piezoelectric effect on the velocities of waves in an anisotropic piezo-poroelastic medium. Proc. R. Soc. Lond. Ser. A Math. Phys. Eng. Sci. 466 (2010), no. 2119, 1977-1992. 1471-2946

In this paper the model introduced by Biot for the motion of a fluid in a porous deformable matrix is completed by means of electrostatics equations for

electric displacement and coupled piezoelectric constitutive equations. Indeed, the author assumes that two different electric (macroscopic) displacements can be defined for the fluid and solid constituent, and postulates that these two displacements are separately divergence free (electrostatics assumption). These assumptions may be questioned from a physical point of view, as they strongly "uncouple" the electric potential in the fluid component from the electric potential in the solid component: it could be very important to establish when they are applicable. Subsequently the author attacks the huge amount of algebraic calculations needed to study plane wave propagation in the framework of the considered model. In section 5 the author claims the following: "Due to the non-availability of real data, a hypothetical numerical model is chosen to explain the variations of velocities. Numerical results from such a particular model may not carry any quantitative significance and hence cannot be generalized. However, a qualitative analysis of these results may certainly be useful in understanding the wave-induced piezoelectric character of composite materials." It is a pity that the author does not try to get, from his analytical results and numerical simulations, a method for establishing a measurement procedure to determine at least some of the constitutive parameters introduced in the considered model: in a sense all numerical models are "hypothetical" and need to be used to find the values of introduced constitutive coefficients for each particular material. Later the author claims, "The APP medium being a two-phase medium, the separation of piezoelectric effect due to each of its constituents may be interesting. To explore the piezoelectric response of the solid frame and pore-fluid, the variations of the phase velocities and ray velocities with phase direction are presented." The following analysis is rather interesting and precise; however, the author should underline clearly that the aforementioned "uncoupling" assumption greatly influences the separation of piezoelectric effect occurring in each constituent. The results and conclusions seem correct and are interesting; particularly remarkable are the comments about the influence of the fluid density or piezoelectric activeness on wave velocity.

Review of paper: Golden J.M. Phase transitions in materials with thermal memory: the case of unequal conductivities. Phys. D. 238 (2009). no. 4, 428-448

The paper addresses an interesting problem: to introduce at the interfacial zone between different phases Cattaneo delay in heat flux constitutive equations. The author refrains from citing the enormous number of papers dedicated to the modelling of interfacial zones and limits himself to [J. M. Golden, Phys. D **237** (2008), no. 19, 2499–2516; E. Fried and M. E. Gurtin, Phys. D **68** (1993), no. 3-4, 326–343; Phys. D **91** (1996), no. 1-2, 143–181], which do not completely represent the whole set of results already available. The name of C. Cattaneo [Atti Sem. Mat. Fis. Univ. Modena **3** (1949), 83–101] is not even cited. Concerning the literature about continuum modelling of interfacial zones

we refer, e.g., to [T. Alts and K. Hutter, J. Non-Equilib. Thermodyn. **13** (1988), no. 3, 221–280; J. Non-Equilib. Thermodyn. **13** (1988), no. 4, 301–329; J. Non-Equilib. Thermodyn. **14** (1989), no. 1, 1–22; F. dell'Isola and W. Kosiński, Arch. Mech. (Arch. Mech. Stos.) **45** (1993), no. 3, 333–359; P. Seppecher, Ann. Phys. **13** (1988), 13–22; per revr.; R. Gatignol and P. Seppecher, J.Méc. Théor. Appl. (1986), suppl., 225–247;], and to all references there cited. In the present paper, although not clearly stated, one can find some results which may be of interest: from the abstract, "these involve the jump in temperature across the transition zone and the normal derivatives of the temperature on each phase boundary, which are related to the velocity of the transition zone and a latent heat dependent on this velocity, as well as the speeds of thermal disturbances in the two phases". However, the author refrains from using a unique constitutive equation for both phases, as he does not introduce second gradient models: instead, he rather artificially introduces an "order parameter" which delineates the interfacial zone. The physical and mathematical basis for such an approach may be considered controversial [see, e.g., G. Bouchitté, C. Dubs and P. Seppecher, C. R. Acad. Sci. Paris Sér. I Math. **323** (1996), no. 9, 1103–1108]. The asymptotics and "averaging along the thickness" in the interfacial zone proposed by the author need to be completed to lead to some more applicable results.

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Review of paper: Budaev Bair V., Bogy David B. A self-consistent acoustics model of interface thermal resistance. SIAM J. Appl. Math. 70 (2009/10), no. 5, 1691-1710

This paper shows that "heat-carrying acoustic waves must obey the introduced principle of heat radiation instead of the Sommerfeld radiation condition used in acoustics". The analysis developed allows the authors to "derive an equation of compatibility that connects the heat flux and the temperature on both sides of the interface". The presentation is limited to the study of one-dimensional wave propagation. However, one may agree with the authors when they state that: "Our model of heat conductance by acoustic waves is frequency dependent, and it can be used to analyze conductance by acoustic waves of any frequency band." [. . .] "It also explains why the interface thermal resistance between sharply contrasting materials is better approximated by the diffuse mismatch model than by the conventional acoustic mismatch model." Moreover the thermal resistance computed by means of the model presented "appears to be in line with expectations". The promising results presented show that the three-dimensional analysis must be attempted. Indeed, the authors state that "Although the one-dimensional model cannot be directly compared with experimental data, it is nevertheless remarkable that the conventional acoustic model overestimates the measured thermal resistance in approximately the same ratio as it overestimates the resistance R_k computed by our model." The promised separate paper focused on three-dimensional problems may represent remarkable progress in the field.

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Review of paper: Ammari Kaïs and Nicaise Serge. Stabilization of a piezoelectric system", Asymptot. Anal. 73 (2011), no.3, 125-146.

Summary: "We consider a stabilization problem for a piezoelectric system. We prove an exponential stability result under some Lions geometric condition. Our method is based on an identity with multipliers that allows to show an appropriate observability estimate."

The reviewer could not find explicitly stated in the paper which stabilization (stabilization is the act of stabilizing something or making it more stable,

obviously here in a mathematical sense) problem for the considered piezoelectric system is solved by the authors. Indeed more generally in the whole paper the reader does not find any discussion about the problems solved and their importance in the applications. The function Q introduced in Equation (1.7) probably is relevant in this context as one may deduce from the sentences which can be read at the end of page 127 and the beginning of page 128. The reader is also left without any suggestion about the meaning and relevance of the boundary conditions introduced in Equations (1.7.5) and (1.7.6). Moreover, have these boundary conditions already been studied and justified elsewhere? What is their possible physical meaning? Are they considered by other authors as sources of mathematical problems? Can the function Q be chosen to stabilize the considered physical system? How should one eventually interpret this choice? The results presented in section 2 prove that the boundary value problem formulated in section 1 is well-posed for a large class of functions Q. The arguments used seem to be correct: however, the authors do not underline where their proof confronts a serious difficulty or departs from those results already obtained in the literature which they so often cite in their presentation. In section 3 the authors choose a particular subset of the set of functions Q considered in section 2 and prove an exponential stability result. Although remarks 3.2 and 3.4 are somewhat obscure, the arguments presented in this section seem to be correct. The paper ends without a conclusion section: the authors perhaps should have shown explicitly the most delicate parts of their reasoning, and underlined where they are novel and why they are relevant for applications.

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Review of paper: Noll Walter, Seguin Brian. Basic concepts of thermomechanics. J. Elasticity 101 (2010), no.2, 121-151. 1573-2681

This work is written to be a "blueprint" for a possible textbook on continuum thermomechanics.

In the Introduction the authors claim that their proposed textbook would differ from most existing textbooks because of some features which are listed in the Introduction. We start by quoting these features with some comments.

"(1) It uses the mathematical infrastructure based on sets, mappings, and families, rather than the infrastructure based on variables, constants, and parameters. (2) It is completely coordinate-free and $\mathbf{R}^n$-free when dealing with basic concepts."

The need to "embed" the physical theories into a "set theoretic" framework has been a continual guiding idea for the authors. However, it is not clear why the sets one needs for developing thermodynamics and mechanics cannot be assumed—from the very beginning—to be subsets of the set of the n-tuples of real numbers or subsets of some suitable set of real-valued functions of real variables. Is the formalism introduced by the authors really needed to describe the complexity of some phenomena? The Appendix of the paper is a simple reproduction of some pages of a standard (modern) textbook in mathematical analysis or linear algebra: it is pervaded by a form of "Bourbakism" which in the reviewer's opinion is not useful in the present context. It is not clear why it is desirable to have a formal foundation of mechanics which is absolutely "coordinate-free" or "$\mathbf{R}^n$-free": there are aspects of this foundation in which one encounters fewer difficulties when using n-tuples of numbers.

"(3) It does not use a fixed physical space. Rather, it employs an infinite variety of frames of reference, each of which is a Euclidean space. The motivation for avoiding physical space can be found in Part 1, entitled 'On the illusion of physical space', of the booklet [W. Noll, *Five contributions to natural philosophy*, 2004, available at www.math.cmu.edu/ ∼wn0g/noll]. Here, the basic laws are formulated without the use of a physical space or any external frame of reference. (4) It considers inertia as only one of many external forces and does not confine itself to using only inertial frames of reference. Hence kinetic energy, which is a potential for inertial forces, does not appear separately in the energy balance equation."

