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Dedicated to Professor J.N. Reddy on the Occasion of His 75th Birthday

MISATTRIBUTIONS AND MISNOMERS IN MECHANICS: WHY THEY MATTER IN THE SEARCH FOR INSIGHT AND PRECISION OF THOUGHT

J. N. Reddy[1,*], Arun R. Srinivasa[1]
[1]*Center of Innovation in Mechanics for Design and Manufacturing,*
J. Mike Walker '66 Department of Mechanical Engineering, Texas A&M University College Station, Texas, USA
*E-mail: jnreddy@tamu.edu

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Abstract. The purpose of this article is to bring some examples of misattributes (i.e., theories and models that bear some one's name while the idea belongs to someone else) and misnomers (i.e., words or phrases that are either incorrect or inaccurate) to the attention of the colleagues in the field, and correct them so that these incorrect phrases and attributions and misnomers are not repeated in the future writings. In the process, we also discuss the purpose of a literature reviews and the need for precision of thought in naming ideas or concepts. It is hoped that people will be careful and precise in using the words, names, and phrases correctly (since after all, these represent ideas that need to be communicated) and not propagate inaccurate information in the literature. The discussion presented is restricted to mostly structural and computational mechanics.

Keywords: applied mechanics, computational mechanics, misattributes, misnomers, shear deformation theories of beams and plates.

1. INTRODUCTION

In recent times with the proliferation of published research papers, and with the "quick search" available from the web, most people do not invest the time to refer to the original sources or think carefully about the meaning or usage of phrases introduced casually even by well-known researchers. They do not determine the validity of the assertions, and simply go on to repeat them in their citations, propagating misinformation in the literature.

It is well known that "nucleation or initiation" is a major problem in mechanics, whether it is cracks or ideas. How are ideas initiated? Who initiates them? What exactly did they mean when they initiated them. It is unfortunately all too common to attribute ideas to very well-known people even though the original ideas were initiated by someone else (misattributions). Just as common, or even more so, is the tendency to attribute a substantial generalization or major extension of an idea to the person or persons who initiated the core of an idea even though they (the original initiators) never envisaged the possible generalizations. This tendency is compounded if the originators are well-known people.

The tendency to attribute an idea to just a single person has a detrimental impact on the whole field of mechanics which is "airbrushed" and presented to a greater or lesser extent, as the march of a few "heroes" and not as the culmination of a painstaking work by many people who have "wrested" or "sculpted or carved out" an idea from the solid rock of nature. Moreover, in contrast to actual sculpting, the result is not a culmination of the vision of a single person - everyone contributes to it and the result emerges. But due to its attribution to a single source, students and new researchers then have an erroneous idea that one has to be a "hero" to have any impact and thus shy away from the area. The misnomers also encourage sloppy thinking and vagueness regarding the core idea that is being described.

It is not uncommon even for an accomplished researcher to introduce or use phrases incorrectly or inaccurately, without realizing that the phrases will be used by their followers without questioning (because they came from a perceived authority in their field). It is also true that many technical works that were carried out in the East and Eastern Europe were either not known to people in the West or they were simply ignored or dismissed because of a minor fault (whether real or imagined). In some cases, people have come up with the same idea in different parts of the world, without knowing each other's work. There are also cases, where people either suppressed or did not acknowledge that their idea came from a colleague. The authors cannot bring all of them to the readers' attention because they themselves do not know all. The spirit of this article is not to be critical of others but to bring some examples of misattributions and misnomers, mostly from structural and computational mechanics, and correct them so that they are corrected in the future writings.

