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The Scientific Life and Influence of Clifford Ambrose Truesdell III

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1. Introduction

Clifford Truesdell was an extraordinary figure of 20th century science. Through his own contributions and an unparalleled ability to absorb and organize the work of previous generations, he became pre-eminent in the development of continuum mechanics in the decades following the Second World War. A prolific and scholarly writer, whose lucid and pungent style attracted many talented young people to the field, he forcefully articulated a view of the importance and philosophy of 'rational mechanics' that became identified with his name.

He was born on 18 February 1919 in Los Angeles, graduating from Polytechnic High School in 1936. Before going to university he spent two years at Oxford and traveling elsewhere in Europe. There he improved his knowledge of Latin and Ancient Greek and became proficient in German, French and Italian. These language skills would later prove valuable in his mathematical and historical research.

Truesdell was an undergraduate at the California Institute of Technology, where he obtained B.S. degrees in Physics and Mathematics in 1941 and an M.S. in Mathematics in 1942. He obtained a Certificate in Mechanics from Brown University in 1942, and a Ph.D. in Mathematics from Princeton in 1943. From 1944–1946 he was a Staff Member of the Radiation Laboratory at MIT, moving to become Chief of the Theoretical Mechanics Subdivision of the U.S. Naval Ordnance Laboratory in White Oak, Maryland, from 1946–1948, and then Head of the Theoretical Mechanics Section of the U.S. Naval Research Laboratory in Washington, D.C. from 1948–1951. He was Professor in the Department of Mathematics at Indiana University from 1950–1961, and spent the remainder of his career at The Johns Hopkins University, where he was Professor of Rational Mechanics until 1989 and Professor Emeritus of Rational Mechanics thereafter.

He received honorary doctorates from the Politecnico di Milano and the Universities of Tulane, Uppsala, Basel and Ferrara, and many prizes, including the Euler medal of the U.S.S.R. Academy of Sciences on two occasions (1958, 1983), the Bingham medal of the Society of Rheology (1963), the Gold Medal and International Prize "Modesto Panetti" of the Accademia di Scienze di Torino (1967), the

George David Birkhoff Prize in Applied Mathematics, jointly awarded by the American Mathematical Society and the Society for Industrial and Applied Mathematics (1978), the Ordine del Cherubino from the University of Pisa (1978), the Humboldt Prize (1985) and the Theodore von Kármán Medal from the American Society of Civil Engineers (1996). He was elected to foreign membership of many Academies: Modena (1960), l'Académie d'Histoire des Sciences, Paris (1961), Istituto Lombardo (1968), Istituto Veneto (1969), Bologna (1971), Accademia Nazionale dei Lincei (1972), l'Académie de Philosophie des Sciences, Bruxelles (1974), Torino (1978), Academia Brasileira de Ciències (1981), the Polish Society for Theoretical and Applied Mechanics (1985), and the Regia Societas Scientiarum Upsaliensis (1987). In the United States he was elected a Fellow of the American Academy of Arts and Sciences in 1991, but was never elected to the National Academy of Sciences.

In addition to his own scientific contributions and writings, he left a permanent scientific legacy by providing the opportunity for others to publish work in the same spirit in the two great journals he founded, the *Archive for Rational Mechanics and Analysis* in 1957, which continued the earlier *Journal of Rational Mechanics and Analysis* begun in 1952, and the *Archive for History of Exact Sciences* in 1960. He also founded and edited the *Springer Tracts in Natural Philosophy* and co-edited several influential volumes of the *Handbuch der Physik*.

Truesdell, together with Coleman, Ericksen, Markovitz, Noll, Rivlin, Serrin, Sternberg, and Toupin, founded the Society for Natural Philosophy in 1963. The first meeting, on *Statistical and Continuum Theories of Materials*, was held 23–26 March 1963 at The Johns Hopkins University, and featured as speakers Ericksen, Grad, Jaynes, Rivlin and Toupin. Those who seek refuge today from the self-absorbed professional societies of science and mathematics will enjoy Truesdell's informal policy statement: "The Society represents no profession or academic branch. It arises from a twofold need: to open and maintain communications neutralizing blind specialization, and to recognize quality in scientific research . . . . A multitude of short communications fails to arouse interest or to impart knowledge. We hold that the main function of the program of a meeting should be to allow the greater number to hear important new or organizing results of the few who have just found them." Despite the fact that a unique mechanism, based on age restrictions, was built into the by-laws of the society for the purpose of keeping it active, the Society slowly declined in the 1990s.

Truesdell was an outstanding historian of science. Among many important contributions, he edited or co-edited six volumes of the collected works of Euler, on whom he was a leading authority and whom he greatly admired. As part of this endeavor he wrote a remarkable treatise [45] on the development of the theory of flexible or elastic bodies in the period 1638–1788, and various other scientific introductions [39, 43] to Euler's collected works. In this scientific appreciation we will not discuss Truesdell's historical work *per se*, leaving this to those better qualified to do so. However, we will see frequently how, as a rare example of an active and gifted scientist reading and studying the original work of the masters, Truesdell enriched both his own research (as he did the history of science) and the scientific culture of his time.

After some years of failing health, Clifford Truesdell died on 14 January 2000, in Baltimore, survived by his wife of 48 years, Charlotte Brudno Truesdell, his son Clifford, and two grandsons. His books and papers are deposited at the Scuola Normale Superiore in an historical building, Complesso San Silvestro, in the center of Pisa. His personal bound copies of the two journals he founded have been donated to the Archives of American Mathematics at the University of Texas at Austin.

2. Scientific contributions

Truesdell made important personal scientific contributions to the areas of Continuum Mechanics, Thermodynamics, Elasticity including membranes, plates and shells, Fluid Mechanics, the Kinetic Theory of Gases, Statistical Mechanics, Wave Propagation, the study of Special Functions, Mixture Theory and Applied Mechanics.

As an undergraduate at Caltech, Truesdell was profoundly influenced by his teacher Harry Bateman. He took courses from Bateman on the partial differential

C. Truesdell, Princeton, 1944

equations of mathematical physics in 1940–41, and in 1941–42 on methods of mathematical physics (where for the entire year he and C.-C. Lin were the only students), aerodynamics of compressible fluids, and potential theory. In his generous tribute to Bateman [56, Chapter 7] TRUESDELL recalls how tough these courses were, and how he devoted long hours to following them and doing the exercises. In subject matter, in particular their coverage of continuum mechanics and physics, and their use of the theory of partial differential equations and special functions, the courses formed a solid foundation for the young researcher. But Bateman was also a man who "saw the whole of the mathematical sciences as a continuum without compartments" and who "saw no difference in style or standard between pure mathematics and applied", values that Truesdell's own research were to exemplify.

Truesdell's Ph.D. thesis of 1943 was on the membrane theory of shells of revolution, a topic that was little related to the research of his advisor Solomon Lefschetz[1]. In his published article [31] TRUESDELL explains in a footnote that he had begun the work in the summer of 1942 while at the School of Mathematical Mechanics at Brown University, and he thanks "Professors Bateman, Bohnenblust, Lefschetz, Prager, E. Reissner, H. Reissner, Tukey, and especially Professor Nemenyi". He develops a formal method for deriving both 'membrane' and 'bending' theory simultaneously from a common expansion, and he studies the singular state at the apex of a cone, relying mainly on the tools of Fourier expansion and formal manipulation of power series. In its use of formal methods, its reliance on special kinematic hypotheses (membranes of revolution only) and its presentation of series solutions of a great many special problems, this work can be considered properly isolated from his other work. Perhaps, through juxtaposition and reaction, it is a point of departure toward loftier goals. The speed with which the dissertation was written might give the impression that Truesdell would have had little time for other activities at Princeton. But he attended an introductory course for graduate students in Mathematical Logic given by Alonzo Church in the spring of 1943, the notes he took forming the basis for CHURCH's book [3].

During the years just following the publication of his thesis, Truesdell forayed into areas both orthogonal and tangential to the directions of research to which he would later contribute so much. A second paper on shells [33] in 1948 examines the boundary conditions that should be imposed in membrane theory at the apex of a cone. His other works of this period on special functions and on the kinematics of vorticity are quite different, being characterized rather by a search for general principles, by a deep appreciation of historical roots, and especially by an overwhelming drive to discover and reveal the logical structure of the subject.

While in Ann Arbor in the summer of 1944, Truesdell had discovered new expansions of the power-Dirichlet series

$$\phi(x, s) = \sum_{n=1}^{\infty} \frac{x^n}{n^s}, \tag{1}$$

where s is rational and $0 < x < 1$. He published this in the *Annals of Mathematics*

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[1] Lefschetz had the previous year published his classic on Algebraic Topology, but he was beginning to become interested in differential equations.