In classical mechanics the axiomatics of space and time can be founded equivalently on (i) the assumption of the existence of a "physical space" or on

(ii) suitable invariance requirements for changes of coordinate systems. The reader interested in a clear and exhaustive treatment on this point may refer to the textbook which is the masterpiece in the field, V. I. Arnol'd's *Mathematical methods of classical mechanics* [(Russian), Izdat. "Nauka", Moscow, 1974; translated from the Russian by K. Vogtmann and A. Weinstein, Springer, New York, 1978]. The point of view assumed in the paper under review reproduces nearly exactly, but with more formalistic complications, the clear presentation found there.

The footnote at the beginning of Section 2 is rather surprising: the observations there contained inspired Euler, d'Alembert, Lagrange, Maxwell and many others.

The content of Section 2 comes exactly from [W. Noll, Arch. Rational Mech. Anal. 52 (1973), 62–92] and [W. Noll and B. Seguin, Int. J. Pure Appl. Math. 37 (2007), no. 2, 187–202] and presents some rephrasing of well-known results from set theory, including De Morgan formulas.

Section 3 presents in a Bourbakistic way results which are presented in every textbook of mechanics: the formalism used was first presented in several papers by Noll.

In Section 4, after recalling the definitions introduced in [W. Noll and E. G. Virga, Arch. Rational Mech. Anal. **102** (1988), no. 1, 1–21], the authors also formulate some results about isometries. In a different language these results are exactly those one can find, e.g., in the cited textbook by Arnol'd. Then the authors introduce the set of configurations and transplacement for a continuous body in a very formal way: however, exactly as it is accepted generally, configurations and transplacement are finally specified by means of two times continuously differentiable invertible functions (diffeomorphisms) defined in subsets of $\mathbf{R}^3$ and taking values in subsets of $\mathbf{R}^3$. The general impression is that the authors want to spell out exactly some concepts which are considered implicitly in every standard presentation of continuum mechanics. Subsequently the authors recognize, as is done more or less explicitly in any textbook, that a continuous body is a Riemannian submanifold of a three-dimensional Euclidean space and formalize—as is usually done—the concept of "infinitesimal body element" and its deformation. Finally, they introduce the concept of subbody. Some results, which rephrase well-known results from set theory, are again reconsidered, to recognize that the abstract concept of subbody which the authors prefer to use actually reduces to the one which is usually accepted. The authors repeat that all these proofs are highly nontrivial. This statement is correct, as these results are the basis of the modern Theory of Sets, and can be found in any textbook of this theory.

Section 5 is a section in which the authors want to explicitly declare that in the past the first of them introduced a notation which could be considered misleading. More results about isometries, which are more clearly presented, e.g., in [V. I. Arnol'd, op. cit.], are recalled: the authors implicitly use the most important property of Euclidean three-dimensional spaces, i.e. that they are isomorphic to $\mathbf{R}^3$. This property makes understandable and logically correct both Sections 4 and 5 and in particular the crucial definition preceding equation

(4.3). On the other hand the existence of these isometries is accepted and used right away in many textbooks (for instance in Arnol'd's) so obtaining a great economy of thought: on the contrary the notations used by the authors— instead of being helpful—become cumbersome and distracting. Indeed, one should never forget the crucial role in differential geometry played by charts.

Section 6 is a simple rephrasing of a part of the set-theoretic introduction in a textbook of mathematical analysis while Section 7 starts with the fundamental distinction between spatial and material fields, formulated, this time, with a notation which is only slightly more complicated than the usual one. However, when introducing motions, velocity fields and spatial velocity gradients the notation becomes once more very difficult for somebody who has not already understood the presented concepts.

Section 8 starts by rephrasing standard definitions and results from real analysis. Then the concept of "contactors" appears: it is simply a very formal way to talk about Cauchy-type contact interactions. Theorems 4, 5 and 6 are trivial consequences of important results from Lebesgue's Theory of Measure. Protocontractors are discussed in a remark which ignores all investigations about generalized continua which are accounted for, e.g., in [G. A. Maugin and A. V. Metrikine (eds.), *Mechanics of generalized continua*, Adv. Mech. Math., 21, Springer, New York, 2010].

Also, Sections 9 and 10 ignore all literature referred to in [G. A. Maugin and A. V. Metrikine, op. cit.] and consist in the formalistic presentation of the balance of forces and torques in Lagrangian and Eulerian description. It actually reduces to the one presented in all textbooks, once the complex notation is decrypted. A citation of a famous statement by Archimedes about levers appears here. The reviewer disagrees with the authors: as it has been clearly proven by L. Russo [*The forgotten revolution*, translated from the 1996 Italian original by Silvio Levy, Springer, Berlin, 2004], Archimedes had a deep understanding of the mathematical structure of mechanics, and he mastered perfectly the principle of virtual work. Deformation processes and mechanical processes are then considered and the very particular form of balance of powers valid for Cauchy continua is obtained.

In Section 11 (which the reviewer found very difficult to understand) and Section 12 the balance of energy and entropy is presented in a form which is valid only for Cauchy continua. One has to remark that when considering generalized continua the methodology presented here needs to be completely changed [see, e.g., P. Germain, J. Mécanique 12 (1973), 235–274; SIAM J. Appl. Math. **25** (1973), no. 3, 556–575; A. E. Green and R. S. Rivlin, Arch. Rational Mech. Anal. **17** (1964), 113–147; Arch. Rational Mech. Anal. **16** (1964), 325–353]. It is questionable whether in a textbook of the type conceived by the authors such a limitation should be accepted.

In Section 13, entitled "Constitutive laws and the second law of thermodynamics", the authors only formulate the statement (due to Coleman and Noll) about the nature of the constitutive equations which can be considered compatible with the second principle of thermodynamics. All relevant and important difficulties and results regarding this statement are not mentioned: therefore

we do not know how the authors would present them to the reader of their textbook.

In Section 14 the principle of material frame indifference and the following surprising statement can be found: "In the literature on continuum mechanics it is often implicitly assumed that the frame of reference being used is inertial so the formula (14.6) is valid. However, when the frame of reference is not inertial then (14.6) is not valid and the concept of kinetic energy is not useful."

In the reviewer's opinion the textbook planned by the authors would be very mathematically complicated and formal; however, its scope would be rather limited, as the presented theoretical frame is not able to encompass generalized continua, the importance of which is generally accepted in the literature.

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Review of paper: Amendola Giovambattista, Fabrizio Mauro, Golden Murrough.Thermodynamics of a non-simple heat conductor with memory. Quart. Appl. Math. 69 (2011), no. 4, 787-806. 1522-4485

The paper under review deals with the problem of generalizing Cattaneo's heat flux constitutive law [C. Cattaneo, Atti Sem. Mat. Fis. Univ. Modena 3 (1949), 83–101].

Actually, as stated by the authors in the Introduction: "We consider a new constitutive equation with memory for the heat flux, which includes not only the effects of the history of the temperature gradient g, but also the history of $\nabla \cdot \nabla g$." For what concerns the physical motivation of the generalization considered the authors refer to [R. A. Guyer and J. A. Krumhansl, Phys. Rev. **148** (1966), no. 2, 766–778], where by using kinetic theory a generalization of Cattaneo and Maxwell's equation containing new higher spatial gradient terms was obtained. The introduction continues by stating, "This choice yields a non-simple material, for which the classical Second Law of Thermodynamics, expressed by the Clausius-Duhem inequality, must be modified by means of a different definition of internal entropy power or by considering the presence of an extra flux." The need to introduce an "ad hoc" extra flux, whose physical meaning remains obscure, is peculiar to those approaches—such as the one chosen by the authors—which refrain from the use of a variational formalism. Actually this formalism can be used also in the context of heat flux phenomena: see, e.g., [B. D. Vujanović and S. E. Jones, *Variational methods in nonconservative phenomena*, Math. Sci. Engrg., 182, Academic Press, Boston, MA, 1989] (and the

references in its Chapter 5), where the heat equations resulting from Cattaneo's constitutive law were obtained from a variational principle. The authors accept without comment the inequality (3.1) and introduce the "ad hoc" relation (4.8) relating the extra flux to the already introduced constitutive equations for heat flux without any physical or logical justification. The sentence on page 793, "The inequality $(4.9)_1$ is equivalent to the representation (4.11) by virtue of an extra flux $\Phi(t)$, which for a local material is zero, while for non-local systems depends on the material", does not clarify the point of view of the authors concerning this crucial issue. In the reviewer's opinion the authors assume an attitude very similar to the one shown in [J. E. Dunn and J. B. Serrin Jr., Arch. Rational Mech. Anal. **88** (1985), no. 2, 95–133], where instead of using a correct form of the Principle of Virtual Works, suitable for second gradient materials, the authors also prefer to introduce an "ad hoc" extra flux without any physical or logical justification, except the cogent need of writing an entropy inequality which could allow for the dependence of deformation energy on strain gradients. Sections 4 and 5 of the paper under review are an interesting effort to overcome the difficulties discussed, an effort which surely will deserve attention.