2. SOME INCORRECT ATTRIBUTIONS IN STRUCTURAL MECHANICS AND THE BENEFIT FROM SEARCHING FOR ORIGINAL SOURCES

2.1. The theory of shear-constrained beams (attributed to Euler and Bernoulli)

It is usual to presuppose that the classical Bernoulli–Euler or *Euler–Bernoulli beam theory* [1] is based on a three-part hypothesis: (1) straight lines (or planes) perpendicular to the beam axis remain straight (plane), (2) the lines (planes) rotate such that they remain perpendicular to the tangent line (to the axis of bending) after deformation, and (3) the straight lines are inextensible. The consequence of the first two assumptions is to neglect transverse shear strain, and the third assumption results in the transverse normal strain being zero. The three-part assumption forms the basis of the following displacement field for bending of straight beams in the xz-plane (i.e., bending about the y axis)

$$\begin{aligned} u_x\,(x,z,t) &= u(x,t) + z \cdot \frac{\partial w}{\partial x}, \\ u_y\,(x,z,t) &= 0, \\ u_z\,(x,z,t) &= w(x,t), \end{aligned} \tag{1}$$

where (u_x, u_y, u_z) are the total displacements along the three coordinate directions (x, y, z), respectively, with x being the coordinate along the length, passing through the geometric centroid of the beam, z being the vertical coordinate, and y being the coordinate into the plane of the page. Once the displacement field is identified, one can compute the strains (including the von Kármán strains) and use Hamilton's principle to obtain the associated equations of motion which include the principal inertia as well as the rotary inertia.

However, in reality this was not the approach of Bernoulli and Euler at all. In fact, they made no assumptions regarding cross sectional deformations. Rather, in 1691, Jacob or James Bernoulli (1654–1705) stated the so-called "elastica" problem—to find the shape of a thin beam (modelled as a curve and not as a 3-D continuum) subject to a certain end load. Daniel Bernoulli (1700–1782), a son of Johann Bernoulli and nephew of Jacob Bernoulli, rephrased it as a variational problem - minimizing the integral of the squared curvature. Finally, his friend and classmate Leonhard Euler (1707–1783) (both studied under Johannes Bernoulli) developed the entire calculus of variations approach [2] and solved this and 100 other problems using his method (which generated the "Euler equations" that correspond to the first variation). In this book [2] is also found the first actual statement of the Principle of Least Action (which is erroneously attributed to Maupertius). In reality, according to [3], Euler had written to Bernoulli about the Principle of Least Action in 1743 and his work [2] was sent to publishers in Dec 1743. Maupertius's work was presented in April 1744, Euler made no effort to claim credit since that was not his philosophy to take "credit" [3].

The considerations of the deformation of the cross-section dates to an earlier period beginning with the attempts by Leonardo da Vinci [4] to study and describe mechanics of beam bending, in his work on mechanics (Codex Madrid I available online). It is apparent that da Vinci had accurately described the cross-sectional deformation of the beam in folio 84 of the Codex Madrid I, whose modern embodiment is in equation (1) although Hooke's law and equilibrium conditions were yet to be discovered.

The misnamed Euler–Bernoulli beam theory (perhaps more accurately called *the theory of shear-constrained beams*), although it does not account for transverse shear strain, has been used for centuries to design and build structures, which are still standing today. The success of the theory is primarily due to its simplicity and the fact that the shear stress and hence shear force (which can never be zero in a beam bent by transverse forces) is not obtained from the constitutive relations but are calculated from equilibrium considerations, that is, it is a constraint response much like pressure in an incompressible fluid.

2.2. First order shear deformation theory of beams (attributed to Timoshenko and Ehrenfest)

The simplest and earliest beam theory that accounts for a rudimentary form of transverse shear deformation is known in the literature as the "Timoshenko beam theory" [5] and it is based on the displacement field [1]

$$\begin{aligned} u_x\,(x,z,t) &= u(x,t) + z \cdot \varphi_x(x,t), \\ u_y\,(x,z,t) &= 0, \\ u_z\,(x,z,t) &= w(x,t), \end{aligned} \tag{2}$$

where φ_x is the rotation of a transverse normal line about the y axis. Again, one can derive the governing equations of motion associated with the displacement field in Eq. (2), accounting for the von Kármán nonlinear strains and rotary inertia, using Hamilton's principle. The principle also gives suitable boundary conditions for the theory [1].