[30], his only comment as to the origin of the problem being that Mr. H. Jacobson had informed him that this function played an essential role in his researches on the structure of polymers. He developed this work ‘in his leisure hours’ at MIT in 1944–45 and at the Naval Ordnance Laboratory in 1946, leading to other short notes and to a monograph [32] published in 1948 in which he developed a general theory based on the functional equation

$$\frac{\partial}{\partial z} F(z, \alpha) = F(z, \alpha + 1), \tag{2}$$

“which motivates, discovers and coordinates seemingly unconnected relations among familiar special functions”, 35 of which he lists in the opening chapter. This monograph, dedicated to the memory of Bateman, who had commented on the manuscript and supplied references, is interesting both for the light it throws on Truesdell’s background in classical analysis, and as an example in a completely different field from continuum mechanics of how he was drawn to methods that unify and lead to “rational methods of discovery”.

His second major work of the early period was the *Kinematics of Vorticity* [38]. In it Truesdell presented the theory of vortex motion as a branch of the kinematics of continuous media, “without reference to any dynamical principles or special models”. Already, the advantages of a strict separation of kinematics, balance laws and special models, later called constitutive equations, emerge.

In the short period 1945–1952 were germinated many key ideas of nonlinear continuum mechanics. These years were infused with the excitement that springs from compelling open questions, unmistakable advances, and promising new approaches, but were equally marked by a certain retrospective quality. They culminated in Truesdell’s monumental paper, “The mechanical foundations of elasticity and fluid mechanics” [35]. In [50] TRUESDELL explains the background of this work:

> When, in 1946, I first began to study the foundations of continuum mechanics, within a few months I had set the whole field in order, to my own satisfaction. I quickly wrote and submitted to an international meeting an expository memoir, which was rejected. In view of the quality of the papers accepted by the same meeting, I was naive enough to be astonished as well as disappointed, and I sent the manuscript for criticism to a number of experts. Most of these did not deign to acknowledge it or reply, but two did. Mr. Friedrichs told me I had underestimated the work of earlier authors. Since my information concerning it was drawn from a number of reputable textbooks, I turned, somewhat taken aback, to the sources they cited, and then to the sources cited by these sources, and so on, until within a period of a year I found out how right he was and how little I had seen of the real issues faced by the great natural philosophers one and two centuries ago.

The “Mechanical foundations” followed a tortuous route to publication, as Truesdell goes on to explain:

> On December 23, 1948, Mr. v. Mises asked me to write a general exposition of recent theories of deformable masses ... I set to work at once and completed the article, severely condensed so as to keep within twice the space allowed, at the appointed time; on May 23, 1949, Mr. v. Mises acknowledged receipt of the final manuscript ... the publisher, after holding the manuscript for six months, decreed it had to be retyped within three weeks. Working day and night, I took this occasion to go over everything in detail and add references and brief descriptions of what I had learned in the interim. The publisher held the new manuscript for eighteen months before informing the editor that it contained too many symbols for any printer to handle; besides, it was too long and contained too many equations, footnotes and references. In particular, only citation of recent literature could be useful to scientists.
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> I withdrew the article from the publisher who had held it for a year and a half, but he refused to return it, having suddenly discovered that it could be printed after all. Again working day and night, within a month I had reconstructed it from an imperfect copy and old notes, adding parenthetically much new material.

The delay in publication explains the somewhat hurried treatment of the work of Rivlin, which was remedied by the extensive corrections in 1953 and 1954. Of "The mechanical foundations" Ericksen recalls, "It was full of ideas which were new to me, as well as being critical and scholarly. I had been exposed to various equations alleged to describe aspects of nature in engineering and science and I had learned how good mathematics could be used to develop them, but I had learned very little about reasoning used in arriving at the equations".

The seeds of these advances were already planted in the influential work of Reiner [23] of 1945, which also was significantly flawed in physical, conceptual and mathematical detail. Truesdell developed the ideas in obscure memoranda issued by the Naval Ordnance Laboratory. In Memorandum 9487, he considers the dissipation function to depend on a reference temperature, a reference viscosity, and a quantity that has the dimensions of the gas constant, together with the mean and thermodynamic pressures, the temperature, the stretching tensor, the vorticity, the derivatives of pressure, temperature and external force field, and the molar concentrations of the components. Adopting a philosophy that was to become central to nonlinear continuum mechanics, he used principles of isotropy, and of tensorial and dimensional invariance to restrict the form of the stress tensor.

From RIVLIN's work [24] of this period on finite elasticity and on non-Newtonian fluid mechanics, it became appreciated that nonlinear problems could actually be solved to yield precise and interesting conclusions on the behavior of materials. In the theory of finite elasticity Rivlin brilliantly foresaw the consequences of isotropy and especially incompressibility: the presence of the free hydrostatic pressure in the equilibrium equations enabled him to find several exact solutions for general isotropic incompressible materials that conveyed so much more about nonlinearly elastic materials than a nonlinear stress-strain curve. Many of these results were independent of the form of the strain-energy function. Beginning with

Reiner's seemingly natural[2] generalization of an incompressible Navier-Stokes fluid to the nonlinear regime, Rivlin ingeniously foresaw the simplifications afforded by viscometric motions. These results were presented in Truesdell's "Mechanical foundations" as part of a unified framework of continuum mechanics in which the consequences flowed from the assumptions by systematic mathematical reasoning. It was the presence of this simple and clear logical structure, leading to beautiful exact results like Rivlin's, that contributed so much to the development of the fields of finite elasticity and non-Newtonian fluid mechanics over the next 15 years.

It was in " The mechanical foundations" that Truesdell first presented his theory of hypoelasticity, later developed in [40], a rate-type constitutive theory based upon a linear relationship between an objective stress rate and the symmetric part of the velocity gradient via a fourth order isotropic tensor depending on the stress. This theory gives rise to some of the fundamental features of plasticity, and these connections, as well as applications to specific materials, continue to be explored.

Truesdell sought a theoretical framework for continuum mechanics that was conceptually clear and deductive, like that of Euclidean geometry. The first indication of the principles that would underlie this structure was in an important paper of Oldroyd [19] in 1950, who then worked at Courtaulds Research Laboratory in Maidenhead. In 1955 this was clarified, generalized and organized in a form that remains largely unchanged to this day, right down to the notation, by Truesdell's student Walter Noll in his thesis, "On the continuity of solid and fluid states". Noll, having spent a year at the Sorbonne and trained as a mechanical engineer before arriving in Bloomington, remained a close associate of Truesdell well into the 1980s. Truesdell rarely missed an occasion to advertise his work.

Little known outside a relatively small group of researchers, but highly influential within that group, is Truesdell's work of 1957 and 1961 on mixture theory [44,47], published originally in Italian[3]. In the first of these papers, he establishes the fundamental field equations for the motion of a mixture of homogeneous constituents that are allowed to flow, diffuse and react. This format seems to be followed by nearly all subsequent workers in this subject. In the second, he proposes a constitutive equation applicable to the case of no chemical reaction.

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[2] While it was not fully appreciated until some 10 years later, the Reiner-Rivlin fluid, based on the constitutive relation for the determinate part of the Cauchy stress tensor $\mathbf{T}$,

$$\mathbf{T} + p\mathbf{I} = \alpha_0\mathbf{I} + \alpha_1\mathbf{D} + \alpha_2\mathbf{D}^2, \tag{3}$$

with the α_i functions of the principal invariants of the stretching tensor $\mathbf{D}$, was found to be too special to describe the flows of typical polymeric fluids. However, Rivlin's exact solutions, later generalized to a broad collection of fluids by Coleman & Noll [4], formed the basis of the theory of viscometry. Until the "hole error" was finally corrected in 1969 by Tanner and Pipkin [29], the fundamental study of viscoelastic fluids was shaken by a variety of unexpected twists and turns.

[3] Truesdell's "Sulle basi della termomeccanica" [44] is reprinted in translation in [51]. It is one of the few of his own papers that he chose to include in that four volume series of reprints.

In 1955[4] Truesdell called attention to his Hauptproblem: determine the restrictions on the strain-energy function that guarantee reasonable behavior. The problem was partly motivated by considerations in the classical theory of linear elasticity, where unreasonable behavior was associated with Lamé constants λ and μ that do not satisfy various inequalities which guarantee existence and uniqueness of solutions, or else the reality of all wave speeds. What was the generalization of these inequalities to general elastic materials? One must realize that in the mid 1950s, in spite of Truesdell's warnings that major segments of 18th and 19th century mechanics had been lost, the work of Gibbs [13] on solids had been largely if not completely forgotten, and stability issues were referred back only to the work of Southwell [27] and Hadamard [14]. Commenting in 1956 on Truesdell's problem, Noll [18] remarked that, "The precise restriction on [the strain-energy function] Σ will probably be derived eventually from the entropy principle, a version of which general enough to be applicable to general continuum mechanics remains to be discovered, too." This presaged profoundly influential work on the formulation of this entropy principle to come four years later by Truesdell and Toupin and seven years later by Coleman and Noll, but of course the conjecture turned out to be incorrect with regard to the restrictions on the strain-energy function. Even in its strongest form, the entropy principle alone did not restrict the form of the strain-energy function in any way.