A final remark is needed here: The authors do not indicate which kind of boundary conditions are physically meaningful and mathematically consistent with the bulk equations which result from their constitutive assumptions. This is not surprising as—without the guide of a variational principle—this issue becomes a very difficult challenge.

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Review of paper: Groves M.D., Wahlén E. On the existence and conditional energetic stability of solitary gravity-capillary surface waves on deep water.
J. Math. Fluid Mech. 13 (2011), no.4, 593-627. 1422-6952

This paper is a beautiful and original work in applied mathematics. The presentation is clear, rigorous and formal: the authors manage to be mathematically precise while also describing the physical meaning of their assumptions and results. The demonstrations seem correct and their presentation does not have any unclear points. The paper starts by proving an existence and stability theorem for gravity-capillary solitary waves on the surface of a body of water of infinite depth. Based on a classical variational principle, the existence of a minimizer of the wave energy $\mathcal{E}$ is proven subject to the constraint that the wave momentum $\mathcal{J}$ is fixed and given in terms of a positive amplitude parameter much smaller than 1. Subsequently, as $\mathcal{E}$ and $\mathcal{J}$ are both conserved quantities, by means of a standard argument the stability of the set of minimizers is established: one is assured that—once a suitable energy space is considered—a solution starting near an energy minimizer remains close to being an energy minimizer over its whole interval of existence. The mathematical results thus obtained are used in order to compare the model for considered solitary waves based on the previously used variational principle and the other one often accepted in the applied mechanics literature which, instead, uses the nonlinear Schrödinger equation with cubic focussing nonlinearity. Actually the authors prove in an elegant way that the waves which are the minimizers of the wave energy $\mathcal{E}$ converge (after the appropriate rescaling) to the solutions of the abovementioned Schrödinger equation as the amplitude parameter tends to zero. A concluding remark: the well-posedness assumption at the end of page 597 may have deserved a more detailed discussion.

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Review of paper: Migórski Stanislaw, ochal Anna, Sofonea Mircea. Analysis of a quasistatic contact problem for piezoelectric materials. J. Math. Anal. Appl. 382 (2011), no.2, 701-713

Summary: "We consider a mathematical model which describes the frictional contact between a piezoelectric body and an electrically conductive foundation. The process is quasistatic, the material behavior is modeled with an electro-viscoelastic constitutive law and the contact is described with subdifferential boundary conditions. We derive the variational formulation of the problem which is in the form of a system involving two history-dependent hemivariational inequalities in which the unknowns are the velocity and electric potential field. Then we prove the existence of a unique weak solution to the model. The proof is based on a recent result on history-dependent hemivariational inequalities obtained in [S. Migórski, A. Ochal and M. Sofonea, Nonlinear Anal. Real World Appl. **12** (2011), no. 6, 3384–3396]."

This abstract describes rather accurately the content of the present paper, the technical details of which cannot be fully understood without reading some of the authors' earlier papers to which they refer to repeatedly.

Then in the introduction one can find the following statement: "The present paper represents a continuation of [Z. Lerguet, M. Shillor and M. Sofonea, Electron. J. Differential Equations **2007**, No. 170, 16 pp.; S. Migórski, A. Ochal and M. Sofonea, J. Math. Anal. Appl. 361 (2010), no. 1, 161–176] and is devoted to the study of a new mathematical model which describes the frictional contact between an electro-viscoelastic body and a conductive foundation. With respect to [Z. Lerguet, M. Shillor and M. Sofonea, op. cit.] the novelty of the model consists in the fact that the contact, the frictional and the conductivity conditions are modeled with the subdifferentials involving nonconvex functionals. With respect to [S. Migórski, A. Ochal and M. Sofonea, op. cit., 2010] the novelty of the model consists in the fact that here the process is quasistatic and, in addition, the elasticity operator involved in the constitutive law is nonlinear and could be time-dependent."

In the reviewer's opinion the mathematical model presented in the present and in the cited papers is indeed rather standard.

The introduction then continues as follows: "Therefore, the arguments on evolutionary hemivariational inequalities used in [S. Migórski, A. Ochal and M. Sofonea, op. cit., 2010] do not work in this case. Our interest in this paper is twofold. First, to describe a physical process in which contact, friction and piezoelectric effects are involved and to derive a consistent mathematical model to this process." In the paper the reader will not find such a "derivation" from basic physical principles or any description of the physical process which the presented model intends to describe. Instead, this model is simply formulated in a formal way.

Immediately subsequent to the aforementioned statements one can read about the second aim declared by the authors: "to illustrate how our recent existence and uniqueness result on history-dependent hemivariational inequalities, obtained in [S. Migórski, A. Ochal and M. Sofonea, op. cit., 2011], can be used to provide the unique solvability of this mathematical model". Indeed the authors present the announced proof in section 5. This proof consists of a series of careful determinations of upper bounds, which can be deduced by correctly applying the assumed (apparently very standard) hypotheses.

The paper actually is an interesting application of powerful analytical methods to prove an existence and uniqueness theorem of a problem of interest in the electrodynamics of deformable piezoelectric bodies in frictional contact with a conductive body.

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Review of paper: Piccolroaz A., Mishuris G., Movchan A.B. Perturbation of mode III interfacial cracks. Recent progress in the mathematics of defects, 41-51, Springer, Dordrecht, 2011

The results presented in this paper are extensively used in a later paper in the same collection [G. S. Mishuris, N. V. Movchan and A. B. Movchan, in *Recent progress in the mathematics of defects*, 121–133, Springer, Dordrecht, 2011]. Here the authors address the problem of determining the variation of the stress intensity factor induced by perturbations of the crack faces and of the crack interface in a mode III interfacial crack. The cracked body under consideration is assumed to be loaded by out-of-plane tractions. The asymptotic formulae which are derived for the first-order perturbation of the stress intensity factor are obtained by extensively using the mathematical tool discussed in the authors' paper [J. Mech. Phys. Solids 57 (2009), no. 9, 1657–1682], i.e., the so-called mode III skew-symmetric weight functions. In that paper the discussion was limited to semi-infinite interfacial cracks while in the present one a discussion about the possibility of extending the treatment to finite bodies is presented. The asymptotic analysis sketched in section 3 seems correct and the illustrative example introduced in section 4 is interesting. Unfortunately, the presentation is often interrupted by references to other papers (by the same authors or by some of their co-workers) so it is difficult to follow the reasoning leading to the results obtained. Also, regrettably, the final section of the paper, entitled Discussions and conclusions, is not as comprehensive as most likely the obtained results deserve.

Review of paper: Mishuris G.S., Movchan N.V., Movchan A.B. Dynamic mode-III interface crack in a bi-material strip. Recent progress in the mathematics of defects, 121-133, Springer, Dordrecht, 2011

From the introduction: "In the present paper, we make an emphasis on a perturbation dynamic model for a Mode-III interfacial crack propagating within a strip with slightly curved boundaries and the interface contour."

In other words, the model used is intended to describe the effect of "small" deviations from planar form eventually occurring at the interface between the "detaching" strips (where displacements and stress are continuous) or at the already detached surfaces (which are stress free) or at external boundaries of

each of these strips, where Dirichlet boundary conditions are prescribed. In particular, the authors investigate how the aforementioned "imperfections" affect the stress intensity factor in the cases when a small smooth perturbation occurs "(a) on the interface at some distance from the crack tip, (b) on the crack faces away from the crack tip, (c) on the sides of the strip". Subsequently, the authors consider a singular perturbation problem, i.e. the problem of determining how a small time-dependent variation of the crack propagation speed influences the stress intensity factor. Finally, the functional which maps the variation of crack propagation speed into the corresponding stress intensity factor is used to get the integro-differential equation which determines this speed. A discussions and conclusions section, which is probably too short, concludes the paper. The results seem correct and are interesting. On the other hand, in the presentation there are so many references to several results obtained by the same authors in other papers that reading it often becomes difficult.