It is not uncommon for people to give extra credit to famous people for things they did not do or words they never spoke or said something remotely connected. In the words of Koiter [6,7] "What is generally known as Timoshenko beam theory is a good example of a basic principle in the history of science: a theory which bears someone's name is most likely due to someone else . . . " (also see [8]). Simmonds [9] noted that ". . . shear deformation effects were first introduced by Rankine [10,11] and rotary inertia effects by Bresse [12]. In his often-cited paper of 1921, Timoshenko [5] without explicit reference to either Bresse or Rankine combined these effects to create what is now almost universally referred to as the Timoshenko equations." The article by Elishakoff [13] states that the so-called Timoshenko beam theory was developed by Stephen Timoshenko and Paul Ehrenfest early in the 20th century, although there is no published paper that is co-authored by the two and Timoshenko [5] did not acknowledge his collaboration in any formal paper by including Ehrenfest as a co-author (one may consult [13] for more detailed and interesting discussion on this story).

2.3. The first-order shear deformation theory for plates

The "*Love–Kirchhoff" plate theory* is the two-dimensional version of the "Euler–Bernoulli" beam theory. The two-dimensional version of the *Timoshenko–Ehrenfest beam theory* is based on the displacement field [1]

$$\begin{aligned} u_x\,(x,y,z) &= u(x,y) + z \cdot \varphi_x(x,y), \\ u_y\,(x,y,z) &= v(x,y) + z \cdot \varphi_y(x,y), \\ u_z\,(x,y,z) &= w(x,y), \end{aligned} \tag{3}$$

where $(u_x, u_y, u_z)$are the displacements along the (x, y, z) coordinate directions, respectively, on the mid-plane of the plate, and (φ_x, φ_y) are the rotations of transverse normal lines about the y and $-x$ axes, respectively (called the generalized displacements).

The plate theory based on the displacement field in Eq. (3) is called the "Mindlin plate theory" or "Reissner–Mindlin plate theory," both titles are incorrectly attributed to Mindlin and Reissner because neither one nor together developed the theory. Some people use the loose phrase "Reissner–Mindlin type shear deformation theories" instead of stating what exactly they mean. Such phrases should be avoided as they do not inform the reader what exactly the author(s) have in mind. There were others before Reissner and Mindlin who have introduced transverse shear strains in the displacement-based theories. In fact, Reissner's works [14,15] on shear deformation theories are stress-based. That is, Reissner begins with an assumed stress field as opposed to an assumed displacement field; only in passing, he writes an assumed displacement field (see Eqs. (9a)–(9c) of [15]) but he does not use it to derive the plate theory (see Reddy [16] for a review of the displacement-based and stress-based plate theories).

The 1951 paper by Mindlin [17] cites the 1948 paper by Uflyand [18] as well as Hencky [19]. Mindlin makes use of the work of Uflyand, which is extension of the works already known to dynamics, to study vibrations of crystal plates, which is considered to be not a major contribution by today's standards because once one has the displacement field in Eq. (3), the use of Hamilton's principle gives the equations of motion. However, there is a 1947 paper by Hencky [19] and 1948 NACA report by Hildebrand, Reissner, and Thomas [20] which have elements of the shear deformation theory. The 1949 NACA report by Hildebrand, Reissner, and Thomas [20] cites the 1890 paper by Basset [21] (see also [22]). Regarding the work of Basset, the report by Hildebrand, Reissner, and Thomas [20] states that "an analysis given by Basset [21] which, in the opinion of the present authors, has not received as much attention as it deserves. The reason for this may perhaps be found in the fact that Basset's work is difficult to read and that the notation employed is somewhat complicated and, from modern standards, somewhat unsystematic." In a 1985 review of the literature on shear deformation theories, Reddy [23] states that "The literature review points out that the basic idea came from Basset [17]; Hildebrand, Reissner, and Thomas [20]; and Hencky [19]. Therefore, by referring to the displacement-based shear deformation theory as Mindlin's theory we are not giving due credit to the others. We shall refer to the shear deformation theory based on the displacement field in Eq. (3) as the first-order shear deformation theory." Thus, the phrase *first-order shear deformation theory* was first introduced by Reddy [23], and it is now more commonly used phrase for the two-dimensional version of the *Timoshenko–Ehrenfest beam theory*. Therefore, the name *Uflyand–Mindlin plate theory* suggested by Elishakoff [13] does not seem right; it is better to stay away from naming the *first-order shear deformation plate theory* after any one researcher because there are numerous people who have contributed to the theory. It is not clear to the authors why it took 25 or more years to extend the *first-order shear deformation beam theory* to plates and shells (there may be other works in between that are not known to the authors).