The search for these reasonable restrictions led to an awakening of interest in the relations between inequalities that restrict the form of the strain-energy function, stability and thermodynamics that continues to this day. Truesdell gave an argument that if the strain-energy function of an incompressible, isotropic material is represented as a function $W(I, II)$ of the principal invariants, then

$$\lambda_i^2 \frac{\partial W}{\partial II} + \frac{\partial W}{\partial I} \geqq 0. \tag{4}$$

Here, the λ_i are the corresponding principal stretches. Ericksen [9] showed that the inequality (4) is equivalent to the reality of all wave speeds, and Baker & Ericksen [1] went on to find that (4) is equivalent to the appealing physical notion that principal stresses are always ordered in the same way as principal stretches, except that $\geqq$ in (4) is to be replaced by $>$ if all principal stretches are unequal. In 1963 Truesdell and Toupin generalized an inequality due to Coleman and Noll, this being the following monotonicity condition for the Piola-Kirchhoff stress $\mathbf{T}_R$:

$$(\mathbf{T}_R(\mathbf{F}^*) - \mathbf{T}_R(\mathbf{F})) \cdot (\mathbf{F}^* - \mathbf{F}) \geqq 0 \tag{5}$$

whenever $\mathbf{F}^* = \mathbf{SF}$ with $\mathbf{S} \neq \mathbf{I}$ positive definite and symmetric. Reviewing this work, Payne [21] opined that, "These results, together with the two uniqueness theorems of the authors, add weight in favor of conditions of Coleman-Noll type as a material requirement, i.e., a requirement to be imposed on the form of the stress-strain relations for all strains in all elastic materials." Truesdell realized that in the nonlinear theory there are many different plausible ways of expressing the

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[4] The printed version of his lecture [41] appeared in 1956. Truesdell had earlier raised the question in less formal terms, [35, 37].

increase of stress with strain. In the Coleman-Noll inequality he saw a candidate for a condition to be satisfied by all elastic materials and which implied several plausible such restrictions. However, this view was mistaken, since it is easily seen that for an isotropic material the Coleman-Noll inequality implies the convexity of the strain-energy function expressed as a symmetric function $\Phi = \Phi(v_1, v_2, v_3)$ of the principal stretches v_i, and such convexity clearly cannot hold for an almost incompressible material, such as natural rubber, due to the non-convexity of the surface $v_1 v_2 v_3 = 1$.[5]

Insight on a deep scientific problem often comes from the least expected quarter. In retrospect, if in the early 1950s one could have brought together for a single day in a quiet room M. S. Wechsler, D. S. Lieberman, T. A. Read, C. B. Morrey, C. Truesdell and L. C. Young, one could have gone a long way toward an understanding of the meaning and depth of Truesdell's Hauptproblem. Wechsler and Lieberman would have been well prepared: on Read's advice they had just attended a course of Mindlin on continuum mechanics, and they were to apply their knowledge of that subject to a problem in phase transformations in one of the most influential papers in materials science [58]. Any reader of that paper will see the unmistakable influence of Truesdell. At the end of the day, perhaps it would have been realized that Hadamard's notions of well-posedness are far too restrictive in the nonlinear setting, that non-uniqueness and even non-existence comprise acceptable behavior, and that there are probably no fundamental restrictions on the strain-energy function at all besides those arising from material symmetry and frame-indifference.[6]

During the same period, Morrey formulated his condition of quasiconvexity [17]. This condition, when viewed as a necessary condition for a minimizer of energy, would have been seen as a natural generalization of the Legendre-Hadamard condition, whose strict form is strong ellipticity. Truesdell correctly viewed strong ellipticity as a condition of macroscopic material stability, while seeking a thermodynamic explanation for adscititious inequalities expressing the increase of stress with strain. A common approach today would be to associate adscititious inequalities with certain particular kinds of behavior of materials, or certain mathematical properties of solutions, such as the smoothness of equilibria or the absence of microstructure in energy minimizers.

Truesdell always regarded energy as a secondary concept, even after the work of Coleman and Noll that he admired so much. The primary concept for him was stress. He emphasized, for example, the distinction between the Generalized Coleman-Noll inequality (5) and the implied condition of restricted convexity of the strain-energy function that had been put forth earlier by Coleman and Noll. He remarked [48], in perhaps the only serious scientific paper published in Latin in the 20th century, that many of the exact solutions in elasticity that assumed the existence of a strain-energy function did not in fact require its existence. In ERICKSEN

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[5] It is not clear who first made this observation; one of us (JMB) learnt it in 1974 from A. C. Pipkin, who may have attributed it to Rivlin. At about the same time it was mentioned by LEE [16] in a discussion of a paper of Rivlin.

[6] Future research on deriving elasticity from atomistic models might conceivably modify this view.

& TRUESDELL's 1958 paper, "Exact theory of stress and strain in rods and shells" [10], energy functions never appear. This influential paper gives a geometrically exact treatment of strain in media described by a deformation and, independently, by a number of vectors they call "directors" attached to each material point. It also contains a full discussion of the transmission of forces through such media, independent of the choice of constitutive relations. It is the point of departure for subsequent work on Cosserat theory[7], which was virtually unknown up to that point.

In contrast to Truesdell's view, modern work on stability and its relation to restrictions on the strain-energy function has so completely embraced the framework of Gibbs that forces and stresses are almost never mentioned. Nowadays, the rapidly developing techniques of Γ-convergence and homogenization are facilitating the derivation of new continuum theories of heterogeneous media, materials with bulk and surface energy, plates and shells, materials with mismatched moduli and materials modeled as a collection of interacting particles. Remarkably, the theories that emerge often can be cast as Cosserat theories, the Cosserat variables arising as limits of certain sequences that determine the limiting energy as the small parameter goes to zero. A missing aspect of much of this recent work is a discussion of how forces are transmitted through the media described by these new theories; in this regard, perhaps it is a good time to revisit the work [10].

Truesdell's work was not confined to continuum theory, though his work on kinetic theory and statistical mechanics consistently pursued a better understanding, and more explicit forms, of the constitutive equations of continuum mechanics. He adopted an approach to the kinetic theory characterized by the direct comparison of its consequences with corresponding results from well-defined thermodynamic theories of fluids. His early work (1952) deals with dimensional analysis in the kinetic theory, in particular, the form of the dependence of the viscosity on temperature according to the kinetic theory [36]. This was followed in 1956 by two ambitious papers published in the same volume of the *Journal*, the first [15] with Ikenberry on the calculation of the contribution of the collision term to the infinite system of equations obtained by taking moments of the Maxwell-Boltzmann equation[8]. The second paper [42] comprised Truesdell's main contribution to the kinetic theory. For molecules whose force of interaction varied according to the inverse 5th power of their separation – so-called Maxwellian molecules – he found an explicit solution of the moment equations corresponding to simple shearing with uniform temperature. This solution is remarkable because it is a rare example of an exact unsteady solution in the kinetic theory of gases. While Maxwellian molecules

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[7] Ericksen and Truesdell note that, "Except for the exposition by SUDRIA [28], this profound work of the Cosserats has attracted no attention". Ericksen and Truesdell's paper concerns only the representation and transmission of forces and moments; subsequent work has led to diverse applications of Cosserat theory, via the specification of constitutive relations.

[8] Truesdell's preference for the attribution Maxwell-Boltzmann rather than simply Boltzmann to describe the equation followed as usual from careful historical study; Maxwell had written explicitly the equations for all the moments of the equation five years before Boltzmann extracted the equation itself.

are quite special, the solution clarifies explicitly what the kinetic theory actually predicts, and does not predict, for constitutive equations. A fascinating sidelight is that the solution exhibits a normal stress difference which in fact exceeds the shear stress at low density [53]. This solution certainly should be more widely known. Truesdell's approach and contributions to the kinetic theory are summarized in his book with Muncaster [54].

The work of the 1950s on the field theories of mechanics was summarized by Truesdell, then at Indiana, and R. A. Toupin, then at the U. S. Naval Research Laboratory, in *The Classical Field Theories* [46], published in 1960. This monumental treatise is interesting for what it contains, and what it leaves out. Truesdell begins with a discussion of the derivation of field theory from atomic theory, this being particularly interesting when viewed from a modern 'multiscale' perspective. Truesdell writes[9], concerning the derivation of continuum theory from atomic theory, "The mathematical difficulties are at present insuperable," and later, speaking of the balance laws, "The formal 'derivations' of the field equations from the mass point equations of mechanics given in many textbooks are illusory, such a derivation being impossible without added assumptions which are rendered superfluous by a direct approach to the continuum. The difficulty can be avoided by a formulation of the fundamental equations as Stieltjes integrals; in essence, this was done by Euler." The article is marked by a treatment of kinematics that is towering in its scholarship and permanence, together with sections on thermodynamics and constitutive equations that may be described as tentative. Perhaps the considerable progress on both of the latter topics in the years surrounding the publication of the *The Classical Field Theories* owes much to the clarity of this work. *The Classical Field Theories* is also notable for its use of the Maxwell-Lorentz aether relations $\mathbf{D} = \varepsilon_0 \mathbf{E}$, $\mathbf{H} = \frac{1}{\mu_0}\mathbf{B}$ both inside and outside matter: modern workers will recognize this as Lorentz's assumption of the presence of the aether between the atoms, unaltered by their presence, and may thereby foresee the problem of the passage from the atomic to the field viewpoint implied by this hypothesis[10].