Review of paper: Hackl Klaus, Fischer Franz Dieter, Svoboda Jiri. A study on the principle of maximum dissipation for coupled and non-coupled non-isothermal processes in materials. Proc. R. Soc. Lond. Ser. A Math. Phys. Eng. Sci. 467 (2011), no.2128, 1186-1196, addendum (5 pp.). 1471-2946

The intention of the authors is meritorious: to study a variational principle valid for a large class of dissipative mechanical phenomena. Their preference is stated in the abstract: "Onsager's principle of maximum dissipation (PMD) has proven to be an efficient tool to derive evolution equations for the internal variables describing non-equilibrium processes." The presented approach is followed by a small number of authors and indeed deserves attention; however, its relationships with other possible approaches, for instance with the Rayleigh-Hamilton conceptual frame, have not been fully investigated yet, as would be desirable. One can agree with the authors when they continue stating that: "However, a rigorous treatment of PMD for several simultaneously acting dissipative processes is still open and presented in this paper." Their presentation proceeds by introducing dissipation functions which seem more general than those usually considered. Using again the words of the authors: "The coupling or uncoupling of the processes is demonstrated via the mathematical structure of the dissipation function. Examples are worked out for plastic deformation and heat flux." Some remarks are in order at this point: (1) The flow of presented arguments is interrupted too often by references to other papers, which makes understanding it rather difficult. Also, the style of presentation is sometimes difficult to follow. (2) Sometimes the needed assumptions are introduced without a true discussion. This is, e.g., the case with Equation (2.7), which is important—and maybe acceptable—but it is justified simply by stating that it comes from continuum mechanics, as if this equation were an analytical result of mathematical nature and not an ansatz based on some physical understanding

of the phenomena to be described. The reader may question the fact that everything in the paper is continuum mechanics. (3) The following opinion of the authors, stated before Remark 2.1, has been changed in the addendum, where the opposite choice is made: "The quantity $T\widetilde{P}$ has been denoted in [K. Hackl and F. D. Fischer, Proc. R. Soc. Lond. Ser. A Math. Phys. Eng. Sci. **464** (2008), no. 2089, 117–132] as 'dissipation' Q. From the physical point of view, it is better to use the term $\widetilde{P}$= Q/T and to denominate it as 'entropy production', which is equivalent to the negative rate of Gibbs energy, divided by T." (4) In the paper there is no mention concerning maximality of dissipation: indeed, in the presented results the authors consider only stationarity. It could be interesting to also explicitly take into account this aspect and its physical consequences. Concerning the main results of the paper one can observe that: (1) The comparison between the PMD for coupled and uncoupled processes leads to important consequences, which the authors should have stressed more clearly. (2) The presented version of PMD is spatially local in nature. (3) The important case of quadratic dissipation Q is carefully considered: the authors leave open the important problem about the link to be established between the presented theoretical frame and the one based on Rayleigh-Hamilton ideas. (4) The role and the significance of the Lagrange multiplier appearing in equation (3.6) and the related parameter f are not discussed: this point needs more attention. (5) Remark 3.3 starting with the sentence "It seems to be interesting to note that coupled processes may exist although for each individual process a contribution to the total dissipation is addressed depending only in the individual thermodynamic kinetic variables characterizing only this process" is a little bit obscure, even if somehow suggestive. The paper is concluded by the following statements which are commented on singularly: (1) "The PMD is established in order to derive evolution equations for heat flux, plastification, fluxes of matter and further internal processes, represented by a further set of internal variables." However, in the paper one cannot find a detailed discussion concerning the problem of determining, for each process, the value of the Lagrange multipliers or their related quantities, as the previously introduced f. (2) "It is shown that the evolution equations for each individual process obtain interaction terms with respect to all other processes. Only if the dissipation is a homogeneous function of the same order for all thermodynamic kinetic variables (generalized fluxes), a decoupling of the processes takes place. Specifically the coupling of heat flux and plasticity is investigated." This summarizes the most important result of the paper, which seems to be still valid also when the assumptions accepted in the addendum replace those formulated in the paper. This result seems to be correct and calls for deeper and more careful investigations.

Review of paper: Ganghoffer Jean-Francois. On Eshelby tensors in the context of the thermodynamics of open systems: application to volumetric growth. Internat. J. Engrg. Sci. 48 (2010), no.12, 2081-2098.

In this paper the author, having in mind an application to some phenomena occurring during the volumetric growth of living tissues driven by the diffusion of nutrients, formulates a thermodynamic extremum principle for a class of open continuous bodies in which Eshelby tensors play a relevant role. The results presented seem very interesting. One may agree with what is stated in the introduction of the paper: "[The] novelty advocated in the present contribution lies in a deeper insight brought into the bridges connecting the three previously mentioned areas, namely Eshelbian mechanics, open system thermodynamics [. . .] and the calculus of variations, especially the transversality conditions for solid bodies having a movable boundary." The presentation starts from a Lagrangian formulation of dissipative constitutive laws, proceeds by calculating the general variation of associated Hamiltonian action, and then constructs Eshelby tensors, thus also proving that the so-called configurational mechanics, at least in the context considered, can be based on a variational principle. The main technical difficulty confronted by the author regards the calculation of functional variations in the presence of varying integration domains: the methods presented derive directly from those used in the works of Edelen (1981). The generalized Gibbs-Duhem condition needed in the present context is obtained by a procedure which underlines its relationship with the notions of domain variation and Eshelby stresses. Particular emphasis is placed on the notion of Eshelby stress related to growth, when the general theory is applied to the aforementioned description of living tissue growth, a phenomenon which is then considered in a spherically symmetric case by suitable (and carefully described) numerical simulations. The style of the paper appears to be too synthetic and sometimes obscure. Therefore reading it is often difficult. A more extended presentation, showing more details, and a decrease in the number of cross-references to other papers which refer to important parts of the deductive arguments presented would enlighten (and make more persuasive) the results of a work which nevertheless seems correct and relevant for applications.

Review of paper: Epstein Marcelo, Maugin Gérard A. Remarks on the universality of the Eshelby stress. Math Mech. Solids 15 (2010), no.1, 137-143. 1741-3028

This paper clarifies—in a timely and precise way—the role of Eshelby (and Mandel) stresses in those theories which include material evolution. With a very balanced and fortunate choice, the authors address the core of the question, refraining from any unessential formal dressing of their presentation. The reader is directed immediately to the point and will easily draw all its important consequences and will ponder the many relevant considerations implied.

In the introduction, the authors raise the question: "Is the Eshelby stress universal?" In the following very short abstract, the reader does not find the answer to the question raised: "The possible role of Eshelby stress in the general

context of theories of material evolution is examined with particular emphasis on evolution laws that are not necessarily of the elastic type." In a few pages simple, rigorous and therefore incontrovertible calculations and definitions are presented, which show the very particular nature of Eshelby stresses, as well as the fact that the introduction of Mandel stress is absolutely equivalent to Eshelby stress. Indeed, the difference between the two treatments consists simply in a different conventional choice: while the Eshelby approach considers Helmholtz free-energy density, the Mandel approach uses free energy per unit mass. The case of pure aging is considered in detail: the result is of relevance. Actually, models based on Eshelby and Mandel stresses can only account for a very limited class of phenomena. One can therefore agree completely with the conclusions presented by the authors: "The analysis presented in this work seems to indicate that the Eshelby and Mandel stresses are not always the natural driving forces behind material evolution and, therefore, they are not universal. It is only in materials abiding by evolution laws of obvious or latent anelastic type (preservation of structural identity) that these forces emerge naturally." This statement is substantiated not only by the considerations developed in the most simple situation represented by pure aging but also by (i) remarking that the free energy presented in equation (29) is "a perfectly legitimate material law that can be arrived at as a generalization of a simple rheological model", and (ii) delivering an example of a physically meaningful continuum model in which the concepts of Eshelby stress or Mandel stress are not sufficient to describe material evolution.

Review of paper: Moonen P., Sluys L.J., Carmeliet J. A continuous-discontinuous approach to simulate heat transfer in fractured media. Transp. Porous Media 89 (2011), no.3, 399-419. 1573-1634

A class of heat conduction phenomena in a body where some "micro-cracks" are concentrated on a surface is considered in this paper. They are surely of interest and deserve a careful mathematical modeling. The results presented are of relevance and may lead to useful applications. However, the presentation is not always clear, and some difficulties can present themselves to the reader: indeed the "micro" level description (i.e. at the length scale of a single micro-crack) of the considered heat flux mechanisms and its corresponding macro level are sometimes confused. The abstract starts with the statement, "A macroscopic framework to model heat transfer in materials and composites, subjected to physical degradation, is proposed." It continues with the following— maybe too concise—passage: "The framework employs the partition of unity concept and captures the change from conduction-dominated transfer in the initial continuum state to convection and radiation-dominated transfer in the damaged state. The underlying model can be directly linked to a mechanical cohesive zone model, governing the initiation and subsequent growth and coalescence of

micro-cracks." Actually, the authors do not propose a model in which some evolution equations for initiation, growth or coalescence of cracks are proposed; the state of the cracks is assumed to be given and not influenced by heat conduction. The change from conduction to convection/radiation regimes is modeled by means of reasonable macroscopic constitutive equations for heat conduction which are obtained with a heuristic micro-macro identification process. The mechanical state of the present cracks is described also by including a cohesive damaged zone which is characterized by means of its own conductivity properties. The most relevant contribution presented regards the derivation of the damage-based "discrete" constitutive equation for heat transfer and the consequent simplified models for heat transfer across micro-cracks and cavities concentrated on a surface. The reader should consider that "discrete" is used by the authors as an adjective referring to the discontinuities of temperature fields. Subsequently some interesting academic exercises are numerically solved to present the scope of considered models, by means of a suitably derived weak formulation of postulated energy balance equations and boundary conditions.