A key question that might arise is **"why does a researcher have to find the original source, what value does it add other than publicity or apportioning credit?"** In the authors' opinion it is not the **finding** of the "original source" (only to be upstaged by an even older source) but the **search for it** that is beneficial to the researcher. As the above examples with beams and plates illustrate, the search for the original source provides an opportunity to gain a deeper insight into the idea itself. The primary aim of a literature review is not to give "credit" - if that is the case, what about the legions of graduate students and post-doctoral fellows who do the bulk of the work - the primary aim is to explore the different schools of thought, both historically and geographically. As part of this task, it is incumbent upon scholars to curb the tendency to rename (or rebrand) what is essentially the same idea with a new name (as if it is a new idea), it is necessary for them to point out that two seemingly differently named ideas are actually the same school of thought.

We try to walk in the shoes of the other researchers and find out how they thought. ***What we learn from the masters is not what to think but how to think. This is the true meaning of scholarship in our field of choice. Modern researchers are losing out on this aspect of research by just following the letter of the law in citations and not its spirit. Citations should not be popularity contests or primarily ways of apportioning credit, but a genuine effort to find what others thought.***

The concept of ownership of ideas was not common to the ancient Greeks, nor to the ancient Indians and Chinese (and to most other cultures) - they considered ideas as gifts from God. Our own experience is that we do not really know how we "get" an idea. It is clear from the very words that we think of ideas as being given to us (we never say, "I made an idea") - so what right do we really have to claim ownership, much less credit for ideas that were given to us? We can indeed claim credit (or blame as the case may be) for ***propagating*** a school of thought—a fundamental activity of all educators and scholars.

A second question might arise in the mind of the reader: **"If it is not possible to be sure about who is the originator of an idea, how do we name these ideas?"** To this we say, why should ideas and variables be named with the originator's name? It would be much better to provide it with a descriptive phrase and do a separate (tentative) attribution. We would never write a computer program where the variables are named using the names of people - every course on computer programming emphasizes the importance of descriptive names for variables for ease of understanding and maintenance. Why do we not also follow it in other areas?

A descriptive name such as "first order shear deformation theory" immediately conveys what the approximation is, as opposed to "Basset–Hildebrand–Uflyand–Reissner–Thoms–Hencky–Mindlin ..." theory which conveys no information at all. In a similar vein it is much better to refer to "current stress" rather than "Cauchy stress" or "referential strain" instead of the "Lagrangian strain," and so on.

3. MISNOMERS AND IMPRECISE THINKING IN COMPUTATIONAL MECHANICS

3.1. Preliminary comments

Even if the proper attribution is agreed upon, then the next challenge is to identify what we attribute to them. For example, if we say "Leonardo Da Vinci was the founder of beam theory," what exactly does it mean? What exactly is attributable to them? Here we enter the realm of imprecise thinking and misnomers.

If one reads the Codex Madrid I carefully with the benefit of hindsight, one finds that the description of the kinematics of classical beams is indeed attributable to Da Vinci. A precise statement would be that Da Vinci was able to identify the mode of deformation of the cross section of a beam. This is no doubt a huge advance - it took researchers another 300 years (after Hooke's Law, differential equations, etc.) to formulate a proper theory of beams - but it is not a "beam theory." As noted above, the variational approach of Euler and Bernoulli does not actually follow the line of thought of Da Vinci, it is only the modern approaches based on equilibrium considerations that do.