If there was tentativeness displayed by *The Classical Field Theories* on the subjects of thermodynamics or constitutive theory, this was demolished five years later with the publication of *The Non-Linear Field Theories of Mechanics* [49] by Truesdell and Noll. Pipkin [22], in his review of 1967 states, "This book is already the standard reference in its field, and it appears unlikely that it can ever have any close competitor." Rivlin also wrote three distinct reviews [25]. The introduction alone stands as one of the most compelling pieces of scientific literature of the 20th century. There, Truesdell states his views on the relation between theory and experiment.

> While laymen and philosophers of science often believe, contend, or at least hope, that physical theories are directly inferred from experiments, anyone

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[9] Truesdell wrote Chapters A–E, while Toupin wrote only the sections relating to electromagnetism, Chapter F and the second half of Chapter G. All parts of the work were discussed and revised jointly.

[10] That is, e.g., $\mathbf{D} = \varepsilon_0 \mathbf{E}$ microscopically, but $\mathbf{D} = \varepsilon_0 \mathbf{E} + \mathbf{P}$ macroscopically. Here $\mathbf{D}, \mathbf{E}, \mathbf{P}, \mathbf{H}, \mathbf{B}$ are, respectively, electric displacement, electric field, polarization, magnetic field, magnetic induction, and ε_0, μ_0 are the permittivity and permeability of free space.

> who has faced the problem of discovering a good constitutive equation or anyone who has sought and found the historical origin of the successful field theories knows how childish is such a prejudice. The task of the theorist is to bring order into the chaos of the phenomena of nature, to invent a language by which a class of these phenomena can be described efficiently and simply. Here is the place for "intuition", and here the old preconception, common among natural philosophers, that nature is simple and elegant, has led to many great successes. Of course, physical theory must be based on experience, but experiment comes after, not before, theory. Without theoretical concepts one would neither know what experiments to perform nor be able to interpret their outcome.

Just as *The Nonlinear Field Theories* was going to press, COLEMAN [7] constructed his general thermodynamics of simple materials, generalizing the pattern of argument used by COLEMAN & MIZEL [6]. Use was made of the general form of the Clausius-Duhem inequality as formulated by Truesdell and Toupin in *The Classical Field Theories*, the sequence of generalizations being, succinctly,

$$d\mathsf{H} \geqq \frac{dQ}{\theta}, \qquad \text{(Clausius)} \tag{6}$$

$$\dot{\mathsf{H}} \geqq \int_{\partial P} \frac{\mathbf{q}\cdot\mathbf{n}}{\theta}\,da, \qquad \text{(Duhem)} \tag{7}$$

$$\dot{\mathsf{H}} \geqq \int_{\partial P} \frac{\mathbf{q}\cdot\mathbf{n}}{\theta}\,da + \int_P \frac{r}{\theta}\,dv, \qquad \text{(Truesdell and Toupin)} \tag{8}$$

where H is the total entropy of P, $\mathbf{q}$ is the heat flux, and r is the supply of energy (e.g., due to radiation). The presence of r/θ, while not a profound generalization in itself, turned out to be critical, in that the balance of energy could be eliminated as a constraint on processes (by the arbitrary adjustment of the innocent r), which allowed easy exploitation of the Clausius-Duhem inequality (8). Later authors have debated, for and against, this essential use of a function whose presence had often been neglected in the theory.

What characterized this new interpretation of thermodynamics was the use of entropy as a primitive concept. Truesdell explains [52] in his essay of 1966, "Method and taste in natural philosophy":

> As mechanics is the science of motions and forces, so thermodynamics is the science of forces and entropy. What is entropy? Heads have split for a century trying to define entropy in terms of other things. Entropy, like force, is an *undefined object*, and if you try to define it, you will suffer the same fate as the force-definers of the seventeenth and eighteenth centuries: Either you will get something too special or you will run around in a circle.

Having contributed to, and chronicled, the spectacular success achieved by fully nonlinear constitutive relations developed during the period 1945–1965, Truesdell was naturally sceptical of restrictions on constitutive relations that are necessarily tied to assumptions of their linearity. The literature of "irreversible thermodynamics" considered expressions for the entropy production of the form (written for homogeneous processes, for simplicity)

$$\frac{d\mathsf{H}}{dt} = \sum_i J_i X_i \quad \text{where} \quad J_i = \sum_j L_{ij} X_j. \tag{9}$$

Here, H is the total entropy, J_i, X_i are labeled forces and fluxes respectively, and the second part of (9) expresses the linear constitutive relation. ONSAGER [20] had given an argument using statistical mechanics that the coefficients $L = (L_{ij})$ in (5) satisfy the symmetry relation $L = L^T$ provided the entropy H depends on state variables $\alpha_1, \ldots, \alpha_n$ and the forces and fluxes are determined by

$$X_i = \frac{\partial \mathsf{H}}{\partial \alpha_i}, \quad J_i = \frac{\partial \alpha_i}{\partial t}. \tag{10}$$

However, later authors in this field, to whom Truesdell gave the name "Onsagerists", frequently disregarded the equations (10), simply asserting the truth of the Onsager relations $L = L^T$ for various physically plausible choices of forces and fluxes. The secondary literature on the subject was horribly muddled. Since most authors did not say precisely how the forces and fluxes were to be chosen, they left themselves open to the criticism, pointed out by COLEMAN & TRUESDELL [5], that a redefinition of them, preserving the equations (9), would change L from a symmetric to a nonsymmetric matrix. There is in addition an absurd branch of this literature that asserts, erroneously quoting a paper of CURIE [8], that only tensors of the same order can be related through constitutive relations! Coleman and Truesdell explained via a simple counterexample that the symmetry of L does not follow from (9) and the Onsager relations. Truesdell's dispraise of the Onsagerists reached a climax in Lecture 7 of his *Rational Thermodynamics* [55]. Today, the validity of the Onsager relations is still warmly debated, but the presence, at least in some circles, of a well-defined theoretical framework under which they are to be proved or refuted is owed to Truesdell's criticism.

The early 1970s marked a period of consolidation for the development of nonlinear continuum mechanics. While many of the basic concepts and theories had been formulated, the subject awaited advances in analysis and an appreciation of its potential by a wider group of scientists. A preoccupation with formality crept into the subject, a notable exception being ERICKSEN's [11] influential 1975 paper on "Equilibrium of bars". As if to emphasize this sterility, the decade began with an unfortunate paper of RIVLIN, "Red herrings and sundry unidentified fish in nonlinear continuum mechanics" [26]. In this thinly veiled attack on Truesdell and his associates, Rivlin chose to emphasize seemingly minor criticisms of their approach to continuum thermodynamics, rather than to look ahead to new research directions or to face the really important difficulties that already existed in the subject.

For Truesdell, the decade was devoted largely to thermodynamics. He began with a fresh reading of CARNOT's celebrated memoir, *Réflexions sur la Puissance Motrice du Feu et sur les Machines propres à Developper cette Puissance* [2]. He emphasized that nowhere in Carnot's work were engines supposed to run slowly or near equilibrium:

> I refrain from remarks on the intestine nature of heat, irreversible changes, and internal "disorder"; and I refer nowhere to the concepts of thermal equilibrium or quasi-static process. Whatever their usefulness for motivation, these topics play no part in the formal structure of the theory for engineers who wish to see engines run, not creep. For example, the "quasi-static process" was barely mentioned for the first time in 1853 and was altogether foreign to the early work.

He based his work on Carnot's own assumption: the work produced by a body undergoing a Carnot cycle is a function of its operating temperatures θ^+ and θ^- and of the heat absorbed at the higher temperature, and he showed how this hypothesis is independent of the Caloric Theory of Heat to which Carnot chose to join it. He then built up all of classical thermodynamics on the basis of a constitutive equation for the heat absorbed, the thermal equation of state, and Carnot's axiom. His treatment of the efficiency of cyclic processes was immediately generalized by Fosdick & Serrin [12], who showed that, in the general setting of continuum thermodynamics, these efficiency estimates are more or less equivalent to the Clausius-Duhem inequality. This body of theory provides one of the most physically intuitive approaches to the second law of thermodynamics.

3. Truesdell and the Archive for Rational Mechanics and Analysis

We are fortunate to possess a detailed account [57] written by Truesdell of his founding of the *Archive* that appeared in Volume 100. In 1951, shortly after he had taken up his post at Indiana University, he had been invited by T. Y. Thomas to join with him in founding a journal to serve the growing fields of mathematical continuum mechanics and the analysis of nonlinear partial differential equations. Truesdell explained the motivation in [50]:

> In those days papers on the foundation of continuum mechanics were rejected by journals of mathematics as being applied, by journals of "applied" mathematics as being physics or pure mathematics, by journals of physics as being mathematics, and by all of them as too long, too expensive to print, and of interest to no one. The anonymous referees succeeded in displaying not only their contempt for the subject but also their pitiable ignorance of it. It was time to found a new journal, devoted especially to the foundations of mechanics and to related mathematics.

