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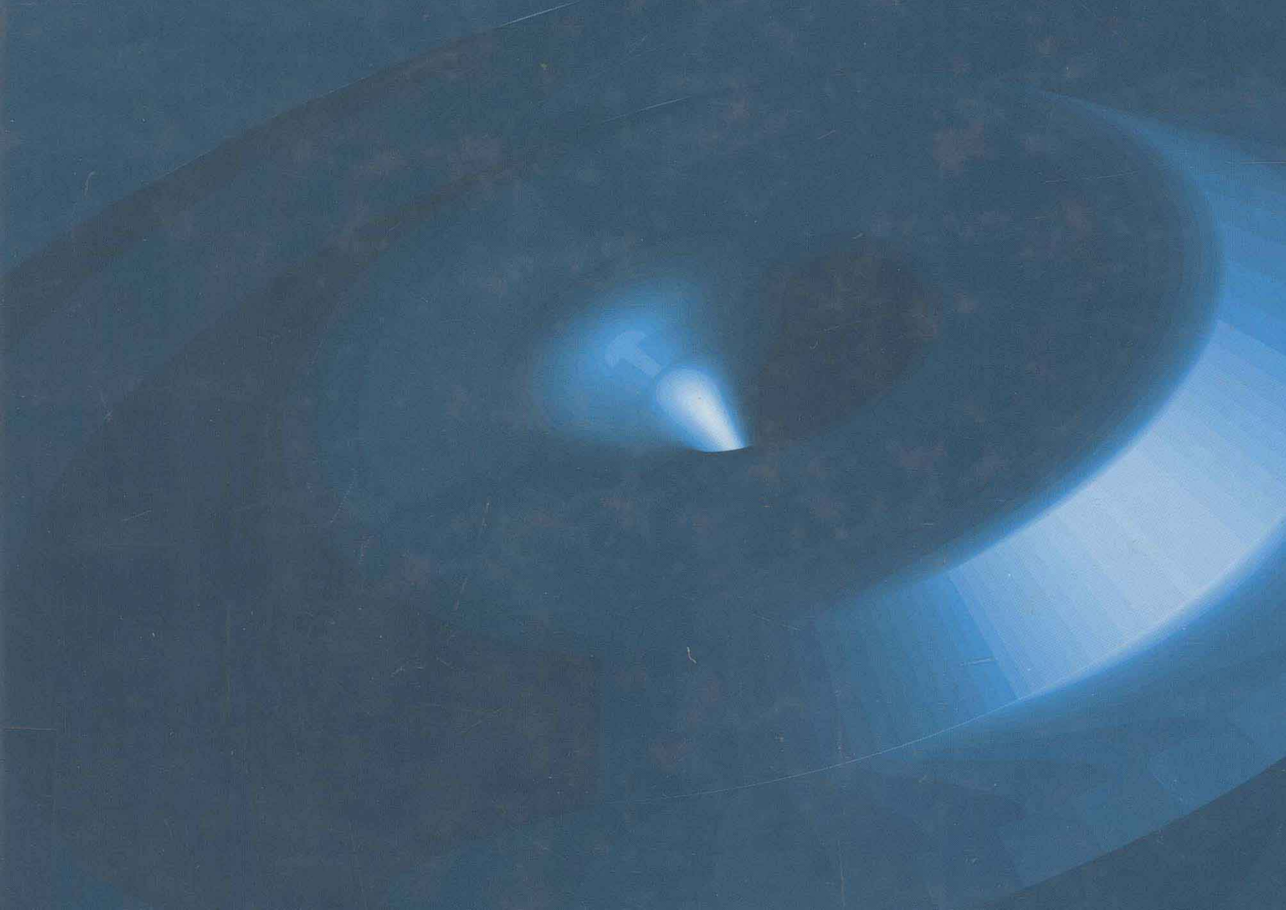

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CAMBRIDGE
UNIVERSITY PRESS

PUBLISHED BY THE PRESS SYNDICATE OF THE UNIVERSITY OF CAMBRIDGE
The Pitt Building, Trumpington Street, Cambridge CB2 1RP, United Kingdom