A different more contemporary example in computational mechanics might illustrate the imprecise thinking in a clear way.

3.2. The finite element method, the finite elements, and finite element models

The finite element method (FEM) is endowed with the following three basic features (see Reddy [21]):

1. The total domain of the problem under consideration is represented as a collection (mesh) of a finite number of non-overlapping but inter-connected (at the boundaries of the) subdomains, called *finite elements*; the elements are of a particular geometry that allows unique derivation of approximation (or interpolation) functions. In general, a mesh (Mesh 1) of "finite elements" (the phrase refers only to the geometric shapes and their interpolation functions and not to the finite element method) is used to approximate the geometry and another mesh (Mesh 2) of finite elements is used to approximate the solution u of the differential equation being solved. It is uncommon but possible for the two meshes to be different in terms of geometry and the order of interpolation.

2. Over each finite element of Mesh 2, the governing differential equation is converted to a set of algebraic equations, called *finite element model*, using a method of approximation (e.g., Ritz method, Galerkin, least-squares, subdomain, and so on). The finite element model (i.e., the set of algebraic equations among the nodal values of the primary variable of the differential equation and its dual variable[1]) obtained is different, even when the same admissible finite element approximation of the solution variable is used, for different method of approximation for the same differential equation. In other words, there can be different finite element models of the same problem, depending on the method of approximation (e.g., Ritz finite element model, Galerkin finite element model, least-squares finite element model, subdomain finite element model, and so on). The element equations relate the nodal values of the solution to the nodal values of its dual variable of that element only.

1. The concept of "duality" is inherent in engineering and also in the FEM. Every engineering problem has duality pairs - *cause* and *effect*, sometimes more than one pair, depending on the phenomena being modeled. Examples of the duality pairs are provided by (displacement, force) and (temperature, heat/flux). One must know one of the quantities of each pair at all mesh points (in some cases, a relation between them is known without having the knowledge of either quantity). In the FEM, the system of discretized equations (i.e., finite element model) often represents the algebraic relations among the nodal values of the duality pairs.

3. The element equations are then "assembled" by combining equations of all elements in the mesh to obtain the finite element model of the whole domain. The assembly makes use of the interelement continuity of the primary variable and balance of the dual variable.

Mesh 1 of finite elements plays a role in the numerical evaluation of element coefficients, while Mesh 2 is used to approximate the solution as well as to satisfy the governing equations using a method of approximation. When Meshes 1 and 2 are the same (i.e., the same interpolation is used for the approximation of the geometry as well as the variables of the problem), we call it an *isoparameteric formulation*. Once the global set of algebraic equations (i.e., algebraic equations for the whole problem) are obtained, boundary conditions of the problem are applied and the equations are solved, as in any numerical method. All numerical methods differ from each other only in the way the global algebraic equations are obtained.

The objective of the above discussion is to inform the reader of the usage of several incorrect terms and phrases in the finite element literature (papers as well as books). Some of examples are given here.

- *The finite element method* is a process that involves several steps to obtain the final algebraic equations. Part of the process is to obtain the element equations, which we term as the *finite element model*. Thus, the "finite element method" is not the same as a "finite element model." Unfortunately, some people use these two phrases interchangeably, which is incorrect.

- It is incorrect to say the "least-squares finite element method" when the least-squares method or approximation is used to obtain the element equations. Correct usage is to call it the "least-squares finite element model." Similarly, the phrase "spectral finite element method" is incorrect; it should be the "finite element method using spectral functions," because we do not say "Lagrange finite element method" when we use Lagrange interpolations functions and "Hermite finite element method" when we use Hermite interpolation functions.

- In the *Ritz method of approximation* [22], an approximate solution to any problem (linear or not) whose governing equations can be cast in the form of minimizing a quadratic functional (or its equivalent; see Reddy [1]) is determined by substituting the approximation with unknown coefficients into the quadratic functional and minimizing it with respect to the unknown parameters. This is actually independent of the Finite Element Method (unless we treat the whole domain as a single element). The Ritz method is a true "meshless method."