It was agreed that as the youngest faculty member, Truesdell should do all the routine labor; he had recently remarried, and his wife Charlotte became editorial assistant. The first issue of the *Journal of Rational Mechanics and Analysis* appeared in January, 1952, with Thomas and Truesdell as chief editors, and a distinguished international editorial board. It was Truesdell who had persuaded his colleagues to use the phrase "rational mechanics" on the basis of its introduction and definition by Newton in his *Principia*. On the inside front cover of the first issue was printed the relevant passage in Latin from the *Principia*, together with the quotation from Lagrange "Ceux qui aiment l'Analyse verront avec plaisir la Méchanique en devenir une nouvelle branche ..." and the following statement:

> The JOURNAL OF RATIONAL MECHANICS AND ANALYSIS nourishes mathematics with physical applications, aiming especially to close the rift between "pure" and "applied" mathematics and to foster the discipline of mechanics as a deductive, mathematical science in the classical tradition. Its scope comprises those parts of pure mathematics or other theoretical sciences which contribute to mechanics; among the included fields are all branches of analysis, differential geometry, analytical dynamics, elasticity, fluid dynamics, plasticity, thermodynamics, relativity, and statistical mechanics. Engineering applications, numerical work, perturbations, etc., are acceptable only as incidental illustrations in a paper devoted to sound mathematical theory. Empirical and semi-empirical conjectures, book reviews, notices, etc., are excluded. Each paper must meet a standard of deductive rigor set by the best work in its field. Contributed papers must contain original research. The editors may occasionally invite an expository paper in a field where they perceive a genuine need.
>
> English, French, German and Italian are the languages of the Journal. A high expository level is desired, and papers written in an excessively condensed or crabbed style will not be printed.

In 1956 relations within the Mathematics Department at Indiana deteriorated, and as a result the new head of the department removed Truesdell as Editor and changed the name of the *Journal* to the *Journal of Mathematics and Mechanics* (which later became the *Indiana University Mathematics Journal*). The five volumes of the *Journal for Rational Mechanics and Analysis* that were published contained about 4000 pages of work contributed by many distinguished authors, over 500 pages authored by Truesdell himself.

Following the intervention of Siegfried Flügge, the Editor of the Handbuch der Physik, Ferdinand Springer agreed in December 1956 that Springer-Verlag would take over publication of the *Journal* under Truesdell's sole Editorship, to Truesdell's surprise and delight. As a courtesy, Springer's personal representative H. Mayer-Kaupp wrote to the President of Indiana University to ask for permission to use the same title. When the latter expressed serious objections, it was decided to change the name to the *Archive for Rational Mechanics and Analysis*. Truesdell invited all the members of the old Editorial Board to join the Board of the *Archive*, all but three agreeing to do so, and added a few more Members to broaden its scope and increase its international representation. He also prepared the following statement of editorial policy for the Board:

> The *Archive* prints only papers written by or communicated by Members of the Board. Authors are urged to submit papers directly to an appropriate Member; Members are encouraged to correspond with authors if that is helpful in reaching a decision on the suitability of the paper or in improving it. Papers communicated by a Member bear that Member's name as vouching for the correctness and value of the paper, in the style long customary for scientific academies. Papers sent in by Members are not subjected to further refereeing.

> Contributions for pure analysis are welcome; while they need not have physical application, when such application is possible it should be developed. Similarly, papers on rational mechanics need not contribute to analysis or to practical application, but if such connections exist, they should be explained. The basic function of the *Archive* is to serve mechanics treated as a mathematical discipline and those parts of pure mathematics that are useful in mechanics. Papers on bordering fields, such as differential geometry and electromagnetic theory, will be printed at the discretion of the Members.
>
> Authors are urged to work out and explain their ideas in full. Long memoirs containing mature and comprehensive work on fundamental theories are especially welcome. Short notes on details or isolated results are less suitable unless their subject is of particular importance.
>
> Good writing is essential for the purpose of the *Archive*, and all members are requested to cooperate in securing a style worthy of the contents.

Thus the defining characteristics of the *Archive* were laid down: its scientific content, quality and ethos, the autonomy of Members of the Board, and its insistence on good writing.

The first issue of the *Archive* was published in September 1957. The statement quoted above from the *Journal* was gently modified, to the quotations from Newton and Lagrange was added one of Euler, and to the list of allowed languages Latin, but the impression was of a seamless transition from the *Journal*. Under Truesdell's editorship, and with devoted and expert editorial assistance from Charlotte Truesdell, the *Archive* became the journal of choice for the leading researchers in continuum mechanics and related analysis, especially partial differential equations, the calculus of variations and functional analysis, publishing a stream of classic papers in these areas, and maintaining exceptional standards of exposition and typography. Truesdell continued as Editor until 1990, when he was succeeded by S. S. Antman. For the period 1967–1985 Truesdell was joined as Co-Editor by J. B. Serrin, who handled papers whose primary content lay in analysis.

Clifford Truesdell's list of publications up to 1979 appears in Volume 70 of the *Archive*, pp. 373–393. His publications from 1980–2000 are listed at the end of this article.

4. Influence, personality and style

Prior to Truesdell's work in continuum mechanics, the subject had become characterized by complicated and confusing notation, the absence of precisely formulated general principles (especially in thermodynamics), the failure to use standard concepts from mathematical analysis in the formulation of the underlying physics, a mystification of how to confront nonlinearity and a general lack of mathematical rigor. Fifty years later there is a widespread clear understanding of the general principles, a realization of the value of formulating them and specific theories of materials with mathematical precision, and a spreading of the ideas and philosophy of continuum mechanics into broad areas of science. While many talented individuals were involved in this movement, it was Truesdell who began it and whose scientific output and leadership gave it momentum. His high profile and uncompromising stands made him the target of considerable criticism, from those who felt threatened by the new philosophy, and from others who, like Rivlin, agreed with much of it but felt that the subject was being stifled and made less relevant by excessive attention to axiomatics. In general, Truesdell did not receive the recognition he deserved for his scientific contributions from the wider mathematical and scientific community, especially within the United States.

Truesdell's scientific life was guided by strong principles. Ericksen describes these principles as follows:

1. In whatever one tries to do, one should strive to attain excellence. This requires that one set high goals and work hard to achieve them. In any worthwhile endeavor, excellence should be recognized and rewarded, so do what you can to promote this.
2. It is very important to be guided by your own judgement. Strongly held views can be unpopular, but one should not be deterred by this.
3. One should assume some responsibility for helping to separate wheat from chaff, which can smother the wheat.
4. Hone your skills in communicating with others. Even in discussing technical matters with persons having quite different backgrounds, it should be possible to make intelligible the basic ideas and results.
5. Be generous and patient with persons of lesser ability who do their best to become excellent. Sloths who don't try deserve to be scorned.

Ericksen recalls how these ideas were put into practice in the influential graduate program he organized with Truesdell at Johns Hopkins from 1961–1980, in which many of the leading researchers in continuum mechanics participated or were trained. This program was not large, involving some 14 visiting professors and senior research associates, 49 post-doctoral workers and 12 successful Ph.D. candidates. But almost all continued to do good research after they left, and a number later reported how they had adopted similar ideas and techniques to improve their own graduate programs. Truesdell would write grant proposals as broadly as possible, to allow those working with him the opportunity to chart their own course, giving them what help he could, while ensuring they worked hard. He led by trying to set a good example, praising good work done by others and ridiculing unsound or tasteless efforts. He was particularly generous to young people, and taught them not to be impressed by status or authority. He personally invited several now famous scientists to submit their theses to the *Archive* while they were still graduate students.

Truesdell was a brilliant and erudite writer, with a magisterial style. His writing stimulated interest and discussion in part because he invariably came down forcefully on one side of every issue. Perhaps he was sometimes carried away by the strength of his own writing, so that he clung to an untenable position for too long. However, he could admit errors, and was quite willing to apply the same standards of criticism to his own work as he did to others; in annotating the *Mechanical foundations* in 1966 [50], he observes that, “The material in this section is quite useless. Unfortunately it has given rise to a literature”, and later, “The entire discussion in

Sections 73–80 shows that in 1952–3 the proper invariance of constitutive equations was not understood, at least by me".

He could sift through mountains of literature, both finding gems and pointing out weak spots or outright errors. Some insight into these abilities can be gained from reading the 500 Mathematical Reviews he wrote, in which he could make insightful scientific comments, give generous praise, sometimes in unexpected directions, or be witheringly witty as he thought the occasion demanded. Here are some extracts [34]:

On a paper on isotropic elasticity:

> This paper, whose intent is stated in its title, gives wrong solutions to trivial problems. The basic error, however, is not new.