Review of paper: Sofonea Mircea, Kazmi Kamran ,Barboteu Mikael, Han Weimin. Analysis and numerical solution of a piezoelectric frictional contact problem. Appl. Math. Model. 36 (2012), no 9, 4483-4501

From the summary: "We consider a mathematical model which describes the frictional contact between an electro-elastic-visco-plastic body and a conductive foundation. The contact is modelled with normal compliance and a version of Coulomb's law of dry friction, in which the stiffness and the friction coefficients depend on the electric potential.We derive a variational formulation of the problem and we prove an existence and uniqueness result. The proof is based on a recent existence and uniqueness result on history-dependent quasivariational inequalities obtained in [M. Sofonea and A. C. Matei, European J. Appl. Math. **22** (2011), no. 5, 471–491]."

The reviewer could not find any relevant wording or substantial difference with the summary of the paper [S. Migórski, A. Ochal and M. Sofonea, J. Math. Anal. Appl. **382** (2011), no. 2, 701–713] that he himself already reviewed for Mathematical Reviews. Also a parallel reading of the two papers does not show easily—regarding the mathematical results described in the aforementioned excerpt—the similarities and the differences between them. It has to be remarked that in the present paper the previous work of Migórski, Ochal and Sofonea is not cited. It would be very helpful—to enlighten the interested reader—to give a detailed comparison of the two mathematical models and methods presented in both publications, especially because in the present paper the theoretical results derived by means of standard methods of functional analysis are sustained by some interesting numerical simulations. Indeed, the second part of the abstract reads: "Then we introduce a fully discrete scheme

for solving the problem and, under certain solution regularity assumptions, we derive an optimal order error estimate. Finally, we present some numerical results in the study of a two-dimensional test problem which describes the process of contact in a microelectromechanical switch."

The partition of the abstract into two parts is reflected by a similar partition of the paper itself. In the first part a list of technical hypotheses is given and some theorems (based on other papers by the same group of authors) are presented. In the second part some details are given about the numerical procedure used. The reviewer finds the description of this procedure too summary and definitively lacking in important details. Also, in this second part the reader is referred to other papers, the most important of which are [M. Barboteu and M. Sofonea, Appl. Math. Comput. **215** (2009), no. 8, 2978–2991; J. Math. Anal. Appl. **358** (2009), no. 1, 110–124; Ann. Acad. Rom. Sci. Ser. Math. Appl. 1 (2009), no. **1**, 7–30]. Most likely the code developed by the authors should be able to describe phenomena much more interesting than those presented. In the conclusion one reads the following statements, with which the reviewer agrees completely: "The algorithm may be used as a benchmark for the calibration of computer codes for more complicated piezoelectric contact problems. Also, the problem is relatively easy to set experimentally, and it may provide an effective way to determine some of the constants associated with the contact process, to be used in more complex physical settings. Finally, it may be of interest to introduce controls into the model, and study the related optimal control problem."

Review of paper: Thompson Alice B., Billingham John. Inviscid coalescence in the presence of a surrounding fluid. IMA J. Appl. Math. 77 (2012), no.5, 678-696. 1464-3634

Any review of this paper needs to consider also the content of two other works.

From the introduction: "In this paper, we discuss the relevance of the work by A. B. Thompson, J. Billingham and R. H. Tew [IMA J. Appl. Math. **75** (2010), no. 6, 857–880] and Thompson [*Surface-tension-driven coalescence*, Ph.D. thesis, Univ. Nottingham, 2012] to the problem of inviscid coalescence.We will avoid giving details of the mathematical analysis here, particularly for the two-fluid problem described in Section 4. We aim to discuss the applicability, features and difficulties of this analysis, and future directions for investigation."

The interested reader is directed to the more detailed presentation [A. B. Thompson, op. cit.] to find the complete formulation of field equations and boundary conditions used by the authors. The parallel lectures of all threeworks allow us to conclude that the authors: (i) consider two immiscible and incompressible fluids and (ii) model the interfacial zone between the two immiscible fluids as a sharp interface at which they postulate that balance of force assumes the form given by so-called nonlinear Bernoulli law.

The authors also state the following.

Continued from the introduction: "In summary, it has been shown that there is no simply connected time-dependent solution for the coalescence of spherical drops surrounded by a vacuum. If such a solution did exist, it should correspond to a self-similar solution for recoil of a very fat wedge of fluid, with far-field angle $\pi - \epsilon$ and imposed tip angle $\pi/2$. However, numerical solutions to the self-similar system show that there is no solution for ϵ less than about 3^0. Investigation of the self-similar system for a recoiling slender fluid wedge suggests that suitable solutions do exist for $\epsilon \longrightarrow 0$, which may correspond to a simply connected time-dependent solution for bubble coalescence. This leaves the important question of what density ratio is required between the coalescing and surrounding fluid in order for simply connected solutions to exist, for both the self-similar and time-dependent problems."

The problem to which the authors are referring in this excerpt is somehow parallel to the problem of the minimal radius of nucleation of bubbles and drops when surrounded by their liquid or vapor respectively. As discussed in detail in [E. A. Carlen et al., J. Geom. Anal. **16** (2006), no. 2, 233–264] a more careful mathematical analysis is possible which predicts the observed (experimentally and via numerical simulations) existence of a minimal nucleation radius. This more careful mathematical analysis is however based on more detailed models which were developed for describing capillary phenomena, more detailed than the Laplace law (even when eventually corrected by the addition of jumps of inertial terms at the interface). In particular interfaces between phases are successfully modeled as interfacial zones where considered fluids are modeled as capillary fluids [see, e.g., S. L. Gavrilyuk and H. Gouin, in *Trends in applications of mathematics to mechanics (Nice, 1998)*, 306–311, Chapman & Hall/CRC Monogr. Surv. Pure Appl. Math., 106, Chapman & Hall/CRC, Boca Raton, FL, 2000]. Using this more general class of models P. Seppecher in [Int. J. Eng. Sci. **34** (1996), no. 9, 977–992] managed to study the fluid motion in the neighborhood of contact lines which are formed in phase transition at impermeable walls between drops or bubbles and respectively the corresponding vapor or liquid. In the paper under review the authors perform an analysis of a model which is in a sense "incomplete" or "ill-posed" as it lacks the needed details to describe carefully the considered physical phenomena: the models in which capillary fluids are introduced are a sort of regularization of the aforementioned incomplete ones. However, the authors in the framework which they have chosen indeed manage to obtain beautiful results and show interesting properties of the solution of all possible generalized models which include the Laplace law. Their analysis relies on several simplifications to the coalescence problem. They restrict themselves to self-similar recoil, and consider only the small-displacement problem, in which the imposed contact angle is $\pi - \lambda\epsilon$ rather than $\pi/2$, with $1 \ll \lambda \ll \epsilon - 1$ so that they no longer need to solve a free boundary problem. Using these limiting assumptions they find in Section 3 that when the surrounding fluid has zero density, there is no simply connected solution to the small-displacement system for large λ.

Continued from the introduction: "In Section 4, we investigate the two-fluid problem for small displacement. We identify a distinguished density ratio, and

find numerical and asymptotic evidence that shows that for this distinguished density ratio, a valid solution persists for large λ, and that its dependence on is consistent with $t^{1/2}$ time dependence for the coalescence of spherical drops."

The authors also state that although the solutions found do not solve the large-displacement system, they display similar qualitative features to the known solutions of the large-displacement problem. The methods presented in the reviewed paper, together with those presented in the literature when dealing with capillary fluids and thick interfacial zones, may allow for a major advancement of the understanding of many capillary phenomena occurring at contact lines.

Review of paper: Nazar M., Shahid F., Saeed Akram M., Sultan Q. Flow on oscillating rectangular duct for Maxwell fluid. Appl. Math. Mech. (English Ed.) 33 (2012), no. 6, 717-730. 1573-2754

The velocity field and the associated shear stresses corresponding to the unsteady flow of generalized Maxwell fluid on an oscillating rectangular duct are determined by means of double finite Fourier sine and Laplace transforms. These solutions are also presented as a sum of the steady-state and transient solutions. The solutions corresponding to Maxwell fluids, performing the same motion, appear as limiting cases of the solutions obtained here. The equation studied in the paper under review exactly coincides with equation (26) on page 75 in [H. T. Qi and J. G. Liu, Eur. Phys. J. Special Topics **193** (2011), no. 1, 71–79, doi:10.1140/epjst/e2011-01382-6]. The initial and boundary condition problems also coincide, and the closed-form solutions are very similar. However, the aforementioned paper is not cited in the present work and therefore the necessary comparison of their results is not made.

Review of paper: Kuttler K.L., Purcell J., Shillor M. Analysis and simulations of a contact problem for a nonlinear dynamic beam with a crack. Quart. J. mech. Appl. Math. 65 (2012), no. 1, 1-25. 1464-3855

This paper is a nice piece of work: in it a mathematical model—relevant for its engineering applications—is studied rigorously. The reviewer believes that the mathematical results presented will supply a solid basis for the formulation of reliable numerical codes, to be used to explore the properties of a beam model which is really promising.