- The phrase "Rayleigh-Ritz method" is a misnomer because there is no such method. We only have the Ritz method (which can be applied to a class of problems which admit weak forms) and Rayleigh quotient (which is a method for finding the eigenvalues of vibrating systems). The senior author has used the phrase in his papers and books before realizing that it is not a correct phrase.

- *Galerkin's method*, as originally introduced by B.G. Galerkin (1871–1945) [23], does not involve integration-by-parts to weaken the differentiability (whether the problem is linear or not). It is a special case of the family of weighted-residual methods (see Reddy [1]). Therefore, use of the Galerkin method results in a different finite element model than the Ritz finite element model (or weak-form finite element model [1,21]), which is the most commonly used form. Thus, one should call all finite element models that use weak forms to be "weak-form Galerkin finite element models" and not "Galerkin finite element models." Wikipedia also has this wrong and needs to be corrected (instead of explaining the Galerkin method correctly it introduces other errors). Changing or generalizing the idea of Galerkin and still calling it the Galerkin method is incorrect (i.e., if the Galerkin method is *modified* to make it the same as the Ritz method and then still calling it as the Galerkin method is incorrect because the two methods are distinctly different).

- There is no such thing as an "isoparameteric element" (i.e., an element cannot be isoparametric). It is only correct to say *isoparametric formulation* (i.e., it is a formulation in which the same finite element approximation is used for the geometry as well as the solution; Mesh 1 is the same as Mesh 2).

- The words "explicit" and "implicit" are used in connection with the solution of the final linearized algebraic equations obtained in any numerical method. These equations (after the imposition of the boundary conditions) have the form $\mathbf{Ku} = \mathbf{F}$, where $\mathbf{K}$ is the coefficient matrix (known), $\mathbf{u}$ is the vector of unknowns, and **F** is the vector of known quantities. When the coefficient matrix **K** is diagonal, the solution of $\mathbf{Ku} = \mathbf{F}$ requires no matrix inversion, and the solution becomes $u_i = F_i/K_{ii}$ (no sum on i). In such cases, we call the equations are *explicit*; otherwise (i.e., when **K** is not diagonal and requires inversion), we call the system to be *implicit*. In the finite element method, the matrix **K**, derived in a consistent manner (i.e., without using additional approximations) is seldom diagonal. When the problem is a time-dependent problem, most finite element formulations use a two-stage approximation: (1) spatial approximation to convert the partial differential equations in space and time to ordinary differential equations in time in terms of the nodal unknowns; and (2) temporal approximation to convert the ordinary differential equations in time to algebraic equations. In the finite element method, the general form of the finite element model after the spatial approximation is of the form $\mathbf{Ku} + \mathbf{C\dot{u}} + \mathbf{M\ddot{u}} = \mathbf{F}$, where the superposed dot denotes time derivative. After the use of a time approximation scheme, such as the Newmark scheme, the fully discretized equation is of the form (which is a time-marching scheme), $\tilde{\mathbf{K}}^s\mathbf{u}^{s+1} = \mathbf{F}^{s,s+1}$ where s denotes time t_s, $\tilde{\mathbf{K}}^s$ is the coefficient matrix that, in general, depends on the time step, parameters of the scheme, and on **K**, **C**, and **M**. In the FEM, $\tilde{\mathbf{K}}^s$ is never diagonal because **K**,**C**, and **M** are not diagonal. For $\tilde{\mathbf{K}}^s$ to be a diagonal matrix, two things must happen: (1) one must use a time approximation scheme that eliminates **K** and **C** from $\tilde{\mathbf{K}}^s$ and (2) diagonalize **M**. When **M** is diagonal because of the particular numerical method (as is the case with certain finite difference schemes in space), then $\tilde{\mathbf{K}}^s$ is diagonal because of the time approximation scheme chosen. Then they call such schemes explicit. However, in the finite element method, we cannot use that terminology because the scheme alone does not make the formulation explicit. Thus, one should call the formulation explicit rather than the scheme.