On a book concerning the analysis of deformation:

> Bearing in mind that really new ideas are not only difficult for a reader to grasp but also difficult for an author to express, the reviewer has long and earnestly sought to grasp what it is that the author spreads out in a maze of complicated diagrams, ungainly new words, quotation marks for phrases which are not quoted, italics, and innumerable dark symbols including a fantastic mournful black letter in some cases hardly identifiable. The reviewer's effort has not been successful.

On a paper on applied mechanics:

> As is customary in the literature of strength of materials, experimental evidence fully confirming the theory is presented.

On a paper on plates and shells:

> In the reviewer's opinion, all work on this subject (including his own) is purely formal, and the various results obtained by different perturbation processes cannot be shown to be right or wrong by the *a priori* arguments always employed. What is lacking is a mathematical theorem making precise the status of solutions of any given set of proposed equations with respect to corresponding solutions of the three-dimensional theory.

On another paper on shells:

> In discussing special cases the author uses an argument equivalent to the following: In a theory where terms of second and higher order in a small parameter ε are to be neglected, suppose we are confronted with the equation $u_x = \varepsilon u_{xx}$; then $u_{xx} = \varepsilon u_{xxx}$, and hence $u_x = \varepsilon^2 u_{xxx}$, but now the right-hand side is of the order neglected, so $u_x = 0$. This ingenious device for cutting the Gordian knot of a differential equation appears to be novel.

On Timoshenko's *History of strength of materials*:

> It is evident that this book is the result of great love and understanding for mechanics combined with many years of study and criticism.

On a paper on compatibility conditions for large strains:

> This paper is the fifteenth since 1902 to derive the compatibility equations for large strain. In the review [*also written by Truesdell*] of the fourteenth ...

references to the first twelve are given, the thirteenth (cited by the author) having been omitted by oversight.

On a paper of Reiner: The author appears to reject his own invariant formulation of elasticity as “not ... satisfactory to the physicist” ... In the reviewer’s opinion, the author’s paper of 1948 was on the right track while this one is on the wrong.

On a paper on finite elasticity appearing in the mid-1950s: The author continues his apparent program of obtaining complicated series solutions according to a special non-linear theory for problems for which a simple solution according to the general theory of finite elastic strain is known.

On the treatise on physical chemistry by Partington: Suppose one wishes to know what the physicists and chemists regard as viscosity, how it is measured, what the theory of the measuring instruments is, what existing theories have predicted and from what assumptions, how well the results fit the data, and what empirical formulae fit it better. All this one finds assembled, discussed, and interrelated, together with hundreds of references to the original sources, in parts VII F (gases) and VIII E (liquids) . . . In the preface to vol. 1 the author defends himself for having written a “pseudo-Teutonic Handbuch”: “ Those who dig deeply in this mine of ready-made material for their slimmer and more attractive volumes mostly omit to say where they have been for their material, and, if they give references, usually reproduce unnamed sources, all the inaccuracies being carefully copied. It is, however, a mistake to assume that a capacity to present a field of science as a whole, not merely a small part in which the author himself has worked, is incompatible with originality . . . ”.

In public lectures Truesdell could be formal, the written version often proving more readable than the lecture listenable. But as a teacher of advanced courses he was excellent – relaxed, candid, organized – and with relentless sets of problems that always illuminated the lectures. He gave year-long courses in continuum mechanics and its history, fluid mechanics, thermodynamics, statistical mechanics and the kinetic theory of gases. Truesdell kept his prodigious lecture notes up-to-date by completely rewriting them every few years: some of them were rewritten more than a dozen times.

Truesdell’s views on experiment and computation are fascinating. It is naive to surmise that he was anti-experiment: he applauded experiments of Markovitz that exposed the ultimate limitations of special theories of viscoelastic fluids, of Rivlin that gave measurements of the normal stresses in torsion, and of many who clarified the true status of the Stokes relation. But he deplored the use of experiment or physical intuition to perpetuate bad theory: “Appeal to experiment to shore up unfounded or overwrought theory is a vacant ritual, as when a pious murderer recites a brief prayer for divine protection before cutting the victim’s throat”. However, he believed that mathematics operating on concepts derived from experience was a more powerful tool than experiment for the founding of the classical theories of mechanics.

He was scornful of thoughtless computation, and his conviction hardened over time; compare his remarks above in the aims of the *Journal* with his essay of 1984, “The computer: ruin of science and threat to mankind” [56]. Ironically, computational mechanics has flourished by using the concepts that he developed and explained, indeed, those particular concepts that organized the structure of continuum mechanics. Witness the widespread use of convected coordinates, and methods that exactly preserve the objectivity of various quantities so as to avoid cumulative errors due to rigid-body rotation. Or, turn to Section 177ff of *The Classical Field Theories* to find a full exposition (and more given there could potentially be used) of the level-set method, excluding of course implementation.

Finally, mention must be made of Truesdell’s love for, and deep knowledge of, art and music. To visit his and Charlotte’s house *Il Palazzetto* in Baltimore was to visit a museum of fine art, where musical soirées of great quality were held, including recitals by their friend the celebrated harpsichordist Gustav Leonhardt. No one who had the good fortune to receive the warm hospitality of the Truesdells there forgot the experience and its style. At home, as in mechanics, Truesdell was unique.

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References

1. M. Baker & J. L. Ericksen, Inequalities restricting the form of the stress-deformation relations for isotropic elastic solids and Reiner-Rivlin fluids, *J. Washington Acad. Sci.* **44** (1954), 33–35.
2. S. Carnot, *Réflexions sur la Puissance Motrice du Feu et sur les Machines propres à Developper cette Puissance*, Bachelier, Paris, 1824.
3. A. Church, *Introduction to Mathematical Logic, Part I*, Annals of Mathematics Studies, Vol. 13, Princeton University Press, 1944.
4. B. D. Coleman & W. Noll, On certain steady flows of general fluids, *Arch. Rational Mech. Anal.* **3** (1959), 289–303.
5. B. D. Coleman & C. Truesdell, On the reciprocal relations of Onsager, *J. Chem. Phys.* **33** (1960), 28–31.
6. B. D. Coleman & V. J. Mizel, Existence of caloric equations of state in thermodynamics, *J. Chem. Phys.* **40** (1963), 1116–1125.
7. B. D. Coleman, Thermodynamics of materials with memory, *Arch. Rational Mech. Anal.* **17** (1964), 1–46.
8. P. Curie, Sur la symétrie dans les phénomènes physiques, symétrie d’un champ électrique et d’un champ magnétique, *J. de Physique et le Radium* **3** (1894), 393–415; *Oeuvres*, Gauthier-Villars, Paris, 1908, 118–141.
9. J. L. Ericksen, On the propagation of waves in isotropic incompressible perfectly elastic materials, *J. Rational Mech. Anal.* **2** (1953), 329–337.
10. J. L. Ericksen & C. Truesdell, Exact theory of stress and strain in rods and shells, *Arch. Rational Mech. Anal.* **1** (1958), 295–323.
11. J. L. Ericksen, Equilibrium of bars, *J. Elasticity* **5** (1975), 191–201.