In the abstract the authors state: "This work studies and simulates the dynamics of a Gao beam that may come in contact with a reactive foundation and which has an evolving symmetric edge crack. The problem is set as a variational inequality coupled with a differential equation for the evolution of the damage variable, which measures the severity of the crack." It has to be remarked that more generally a variational approach can also be used to get the evolution equation for damage [see, e.g., N. K. H. Pham, J.-J. Marigo and C. Maurini, J. Mech. Phys. Solids **59** (2011), no. 6, 1163–1190]. Of course, including the evolution of damage in the variational scheme is very useful when

dealing with the establishment of existence and uniqueness theorems and when formulating numerical procedures to apply the model to practical problems.

The abstract continues as follows: "The existence of a local weak solution for the model is proved using approximations, result for abstract problems in Hilbert spaces, and a priori estimates. A finite-element scheme for the problem is proposed, and representative simulations presented. In particular, the dependence of the beam's vibration frequency on the crack's position and size is depicted." The part dealing with the formulation of the finite-element scheme and its theoretical background seems correct and in some aspects original. Very interesting is the presented application to the study of the influence of crack geometrical properties on beam dynamical response to externally applied loads, which seems to open interesting new research perspectives.

In the introduction one reads: "We extend a model for the dynamics of a nonlinear beam, developed by Gao in [D.Y. Gao, Mech. Res. Comm. **23** (1996), no. 1, 11–17; Internat. J. Non-Linear Mech. **35** (2000), no. 1, 103–131; D. Y. Gao and H. D. Sherali, in *Advances in applied mathematics and global optimization*, 257–326, Adv. Mech. Math., 17, Springer, New York, 2009] and analysed there and in [K. T. Andrews et al., "Analysis and simulations of a nonlinear dynamic beam", preprint, 2009; per bibl.; M. F. M'Bengue, *Analysis of models for nonlinear dynamic beams with or without damage or frictionless contact*, Ph.D. thesis, Oakland Univ., **2008**; No. 27], which allows for the study of buckling by including the growth of a symmetric edge crack. The damage to the beam, which in this case is the growth of the crack as a result of cycles of tension/compression, is described by using the so-called damage function [M. Frémond, *Non-smooth thermomechanics*, Springer, Berlin, 2002; M. Shillor, M. Sofonea and J. J. Telega, *Models and analysis of quasistatic contact: variational methods*, Lecture Notes in Phys., 655, Springer, Berlin, 2004, doi:10.1007/b99799; M. Sofonea, W. Han and M. Shillor, *Analysis and approximation of contact problems with adhesion or damage*, Pure Appl. Math. (Boca Raton), 276, Chapman & Hall/CRC, Boca Raton, FL, 2006]. The model also allows for contact between the beam and a reactive foundation that is situated under it." However, it is difficult to find a clear statement about the features of the model presented which are not already given in the literature.

The Conclusions section seems too short. In it, the authors state: "The numerical method was implemented and two types of simulations performed. First, vibrations of the beam until failure were simulated for the under-buckled and buckled states. It was seen that due to the choice of a large damage rate coefficient k_d, the beam broke fairly quickly in both cases." In these simulations it is shown how the model is able to describe the growth of damage, in the presence of external actions which are able to lead to the failure of the beam. A more detailed study of the influence of buckling on crack growth seems to be needed here.

The Conclusions section continues as follows: "The second type of simulations were run to explore the behaviour of the beams' oscillations using their spectra. It was found that the spectrum depends heavily on the depth of the crack (the damage r) and on the position of the crack x_c. The simulations found

that the dependence on r seems to follow the sequence of stable, transitional and chaotic behaviour (Figs 5–8). The results of the simulations indicate the difficulties in identifying the crack position and depth for post-buckled states. Additional study is needed to determine if one can determine the position and depth of a crack from the distribution of the spectral lines for different frequencies." In the reviewer's opinion the issue considered in the previous sentences is the most important one when dealing with damage growth in beams. It is a pity that this point has not been more deeply studied by the authors, and therefore the question of the efficacy of the proposed approach remains.

Review of paper: de Saxcé Géry, Vallée Claude. Bargmann group, momentum tensor and Galilean invariance of Clausius-Duhem inequality. Internat. J. Engrg. Sci. 50 (2012), no. 1, 216-232

A central problem in the foundations of continuum mechanics is represented by the search for the most comprehensive postulation framework to be used for all continuum models. The dichotomy is between those who accept the d'Alembert and Lagrange principle of virtual velocities (or works or powers) and those (following the ideas of Newton and Poisson) who want to base on a series of balance principles all the postulation processes in continuum mechanics. A beautiful defense of the Lagrangian view of mechanics can be found already in the work by Gabrio Piola [*Di un principio controverso della meccanica analitica di Lagrange, e delle molteplici sue applicazioni: memoria postuma*, Giuseppe Bernardoni Giovanni, Milano, 1856].Piola confuted in his [*Intorno alle equazioni fondamentali del movimento di corpi qualsivogliono, considerati secondo la naturale loro forma e costituzione: memoria*, B. D. Camera, Modena, 1846] all of Poisson's arguments against the principle of virtual velocities (or work or powers) as presented in [S. D. Poisson, "Mémoire sur les équations générales de l' équilibre et du mouvement des corps solides élastiques et des fluides", J. École Polytech **13** (1831), 1–174; "Mémoire sur l' équilibre et le mouvement des corps élastiques", Mém. Acad. Sci. Inst. France **8** (1829), 357–570]. Modern supporters of Poisson's party are Truesdell, Noll and their followers, while Toupin seems to belong to the Lagrangian party.

The paper under review, presented at the 13th Colloque Internationale de Théories Variationnelle (2008), does not openly embrace one of the two opposed theses; however, the results presented may have an impact on the controversy, at least for what concerns continuum mechanics. The authors base their analysis on previous research by Souriau and Valle (see references in the paper) aimed at finding the correct geometrical structures for physical theories and fix their attention on continuum thermodynamics. The main results they show in sections 3–6 consist in: (i) finding a momentum tensor in terms of Planck's potential verifying the requirement of material indifference, (ii) introducing an irreversible (dissipative) momentum tensor, (iii) calculating the divergence of the sum of both these tensors (which is called the total momentum tensor) and (iv) recognizing that the balances of mass, linear momentum and energy are all together equivalent to assuming that the total momentum tensor is divergence-free. The

geometrical structure in which tensorial quantities are introduced by the authors is a five-dimensional linear space in which classical space-time is embedded. The tensorial rule—suitably introduced to frame Galilean mechanics—needed to impose invariance conditions is supplied by the Bargmann group of transformation (see references in the paper). In sections 7 and 8 the Clausius-Duhem equation and its relation with the constitutive equations for a dissipative momentum tensor are discussed in the geometrical framework previously introduced. This is done by expressing entropy production in terms of the previously introduced total momentum tensor and by characterizing nondissipative processes. The results presented seem sound and correct, but to fully establish their importance some deeper investigations are needed and their relationship with a suitable version of the principle of virtual velocities (or the less general principle of least action) must be clarified. In the reviewer's opinion, however, a reproach has to be formulated to the style of this paper (and of many others originated by the school founded by Souriau): it seems too Tacitean and esoteric. Moreover, readers may be discouraged by the dense citations of other papers from which crucial points of the presented arguments need to be extracted and thus feel that they are missing an important part of the arguments.

Review of paper: Galeş C. Spatial behavior and continuous dependence results in the linear dynamic theory of magnetoelectroelasticity. J. Elasticity 108 (2012), no.2, 209-223. 1573-2681

In classical piezoelectricity and piezomagnetism only static or quasi-static electromagnetic fields are considered. Thus, although the mechanical equations are dynamic, the electromagnetic equations are static and the electric field and the magnetic field are not dynamically coupled. While for many applications quasi-static theory is sufficient, still there are situations, e.g. in the presence of radiated electromagnetic power from a vibrating piezoelectric device, where full electromagnetic coupling needs to be considered. Such a fully dynamic theory has been called piezoelectromagnetism and is being developed in the current literature. The purpose of the paper under review is to investigate some qualitative properties of solutions for the equations of linear dynamic magnetoelectroelasticity. As for what concerns this theory, relatively few comparable results are known; the author extends some arguments already used for hyperbolic equations to investigate the spatial behavior of dynamic magnetoelectroelastic processes. The results presented hold not only for piezoelectric materials but more generally for anisotropic magnetoelectroelastic solids. They can be described as follows: After defining the "support" of the given data in a fixed interval of time [0, T], that is the set of all points for which at least one of the given data in [0, T] (boundary or initial data or body force) is nonzero, the appropriate time-weighted surface power function for the problem considered is introduced. Then it is proven that, for each fixed time t in a given compact interval [0, T], the whole activity is vanishing at distances from the support of the given data on [0, T] greater than κt, where κ is a constant depending on the constitutive coefficients of the magnetoelectroelastic medium. Inside the

domain of influence, an exponential decay estimate is established for describing the spatial behavior of solutions for both bounded and unbounded bodies. A direct consequence of this study is the uniqueness of solutions. Due to the generality of the problem, the value of the parameter κ provided here is not an optimum one. Finally, the particular case of nonzero initial conditions is considered to establish some results on the spatial evolution of transient solutions, provided that the initial data is assumed to be bounded in a certain energy norm. Subsequently the continuous dependence of solutions with respect to the external given data is studied. By means of Gronwall's lemma a Lyapunov stability theorem is established. This result proves that, in the motion following any sufficiently small change in the external data system, the deformation of the body is arbitrarily small in magnitude.