3.3. The finite volume method and control volume finite element methods

In the *finite volume method* (FVM), one represents a given domain as a collection of non-overlapping domains, called *control volumes*[2] (see [24–26]). Then an integral (not weighted-integral) statement of the governing equation (after invoking the Green-Gauss theorem to covert the domain integral to the boundary integral for second-order equations) is used over a typical control volume to derive the algebraic equations among the values of the variables at grid or nodal points of the domain. The algebraic equations derived using a control volume consist of nodal values from other control volumes (a notable difference from the FEM). In the FVM, inside the control volume (e.g., at the center of the control volume for uniform meshes) lies a computational node, and the derivatives of the dependent variables (e.g, dual variables) at the control volume interfaces are calculated in terms of the values of the dependent variables at the nodes using the Taylor series approximations. The resulting algebraic equations resemble a finite difference stencil, that is, a relation among values of the unknowns at mesh points on the left, right, top, and bottom (also front and back in 3-D problems) of the mesh point of interest. Then the algebraic equation (i.e., the stencil) is evaluated at all grid points of the mesh, except where the nodal value is specified, to obtain the required algebraic equations of the problem. Thus, in the FVM there is no explicit assembly of elements. The imposition of gradient type boundary conditions involves, sometimes, fictitious nodes from inside and outside the domain, and no unique methodology seems to exist for the computation of domain integrals and the imposition of boundary conditions in the FVM. Since a vast majority of developers of the FVM came from the traditional finite difference community, they tend to borrow ideas like upwind, power-law, centered difference, SIMPLE (Semi IMplicit Pressure Linked Equation), LUST (Linear Upwind Stabilized Transport), and other ad-hoc approaches in representing the nonlinear terms to make the numerical schemes perform to their satisfaction. Also, there is no concept of duality in the FVM.

The so-called "control volume finite element method" (CVFEM) (see [27, 28]) is a misnomer. The CVFEM has nothing to do with the FEM, and the phrase "finite element method" should not even appear in the title. The CVFEM is the same as the FVM except that it uses approximation functions based on triangles and tetrahedrals, similar to the elements (i.e., geometric shapes) used in the FEM.

2. It is not clear why the phrase "control volume" is used when "control domain" would have been more meaningful even in 1D and 2D problems.

Mere use of triangle and tetrahedral geometries for meshes and their interpolation functions (which existed long before the arrival of FEM or FVM) as approximation functions does not make the FVM to be a "control volume finite element method." There is a considerable vagueness and arbitrariness (because of the various ad-hoc and specialized schemes used to address nonlinearities) in the FVM and CVFEM in the evaluation of domain integrals, imposition of the gradient boundary conditions, and approximation of nonlinear terms. The method that makes use of the main idea of satisfying the global form of a balance or conservation law in the FVM and the concept of duality is the *dual mesh finite domain method* introduced by the first author [29,30].

4. CONCLUSIONS

As illustrated by the above examples, it is clear that misattribution and misnomers are but symptoms of two underlying deeper problems - both the lack of effort by researchers to find the original sources and imprecise thinking. Misattribution is due to the misapprehension by modern researchers as to the purpose of citation - is not to just a way to pay tribute (or give credit) to past work, but an effort to learn and understand their way of thinking and thus deepening one's own insight into ideas. Misnomers are due to sloppy and imprecise thinking, compounded with the "march of heroes" view of science. This tends to attribute all ideas to a few "anointed" people who are then deified rather than seeing science for what it is, a process of slow and steady work gradually resulting in a transformative idea. Since we think that great ideas must be due to a single great man (unfortunately from time immemorial transformative ideas were usually attributed to men), we then erroneously attribute it to a particular person by "elastically deforming" the facts to fit the narrative. There are issues of power, prestige, and patronage also tied up with these two problems, needless to say that both are detrimental to science and to budding scientists all over the world. We hope that this brief discussion will encourage more researchers to endeavour to dig deep and also to realise that science is a collaborative endeavour with many contributions and not works by just a chosen few.

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