CAMBRIDGE UNIVERSITY PRESS
The Edinburgh Building, Cambridge CB2 2RU, United Kingdom
40 West 20th Street, New York, NY 10011-4211, USA
10 Stamford Road, Oakleigh, Melbourne 3166, Australia

First published 1997

Printed in the United States of America

Library of Congress Cataloging-in-Publication Data

Villaggio, Piero.
Mathematical models for elastic structures/Piero Villaggio.
p. cm.
Includes bibliographical references and indexes.
ISBN 0 521 57324 6 (hardcover)
1. Elastic analysis (Engineering) 2. Nonlinear mechanics-
Mathematical models. 3. Structural analysis (Engineering)
I. Title.
TA653.V55 1997 96-44172
624.1′71 DC20 CIP

A catalog record for this book is available
from the British Library

ISBN 0 521 57324 6 hardback

Elastic structures, conceived as slender bodies able to transmit loads, have been studied by scientists and engineers for centuries. By the seventeenth century, several useful theories of elastic structures had emerged, with applications to civil and mechanical engineering problems. In recent years improved mathematical tools have extended applications into new areas such as geomechanics and biomechanics.

This book offers a critically filtered collection of the most significant theories dealing with elastic slender bodies, as well as the mathematical models, that have been developed over time. The book also shows how these models are used to solve practical problems involving elastic structures with particular emphasis on nonlinear problems. Chapters progress from simpler to more complicated structures, including rods, strings, membranes, plates, and shells.

As a collection of interesting and important problems in elastic structures, this book will appeal to a broad range of scientists and engineers working in the area of structural mechanics.

MATHEMATICAL MODELS FOR ELASTIC STRUCTURES

Preface

Few words are used with so many different meanings as the term "model." In everyday language the word "model" can be applied in a moral, fashion, economic, linguistic, or scientific context; in each case it means something completely different. Even if we restrict ourselves to the category of scientific models, the notion is ambiguous, because it could signify the reproduction in miniature of a certain physical phenomenon, and at the same time present a theoretical description of its nature that preserves the broad outline of its behavior. It is the theoretical aspect of models that we wish to consider; in order to emphasize this, we describe this type of model as "mathematical" (Tarski 1953). Formulating a mathematical model is a logical operation consisting in: (i) making a selection of variables relevant to the problem; (ii) postulating statements of a general law in precise mathematical form, establishing relations between some variables said to be data and others unknown; and (iii) carrying out the treatment of the mathematical problem to make the connections between these variables explicit.

The motivations underlying the use of mathematical models are of different types. Sometimes a model is the passage from a lesser known theoretical domain to another for which the theory is well established, as, for example, when we describe neurological processes by means of network theory. In other cases a model is simply a bridge between theory and observation (Aris 1978). The word "model" must be distinguished from "simulation." The simulation of a phenomenon increases in usefulness with the quantity of specific details incorporated, as, for example, in trying to predict the circumstances under which an epidemic propagates. The mathematical model should instead include as few details as possible, but preserve the essential outline of the problem. The "simulation' is concretely descriptive, but applies to only one case; the "mathematical model" is abstract and universal. Another special property of a good mathematical model is that it can isolate only some aspects of the physical fact, but not all. The merit of such a model is not of finding what is common to two groups of observed facts, but rather of indicating their diversities. A long-debated and important question is that of how to formulate a model in its most useful form. The answer is clearly not unique, because there are examples in which the same observed phenomenon can be described equally well by two completely different models. However, in order to initiate the formulation of a good model, six precepts have been proposed (Hammersley 1973): (i) notation should be clarified; (ii) suitable units should be chosen; (iii) the number of variables should be reduced, whenever possible; (iv) rough sketches should be made and particular cases examined; (v) rigor should be avoided at this stage; and (vi) equations should be adjusted to have roughly the same number of terms on each side. Among these rules the third is by far the most important. The essential elements needed to describe a physical phenomenon should initially be isolated. The description is not improved by adding new terms to them. All the theories that have contributed decisively to the progress of physics are remarkable for their simplicity. If we look at the history of mechanics we see that the most important advances conform to the Baconian criterion of *dissecare naturam*, that is to strive to retain only those ingredients in a model that give the answer we require from a specific physical event.