The mathematical results presented seem novel, interesting and appear to be obtained by correct arguments, while the physical phenomena described are surely relevant.

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Review of paper: Khludnev Alexander. Contact problems for elastic bodies with rigid inclusions. Quart. Appl. Math. 70 (2012), no. 2, 269-284. 1552-4485

In the reviewed paper some equilibrium problems arising in the mechanics of elastic bodies with rigid inclusions possibly in contact with another rigid inclusion or with a non-deformable punch are solved. Some problem formulations with inequality type boundary conditions of a non-local type describing a mutual non-penetration between contact surfaces are presented, which seem correct and physically meaningful. In the problems treated the set of points in which rigid inclusions may be in contact with other rigid inclusions or an external rigid obstacle is unknown and the boundary conditions considered are nonlinear and of inequality type: the contact set is determined provided that a solution of the problem is found. However, the reviewer believes that regarding the problems presented in this paper as free moving boundary problems may be somehow misleading. Indeed the shape of the boundary of rigid inclusions or of external rigid obstacles cannot change and rigid inclusions can only move with infinitesimal rigid displacement. Moreover all problems studied are two-dimensional: therefore except when the rigid inclusions (and the external obstacles) have as boundaries some curves which can (eventually partially) be rigidly superimposed it will happen that the set of contact points will be finite.

The differential and variational problem formulations (including variational inequalities) are discussed, and a tricky fictitious domain approach is applied which allows one to establish useful relations between different types of equilibrium problems. Using standard mathematical techniques, the author then proves the existence of solutions for all the types of inclusions and geometries listed in the paper's section titles: 2. Rigid inclusion being in a contact with non-deformable punch on the boundary; 3. Thin rigid inclusion on the contact boundary; 4. Thin rigid inclusion contiguous to volume rigid inclusion; 5. Thin rigid inclusion being in contact with volume rigid inclusion; 6. Thin rigid inclusion located inside the domain and on the boundary.

The contact problems studied are also compared with those formulated by Signorini and solved in other papers in the literature, including some by the

author himself. To this aim a rigorous passage to a limit is justified in which a rigidity parameter tends to infinity. The technical difficulties to be confronted when treating the more general three-dimensional problems of the kind presented in the reviewed paper seem relevant. A discussion of possible extensions of the results presented is unfortunately missing.

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Review of paper: Mellet Antoine, Nolen James. Capillary drops on a rough surface. Interfaces Free Bounds. 14 (2012) no. 2, 167-184. 1463-9971

The paper studies liquid drops lying on a rough planar surface. The drops are minimizers of an energy functional that includes a random adhesion energy. The existence of minimizers and the regularity of the free boundary are proven. When the length scale of the randomly varying surface is small, the minimizers are seen to be close to spherical caps which are minimizers of an averaged energy functional. In particular, an error estimate is given that is algebraic in the scale parameter and holds with high probability. Only surface tension effects (proportional to the free surface area and a class of wetting interactions between the liquid drop and the solid support) are accounted for: in particular the gravitational forces are not included. The volume of the drop is assumed to be fixed. Indeed the mathematical model considered describes wetting interaction in a rather rough way, while in the literature more sophisticated models were studied in a similar context [see, e.g., G. Bouchitté and P. Seppecher, in *Motion by mean curvature and related topics* (Trento, 1992), 23– 42, de Gruyter, Berlin, 1994]. The relative adhesion coefficient between the liquid and the solid

is assumed to be a random field taking values in the interval (-1, 1). It is well known that when this coefficient is smaller than or equal to -1 the global energy minimizers correspond to spherical sets having no contact with the solid interface, while when it is greater than -1 the absolute minimizer must touch the solid support. The first result which the authors claim to obtain is a proof of the existence and regularity of global minimizers under an additional constraint that confines the drop to a bounded region. Next an issue of homogenization is considered: for relative adhesion coefficients which depend on the variable x ranging in the solid contact wall and which are piece-wise constant the authors assume that the coefficients have a small parameter increasing their variation. They find a homogenized energy functional and estimate the probability of the error induced when using it. The mathematical part of the paper seems correct, although rather involved: the authors do not clearly indicate which among the presented arguments are novel and where they confront technically challenging issues. The setting is rather abstract for what concerns the set of regions which are considered in the studied minimization problem and this choice clearly makes arduous the use of more physically meaningful energy functionals.

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Review of paper: Negri Matteo. Crack propagation by a regularization of the principle of local symmetry. Discrete Contin. Dyn. Syst. Ser. S 6 (2013), no. 1, 147-165. 1937-1179

Summary: "For planar mixed mode crack propagation in brittle materials many similar criteria have been proposed. In this work the Principle of Local Symmetry together with Griffith Criterion will be the governing equations for the evolution. The Stress Intensity Factors, a crucial ingredient in the theory, will be employed in a 'non-local' (regularized) fashion. We prove existence of a Lipschitz path that satisfies the Principle of Local Symmetry (for the approximated stress intensity factors) and then existence of a BV-parametrization that satisfies Griffith Criterion (again for the approximated stress intensity factors)."

The paper presents interesting results which need nontrivial technical properties to be proven. Remarkable are the approximations obtained for stress intensity factors and the corresponding estimates of the error in their evaluation.

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Review of paper: Treinen Ray. On the symmetry of solutions to some floating drop problems. SIAM J. Math. Anal. 44 (2012), n0.6, 3834-3847. 1095-7154

This paper starts from a physical problem formulated and studied in [A. R. Elcrat, R. W. Neel and D. Siegel, J. Math. Fluid Mech. **6** (2004), no. 4, 405–429] to which the author refers often. The model which is considered there is based on Laplace's classical treatment of capillary phenomena at interfaces: three different surface energies—not depending on the deformation of the interfaces—are introduced. The total energy of the system is calculated by adding the energy

concentrated on the aforementioned interfaces (which is therefore proportional to their area) to the total bulk gravitational energy (see the expression between equations (1.5) and (1.6) in the paper under review). (Concerning this choice, in the reviewer's opinion the more effective model found in the framework of second gradient fluids should also be considered, as its potentiality in this context is well established, as proven for instance by the beautiful paper [P. Seppecher, Int. J. Eng. Sci. **34** (1996), no. 9, 997–992, doi:10.1016/0020-7225(95)00141-7].)

The main result of the paper is stated in its abstract: "The symmetry of floating drops is considered. Under conditions that the free boundary is contained in a horizontal plane it is shown that all three component interfaces are symmetric about a vertical line." The proof is based on many different papers among those available in the literature and the author shows a great capability to exploit efficiently the results found there. The proof seems correct and is obtained with a careful examination of all possible cases in the formation of the considered surfaces. The author writes: "The present work applies some tools used by other authors. These tools are collected in section 2. Section 3 is the proof of the main theorem. The approach is as follows. The symmetry results of H. C.Wente [Pacific J. Math. **88** (1980), no. 2, 387–397] for fluid-fluid configurations are adapted to show that under some circumstances the drop is symmetric about a vertical axis in both the light drop and the heavy drop problems. R. S. Finn and T. I. Vogel [Analysis (Munich) **29** (2009), no. 4, 387–402] developed criteria for floatation of solid objects, and some of their methods are adapted to show that the external interfacial surface between the supporting fluid and the fluid above is also symmetric about the same vertical axis. These methods rely on a comparison principle of Siegel [Pacific J. Math. **88** (1980), no. 2, 471–515], which is an extension of the comparison principle of P. Concus and Finn [Acta Math. **132** (1974)]. (See [R. S. Finn, *Equilibrium capillary surfaces*, Grundlehren Math. Wiss., 284, Springer, New York, 1986].)" Unfortunately the paper lacks a clear statement about the novel technical difficulties which the author confronts in his proof; however, it is likely true that the interesting result he presents is new.

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Gianpietro Del Piero: a scientist on the edge between engineering sciences and functional analysis

A constant in prof Del Piero's scientific life was the revisitation of the fundamentals and the quest for new, effectivemathematical models for Continuum Mechanics. This forced him to repeatedly enlarge his background, to include significant parts of modern mathematics. Del Piero is a true engineer: he deeply knows the correct "practice" needed to construct in an efficient ways structures and complex engineering devices. In a pretty seminar about the "Domes," (http://www.sdelevicivita.it/2012/11/seminar-by-prof-gianpietro-del-piero-title-domes/) their constructive properties and the history of their construction during the centuries, he managed to show to the audience how the knowledge of the most practical aspects of structural engineering cannot be separated from the mastering of important parts of theoretical mechanics.

Professor Del Piero is one of the first mechanicians who, since the very beginning of their diffusion outside the cultural environment of pure mathematics, understood the impact and importance that measure theory and many aspects of functional analysis may have in continuum mechanics.