12. R. L. FOSDICK & J. SERRIN, Global properties of continuum thermodynamic processes, *Arch. Rational Mech. Anal.* **59** (1975), 97–109.
13. J. WILLARD GIBBS, *The equilibrium of heterogeneous substances*, Vol. 1 of The Scientific Papers of J. Willard Gibbs, Longmans, Green and Company, 1906.
14. J. HADAMARD, *Leçons sur la Propagation des Ondes et les Equations de l'Hydrodynamique*. Hermann, Paris, 1903.
15. E. IKENBERRY & C. TRUESDELL, On the pressures and the flux of energy in a gas according to Maxwell's kinetic theory, *J. Rational Mech. Anal.* **5** (1956), 1–54.
16. E. H. LEE, Some restrictions on constitutive relations; discussion paper, in *Foundations of Continuum Mechanics*, (ed. J. J. DOMINGOS, M. N. R. NINA, J. H. WHITELAW), Macmillan, London, 1974, 251–256.
17. C. B. MORREY, Quasi-convexity and the lower semicontinuity of multiple integrals, *Pacific J. Math.* **2** (1952), 25–53.
18. W. NOLL, Review of: C. TRUESDELL, Das ungelöste Hauptproblem der endlichen Elastizitätstheorie, *Z. Angew. Math. Mech.* **36** (1956), 97–103; *Mathematical Reviews* **18**, 162a.
19. J. G. OLDROYD, On the formulation of rheological equations of state, *Proc. Royal Soc. Lond. A* **200** (1950), 523–541.
20. L. ONSAGER, Reciprocal relations in irreversible processes, *Physical Review* **37** (1931), 405–426; **38** (1932), 2265–2279.
21. L. E. PAYNE, Review of: C. TRUESDELL & R. TOUPIN, Static grounds for inequalities in finite strain of elastic materials, *Arch. Rational Mech. Anal.* **12** (1963), 1–33; *Mathematical Reviews*, **26** #2055.
22. A. C. PIPKIN, Review of: C. TRUESDELL & W. NOLL, The Nonlinear Field Theories of Mechanics, in the *Handbuch der Physik* Vol III/3 (ed. S. FLÜGGE), Springer-Verlag: Berlin, 1965; *Mathematical Reviews*, **33**, #2030.
23. M. REINER, A mathematical theory of dilatancy, *Amer. J. Math.* **67** (1945), 350–362.
24. R. S. RIVLIN, Large elastic deformations of isotropic materials. IV. Further developments of the general theory, *Phil. Trans. Royal Soc. Lond. A* **241** (1948), 379–397.
25. R. S. RIVLIN, Reviews of: The Non-Linear Field Theories of Mechanics, by C. TRUESDELL & W. NOLL, Vol. III/3 of the *Handbuch der Physik*, *J. Franklin Institute* **282** (1966), 338–340; *Quart. Appl. Math.* **25** (1967), 119–120; *J. Acoustical Soc. America* **40** (1966), 1213.
26. R. S. RIVLIN, Red herrings and sundry unidentified fish in nonlinear continuum mechanics, in *Inelastic Behavior of Solids* (ed. KANNINEN, ADLER, ROSENFELD, JAFFEE), McGraw-Hill, New York, 1970, 117–134.
27. R. V. SOUTHWELL, On the general theory of elastic stability, *Phil. Trans. Royal Soc. Lond. A* **213** (1914), 187–244.
28. J. SUDRIA, *L'action euclidéenne de déformation et de mouvement*, Mém. Sci. Phys. No. 29, Paris, 1935, 56 pp.
29. R. I. TANNER & A. C. PIPKIN, Intrinsic errors in pressure-hole measurements, *Trans. Soc. Rheol.* **13** (1969), 471–484.
30. C. TRUESDELL, On a function which occurs in the theory of the structure of polymers, *Ann. of Math.* **46** (1945), 144–157.
31. C. TRUESDELL, The membrane theory of shells of revolution, *Trans. Amer. Math. Soc.* **58** (1945), 96–166.
32. C. TRUESDELL, *An Essay Toward a Unified Theory of Special Functions Based upon the Functional Equation* $(\partial/\partial z)F(z,\alpha) = F(z,\alpha+1)$, Annals of Mathematics Studies, no. 18, Princeton University Press, Princeton, N.J., 1948.
33. C. TRUESDELL, On the reliability of the membrane theory of shells of revolution. *Bull. Amer. Math. Soc.* **54** (1948), 994–1008.
34. C. TRUESDELL, various *Mathematical Reviews*, in the order 12,561a, 16,307a, 11,482g, 15,579a, 14,817b, 14,1050c, 12,770b, 22 #11587, 17,315e, 14,593b.

35. C. TRUESDELL, The mechanical foundations of elasticity and fluid mechanics, *J. Rational Mech. Anal.* **1** (1952), 125–171, 173–300.
36. C. TRUESDELL, On the viscosity of fluids according to the kinetic theory, *Zeitschrift für Physik* **131** (1952), 273–289.
37. C. TRUESDELL, Corrections and additions to "The mechanical foundations of elasticity and fluid dynamics", *J. Rational Mech. Anal.* **2** (1953), 593–616.
38. C. TRUESDELL, *The Kinematics of Vorticity*, Indiana University Science Series **19**, 1954.
39. C. TRUESDELL, Editor's Introduction: Rational fluid mechanics, 1687–1765, pp. VII–CXXV of *Leonhardi Euleri Opera Omnia*, Series II, Volume 12, Zürich, Füssli, 1954.
40. C. TRUESDELL, Hypo-elasticity, *J. Rational Mech. Anal.* **4** (1955), 83–133.
41. C. TRUESDELL, Das ungelöste Hauptproblem der endlichen Elastizitätstheorie, *Z. Angew. Math. Mech.* **36** (1956), 97–103.
42. C. TRUESDELL, On the pressures and the flux of energy in a gas according to Maxwell's kinetic theory, II, *J. Rational Mech. Anal.* **5** (1956), 55–128.
43. C. TRUESDELL, Editor's Introduction: The first three sections of Euler's treatise on fluid mechanics (1766), II The theory of aerial sound, 1687–1788, Rational fluid mechanics, 1765–1788, pp VII–CXVii of *Leonhardi Euleri Opera Omnia*, Series II, Volume 13, Zürich, Füssli, 1956.
44. C. TRUESDELL, Sulle basi della termomeccanica, I, II, *Atti Accad Naz. Lincei. Rend. Cl. Sci. Fis. Mat. Nat.* **22** (1957), 33–38, 158–166.
45. C. TRUESDELL, The Rational Mechanics of Flexible or Elastic Bodies 1638–1788, Introduction to *Leonhardi Euleri Opera Omnia*, Series II, Volume II, Part 2, Zürich, Füssli, 1960, 435 pp.
46. C. TRUESDELL AND R. A. TOUPIN, *The Classical Field Theories* in the *Handbuch der Physik* Vol III/1 (ed. S. FLÜGGE), Springer-Verlag, Berlin, 1960.
47. C. TRUESDELL, Una teoria meccanica della diffusione, *Celebrazioni Archimedee del Sec. XX* (Siracusa, 1961), **3** (1962), 161–168.
48. C. TRUESDELL, Solutio generalis et accurata problematum quamplurimorum de motu corporum elasticorum incomprimibilium in deformationibus valde magnis, *Arch. Rational Mech. Anal.* **11** (1962), 106–113.
49. C. TRUESDELL AND W. NOLL, *The Nonlinear Field Theories of Mechanics* in the *Handbuch der Physik* Vol III/3 (ed. S. FLÜGGE), Springer-Verlag: Berlin, 1965.
50. C. TRUESDELL, *Continuum Mechanics I, The Mechanical Foundations of Elasticity and Fluid Mechanics*. Gordon & Breach, New York, 1966.
51. C. TRUESDELL, *Continuum Mechanics II–IV*, Gordon & Breach, New York, 1966.
52. C. TRUESDELL, *Six Lectures on Modern Natural Philosophy*, Springer-Verlag, Berlin, 1966.
53. C. TRUESDELL, The meaning of viscometry in fluid mechanics, *Annual Review of Fluid Mechanics* **6** (1974), 111–146.
54. C. TRUESDELL & R.G. MUNCASTER, *Fundamentals of Maxwell's Kinetic Theory of a Simple Monatomic Gas*, Harcourt Brace Jovanovich, New York-London, 1980.
55. C. TRUESDELL, *Rational Thermodynamics*, Second Edition, Springer-Verlag, New York, 1984.
56. C. TRUESDELL, *An Idiot's Fugitive Essays on Science*, Springer, New York, 1984.
57. C. TRUESDELL, Editorial, *Arch. Rational Mech. Anal.* **100** (1988), 9–22.
58. M. S. WECHSLER, D. S. LIEBERMAN & T. A. READ, On the theory of the formation of martensite, *Trans. AIME J. Metals* **197** (1953), 1503–1515.

Clifford Truesdell's Publications 1980–2000
See the *Archive for Rational Mechanics and Analysis* **70**, 373–393
for those up to the end of 1979.

P	Preliminary report or preprint
A	Abstract, separately published or only published version
C	Condensed or extracted version
L	Lecture concerning part or all of the contents of the main entry
RE	Reprint of an extract
T	Translation, entire

1980

[224] (Co-author R. G. Muncaster), *Fundamentals of Maxwell's Kinetic Theory of a Simple Monotomic Gas, treated as a branch of Rational Mechanics*, xxvii + 593 pp., Academic Press. New York etc.

[225] *The Tragicomical History of Thermodynamics, 1822–1854*, xii + 372 pp., Springer-Verlag, New York.

P. *The Tragicomedy of Classical Thermodynamics* (1971), International Centre for Mechanical Sciences, Udine, Courses and Lectures, No. 70, 41 pp., Springer-Verlag, Wien and New York, 1973 [Publication in this form was not authorized by C.T. and was contrary to his wishes.]

RE. The disasterous effects of experiment upon the early development of thermodynamics, pp. 415–423 of *Scientific Philosophy Today. Essays in Honor of Mario Bunge,* Dordrecht, Reidel, 1981.

[226] Sketch for a history of constitutive relations, pp. 1–27 of *Proceedings of the 8th International Congress on Rheology,* Volume 1.

[227] The nature and function of constitutive relations, pp. 9–41 through 9–44 of Volume 2 of *EPRI Workshop Proceedings: Basic Two-Phase Flow Modelling in Reactor Safety and Performance,* Electric Power Research Institute, Palo Alto, March.

[228] (Co-author J. F. Bell) Sections 3–6 of Physics of Music, pp. 666–667 of *Grove's Dictionary of Music and Musicians,* Volume 14. [Editorial revisions introduced a good many errors.]