His appreciation of the powerful tools supplied by functional analysis began as early as 1974, when he became assistant professor of Variational Methods at the Università di Pisa, in this way starting the construction of the strong mathematical background on which he based his whole scientific activity.

The reader who looks at his list of publications and his CV discovers that he applied sophisticated mathematics to model the mechanical behavior of concrete, brick walls, maintenance and durability of concrete structures, ductile-brittle transition in engineering materials, and many other phenomena in structural mechanics, plasticity, damage propagation, viscoelasticity, cohesion and adhesion.

He also was involved in the conception of experimental measurements and numerical codes developed by younger researchers, always using his theoretical knowledge to choose the appropriate methodologies.

Professor Del Piero has been a paladin of theoretical knowledge as an essential tool in engineering sciences, in a time in which mathematics and any form of theoretical knowledge are being more or less openly disdained.

He is one of the most skilled contemporary experts of fracture and plasticity. He is well aware of the importance of a careful description of the phenomena. For this, he resorts to the main tool which human mind conceived for problem solving, that is, what Galileo called "the language of mathematics".

Even if he has been sometimes considered as a crypto-mathematician infiltrated among the engineers or, conversely, as an engineer infiltrated in among the mathematicians, what he did was simply to follow his Archimedean point of view: mathematics is a tool which the engineer has to master if he wants to master the real world. This point of view we share with him, and for this reason it was a great pleasure and honor to be called to edit this special issue.We hope that Prof. Del Piero, now free of all kind of obligation, will eventually devote himself exclusively to the activity he likes more: research.

Gianpietro Del Piero: a continuator of the Italian tradition in continuum mechanics, as started by Gabrio Piola

I had the opportunity to meet Gianpietro Del Piero only recently. Indeed, even if I followed several of his inspired lectures and seminars during the years, I did not have the occasion of talking with him on scientific subjects until 2010. This occasion came with a decision of the Scientific Committee of the First Sperlonga Summer School on Mechanics and Engineering Sciences, held in September 2011, whose subject was "Atomistic and continuum descriptions of microstructures".

Indeed, agreeing with the proposal of Antonio Di Carlo, the Committee decided to put Prof. Del Piero and Prof. Presutti in charge for the 2011 edition of the school. In order to organize the school, Prof. Del Piero visited me several times, discussed with me about many scientific and non-scientific subjects, and shared with me his vision about many aspects of life and his experience about academy, science and human beings.

A consequence of these meetings was that I was able to better understand my own papers, thanks to the deep criticisms of their foundations made by Prof. Del Piero. Also, I understood better some of his papers. A few months later, with the support of Samuel Forest, I decided to submit to the Editor-in-chief of Continuum Mechanics and Thermodynamics a proposal for the present special issue, which gathers some of the contributions presented at the Symposium in honor of Gianpietro Del Piero, in occasion of his retirement. This event took place at the end of the School cited above.

(http://www.sdelevicivita.it/category/videolezioni/symposiumdelpiero/) Later, Samuel and myself decided to extend the invitation to publish a paper in this special issue to other scientists working in the same area and sharing the taste for rigorous presentations.

Since our first meeting at the Smart Materials and Structures Laboratory in Cisterna di Latina, I had the pleasure to meet Gianpietro periodically, and to discuss with him a lot: during these meetings, projects for joint papers started to be made.

I am not sure that any paper will be completed rapidly, because, as I have learnt quickly, the most demanding reviewer of Del Piero's papers is Del Piero himself. I have seen him to abandon a nearly completed paper, which many of our colleagues would have considered a good one, simply because he considered it not original enough.

In a time of h-indices, impact factors, and citations counting, this sound attitude toward publishing activity is an example to be followed: it is indeed absolutely useless, except maybe for somebody's career or somebody's else inflated self-esteem, to publish papers with a poor innovative content. In another occasion, I had the opportunity of discussing about these subjects, and many others related to Ancient Science, with Gianpietro and Lucio Russo, the author of the remarkable book "The Forgotten Revolution" published by Springer. Indeed I discovered, with my great intellectual amusement, that Gianpietro had enthusiastically read the book and that he was eager to discuss with the author.

The dinner which the three of us had was a true intellectual enjoyment.We agreed —for instance— that the decline of the technology in domes construction was a confirm of Russo's ideas about the general decline of science and technology. Moreover, we agreed that the risks of a degradation of scientific knowledge are always present and that in our times, they seem to be particularly dangerous.

Gianpietro Del Piero is not writing so many papers, because he is pondering them seriously. However, he does not suffer of the disease of extreme and compulsive perfectionism: he manages to be happy with a good approximation of perfection. Therefore, he manages, by means of carefully written papers, to spread his sometimes very original ideas and scientific conceptions.

In the same way, his lectures are rigorous, precise and demanding, very demanding. The audience is asked to reason step by step with him to follow the arguments which he presents at a breathtaking rhythm. He demands to his audience much less than he demanded to himself while preparing his lectures, but still the majority of the persons suffer the great intellectual effort they require.

In many aspects, he is a continuator of Piola's scientific work: Gianpietro Del Piero was among those who infused in (Continuum) Mechanics some of the mathematical rigor needed for its development. Like Piola, he used advanced mathematical tools to found Continuum Mechanics on solid bases, but, like Piola, he never lost contact with applications. One night, after a glass of wine, he admitted that he had another very important point in common with Piola: they both share their family names with an outstanding football player!

Gianpietro belongs to the age of reason: his vision of life is based on the

rational analysis of problems and on the effort of finding rational solutions. He is also a very opinionated person: his beliefs are strong as is his personality. I personally do not agree with many of his opinions: however, we managed to discuss peacefully about our divergent opinions, because, perhaps contrary to common belief, Gianpietro is very tolerant and open minded. In his political, scientific and cultural battles, he always fought in absolute good faith, and he is capable to understand and admit when and where he was wrong.

As happened to many "caposcuola" (i.e. leaders of a scientific group), he played an eminent role in academic politics during his career. Although some of his choices were considered controversial, I am persuaded that also in this role he tried to do his best for the advancement of science and for the well-being of Italian academic institutions. Looking at his scientific results and at his academic choices, I hope that for a long time, after his retirement, he will be given the force to continue his intellectual activity.

For his life, he worked

AD SCIENTIARUM HAUSTUM ET SEMINARIUM DOCTRINARUM

towards a source of science and a seedbed of doctrine,

as demanded by Frederick the Second Hohenstaufen to the professors of my Alma Mater.

The academic and scientific activity of a "Maestro" in applied mechanics: Laudatio of Professor Antonio Di Carlo, Università di Roma TRE (Italy)

Antonio Di Carlo, both as scientist and professor, is open minded, creative and original. His elocution is impeccable, elegant and appropriate, his lectures suggestive, deep and informative, his mastery of Mathematics and Mechanics exceptional. I had the privilege of being his assistant professor when he was Chair of Structural Mechanics at the beginning of my career. I greatly enjoyed his lectures, gaining from them a deep and thoroughly documented insight into continuum and structural mechanics, thanks to the collection of files and documents that he prepared for his students. I learned – working with him – how to adapt my lectures to the audience, how to effectively prepare serious written tests and how to fairly examine students during an oral examination. I also learned to be more demanding and self-critical when writing my own papers.

The lessons of Professor Di Carlo would not be fully learned, however, by limiting attention to a rhetoric laudatio without discussing some of the most controversial aspects of his academic personality. Indeed, the apparent impact of his research in the literature of mechanical science would seem less compelling than it actually is. In the Hellenistic tradition, also exemplified, for example, by Bacon, a large role in the transmission and advancement of science is attributed to the melitta; that is, the scientist acting as a bee that flies from one flower to the other carrying with it the pollen of knowledge. Melittas do not

leave written traces of their work on the elaboration of ideas and the transmission of knowledge. Even more important is their work of selection: they decide which information has to be transmitted to a given scientist whose competence they believe is needed to advance a particular field of study. They tend to disappear behind the knowledge they transmit and behind the papers that they eventually author explicitly. Therefore, while their original contributions to the advancement of science may be regarded as invaluable, they are often hidden and invariably underestimated. The attitude of Professor Di Carlo towards perfection in presenting his work in written form magnified his tendency to transmit it mainly in oral form during seminars, conferences and scientific discussions. Antonio is the most generous academician whom I have ever met – especially with younger scientists: he dedicated many hours to the scientific training of many more scientists than is apparent.

I am proud to publicly recognizemy personal debt to him on this occasion and honoured by the friendship and esteem that he has always manifested to me, albeit dispassionately. He has a shy and, at the same time, sarcastic temperament and has always been strong with the strongest and weak with the weakest while refraining from any opportunistic behaviour; therefore, he cannot be accused of being a good politician. However, he is the best, most effective and honest department head I know of, this having been recognized and corroborated by his election to the office twice. For this reason, when discussing with Professor Steigmann an appropriate way to honour Professor Di Carlo's achievements, we finally decided to invite promising young scientists to submit dedicated papers to the normal issues of *Mathematics and Mechanics of Solids*, subsequent to the publication of this laudatio, including the simple statement: "Dedicated to Prof. Antonio Di Carlo in recognition of his academic activity".