[229] Biographies [mangled by the editor and hence all but one unsigned] of D. Bernoulli, Chladni, Euler, (co-author J. F. Bell) Helmholtz, Hooke, Lagrange, Lambert, and Sauveur, p. 628 of Volume 2, pp. 289–290 of Volume 4, p. 292 of Volume 6, pp. 465–466 and 686 of Volume 8, p. 361 and 397 of Volume 10, p. 524 of Volume 16 of *Grove's Dictionary of Music and Musicians.*

[230] Rapport sur le pli cachété N^{o}. 126, paquet présenté à l'Académie des Sciences dans la séance du 1er October 1827, par M. Cauchy, et contenant le Mémoire "Sur l'équilibre et le mouvement intérieur d'un corps, solide considéré comme un système de molécules distinctes les unes des autres", *C. R. Acad. Sci.* (Paris) **291** (Vie Académique), 33–46.

Cauchy's first attempt at molecular theory of elasticity, *Bollettino di Storia delle Scienze Matematiche* **1** (1981), 135–143.

[231] Proof that my work estimate implies the Clausius-Planck inequality, *Atti Accad. Naz. Lincei Rend. Cl. Sci. Fis. Mat. Natur.* (8) **68**, 191–199.

1981

[232] [Paroles de reconnaissance], *C. R. Acad. Sci.* (Paris) **292** (Vie Académique), 45.

[233] The role of mathematics in science as exemplified by the work of the Bernoullis and Euler, *Verhandlungen der Naturforschenden Gesellschaft in Basel* **91**, 5–22.

[234] [Translation of an unpublished text in English], Il calcolatore: rovina della scienza e minaccia per il genere umano, pp. 37–65 of *La nuova ragione, Scienza e cultura nella società contemporanea* Bologna, Scientia/Il mulino. 234R *Notiziario della Unione Matematica Italiana* **11** (1984), 52–80.

[235] The kinetic theory of gases, a challenge to analysts, pp. 321–324 of *Contributions to Analysis and Geometry* (The Philip Hartman Symposium), edited by D. N. Clark, G. Pecelli & R. Sacksteder, Johns Hopkins University Press.

1982

[236] Our debt to the French tradition; "catastrophes" and our search for structure today, *Scientia* **76**, 63–77.

T. Il nostro debito verso la tradizione francese: le "catastrofi" e l'attuale ricerca di struttura, *ibid.* 79–87. [This translation is faulty at some essential points.]

[237] Perpetual motion consistent with classical thermodynamics, *Atti della Accademia di Scienze di Torino* **114** (1980), 433–436.

[238] Fundamental mechanics in the Madrid Codes, pp. 309–324 of *Leonardo e l'Età della Ragione*, Milano, *Scientia*.

T. [gravely defective], I primi principi di meccanica nei codici di Madrid, pp. 325–332 *ibid.*

1983

[239] Euler's contribution to the theory of ships and mechanics, *Centaurus* **26** (1982/83), 323–335.

[240] The influence of elasticity on analysis: the classic heritage, *Bull. Amer. Math. Soc.* **9**, 293–310.

1984

[241] Preface to the reissue of *Handbuch der Physik*, Volume VIa, pp. V–VIII of each of the four parts.

[242] Correction of some errors published in this journal, *Journal of Non-Newtonian Fluid Mechanics* **15**, 249–251.

[243] *An Idiot's Fugitive Essays on Science: Methods, Criticism, Training, Circumstances,* xvii + 645 pp., Springer-Verlag, New York etc. Second printing, revised and augmented, xvii + 661 pp., 1987.

C. Essay 33d, with most of the quotations from *Gulliver's Travels* omitted, pp. vii–xxxix of L. Euler, *Elements of Algebra*, Springer, New York etc., 1984.

[244] *Rational Thermodynamics, A Course of Lectures on Selected Topics*, with an appendix by C.-C. Wang, second edition, corrected and enlarged, to which are adjoined appendices by R. M. Bowen, G. Capriz, P. J. Chen, B. D. Coleman, C. M. Dafermos, W. A. Day, J. L. Ericksen, M. Feinberg, M. E. Gurtin, R. Lavine, I.-S. Liu, I. Müller, J. W. Nunziato, S. L. Passman, M. Pitteri, P. Podio-Guidugli, D. R. Owen, P. A. C. Raats, M. Silhavy, C. Truesdell, E. K. Walsh, & W. O. Williams, xvii + 578 pp., Springer-Verlag, New York etc.

L,A. [of Appendix 1A] Classical thermodynamics is a mathematical science, pp. 40–53 of *Proceedings of the International Conference on Nonlinear Mechanics*, Shanghai, October 28–31, 1985, Science Press, Beijing, 1985.

[245] A puzzle divided: English and Continental chairs following a unique design of the early eighteenth century, *Furniture History (John Hayward Memorial)* **20**, 56–60 and plates 76–80.

1986

[246] A third line of argument in thermodynamics, pp. 79–83 of *New Perspectives in Thermodynamics* (workshop at the Institute for Mathematics and its Applications, University of Minnesota, June, 1983), Springer-Verlag, New York.

[247] What did Gibbs and Carathéodory leave us about thermodynamics? pp. 101–124 of *New Perspectives in Thermodynamics* (workshop at the Institute for Mathematics and its Applications, University of Minnesota, June, 1983), Springer-Verlag, New York.

[248] Classical thermodynamics cleansed and cured, pp. 265–291 of *Meeting on Finite Thermoelasticity*, Contributi del centro Linceo interdisciplinare Nazionale dei Lincei. Corrected reprint circulated in 1988.

[249] (Co-editor with C. M. Dafermos, D. D. Joseph & F. M. Leslie), Preface, p. V of *The Breadth and Depth of Continuum Mechanics*, A collection of papers dedicated to J. L. Ericksen, Springer-Verlag, Berlin etc.

1987

[250] *Great Scientists of Old as Heretics in "The Scientific Method"*, 96 pp., University of Virginia Press, Charlottesville.

[251] (Co-editor with J. B. Serrin), Preface, pp. v, vi of *Analysis and Thermodynamics*, a collection of papers dedicated to W. Noll., viii + 542 pp., Springer-Verlag, Berlin etc.

1988

[252] Editorial, *Archive for Rational Mechanics and Analysis* **100** (1987/88), ix–xxii (1988).

[253] On the vorticity numbers of monotonous motions, *Arch. Rational Mech. Anal.* **104**, 105–109.

[254] Review of U. Bottazzini's "The Higher Calculus: A History of Real and Complex Analysis from Euler to Weierstrass", *Archives Internationales d'Histoire des Sciences* **38**, 125–137.
A. Same title, *Bull. Amer. Math. Soc.* (2) **17**, 186–189 (1987).

1989

[255] Preface, pp. v, vi of *Analysis and Continuum Mechanics*, a collection of papers dedicated to J. Serrin, Springer-Verlag, Berlin etc.

[256] [Comment on the article by Charles J. Sykes on The Johns Hopkins University, September 6], *Wall Street Journal*, September 19.

[257] Newton's influence on the mechanics of the eighteenth century.
T. (by K. Hutter) Newton's Einfluß auf die Mechanik des 18. Jahrhunderts, pp. 47–73 of *Die Anfänger der Mechanik gedeutet aus ihrer Zeit und ihrer Wirkung auf die Physik*, Springer-Verlag, Berlin etc.

[258] Maria Gaetana Agnesi, *Archive for History of Exact Sciences* **40**, 113–142; corrections and additions, **43**, 113–142 (1992).

1991

[259] Foreword, Edoardo Benvenuto, *An Introduction to the History of Structural Mechanics*, (2 Volumes), Springer-Verlag, New York etc.

[260] Letter to the Editor, *Isis* **82**, 1: 311: 90.

[261] *A First Course in Rational Continuum Mechanics*, Volume 1, 2nd Edition, corrected, revised and enlarged, xviii + 391 pp., Academic Press.

1992

[262] Jacopo Riccati, un grande "Savant" del '700: Vita, Studi, Carattere, pp. 1–25 of *Riccati e la Cultura della Marca nel Settecento Europeo, (Atti del Convegno internazionale di Studio, Castelfranco Veneto, 5–6 aprile 1990)*, edited by Gregorio Piaia & Maria Laura Soppelsa, Leo S. Olschki, Firenze.

[263] Cauchy and the modern mechanics of continua (1989), *Revue d'Histoire des Sciences* **45**, 5–24.

[264] Sophie Germain: fame earned by stubborn error, *Bollettino di Storia delle Scienze Matematiche* **11**, 3–24.

[265] Functionals in the modern mechanics of continuua, Convegno Internazionale in Memoria di Vito Volterra (1990), *Atti dei Convegni Lincei* **92**, 225–242.

1994

[266] A modern exposition of classical thermodynamics, Professor Cesare Codegone, Scuola di Ingegneria, *Atti della Accademia delle Scienze di Torino* **128**, 71–94.

1995

[267] Jean-Baptiste-Marie Charles Meusnier de la Place (1754–1793): an historical note, *Meccanica* **31**, 607–610.

1996

[268] Editorial, The Thirty-Fifth Anniversary of this Archive, by the Editor, *Archive for the History of the Exact Sciences* **50**, 1–4.

2000

[269] (Co-author K. R. Rajagopal), *An Introduction to the Mechanics of Fluids,* xiv + 277 pp., Birkhäuser, Boston etc.

C. Truesdell, 1972

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