The theory of elastic structures is, by definition, the collection of all reasonable models, proposed during almost three centuries, concerned with simplifying the solutions of problems involving elastic bodies. The equations describing the motion and equilibrium of a three-dimensional elastic body were formulated in full generality during the first half of the nineteenth century, but their solutions are known only in a few cases. From the beginning of the theory of elasticity the interest of many scientists was focused on the solution of the problems of the bending of a beam, the vibrations of bars and plates, and the stability of columns. Later, other problems were formulated and solved, as, for example, those concerning the torsion of a beam, the equilibrium and vibrations of thin shells, and the longitudinal impact of rods. The progress of the theory is not uniform, because we can find frequent retrogressions on the part of experimentalists in adopting hypotheses that had not been properly established, or on the part of mathematicians in using approximate methods beyond the limits of their validity. However, in the case of linear elasticity, a satisfactory degree of knowledge about the range of applicability of each of the theories has been achieved.

Nevertheless, things are radically different when the hypothesis of small strains and small displacements in elastic materials is removed. We may think that the equations of elastic structures with large deformations can be simply obtained from those valid with small strains and displacements by replacing the linear constitutive equations of classical elasticity with the constitutive equations of nonlinear elasticity, while referring the data and the unknown to the reference configuration. This is what is customarily done in deriving the equations of three-dimensional finite elasticity. In the theory of structures, however, a new aspect arises. When formulating the model of a structure, such as a rod or a plate, we try to describe it without using the three-dimensional equations. The aim is to find certain reduced equations capable of representing the essential features of the state of strain and stress in simplified form. To do this we make some conjecture about the form of the solutions, as, for example, the conservation of the planarity of the cross-sections in a bent beam, and we write certain averaged equations for the simplified unknowns. The procedure is perfectly rigorous, provided that the hypotheses are coherent and physically plausible. Unfortunately, the range of applicability of assumptions of this kind is very narrow. It may happen that it succeeds if the strains are infinitesimal, although not when there are large strains, and vice versa. One of the reasons why the theory of structures is exposed to serious criticisms is that, very often, a set of equations valid under certain restrictions has been illegitimately extended to different situations.

In the history of mechanics of structures the use of models incapable of giving results for suitably large extensions is very frequent. Theories perfectly sound for thin rods have been applied to thick rods or to thin-walled beams; the theory of Kirchhoff's plate has been extrapolated to thick and sandwich plates; and methods of solution well established for curved membranes and shells in small deformations have been employed in the presence of large deformations, with arbitrary adjustments that invalidate the results. Those who have proposed these generalizations have often realized the weakness of some arguments. However, instead of setting up new, consistent, formulations of problems, they have limited themselves to adding small corrections to the old models, with the consequence that they have proposed dubious theories from both physical and logical aspects.

This kind of attitude has been common for several decades, save for a few praiseworthy exceptions, although remarkable change has occurred in the last twenty years. Though the scientific literature is still abundant in old-style articles, we can now find a number of novel contributions. These are important in two respects: they pay more attention to the formulation and to the treatment of the mathematical problem, drawing on developments in modern mathematics; and they tackle new problems arising in the fields of technology or everyday life. An example of the first type of advance is seen in the application of bifurcation theory. This allows the characterization of solutions of a nonlinear problem even when they are very far from those of the corresponding linearized problem. An example of the second development comes in the extended application of the theory of structures to new fields, such as those of geomechanics, biology, and medicine.

The purpose of this book is to give an account of the advances in the theory of structures in both directions. The number of papers actually published on subjects involving the theory is so vast that it is physically impossible to enumerate them. The selection of the most significant contributions is naturally conditioned by the taste and prejudices of the author. It is therefore natural to ask what criterion has been used in selecting or rejecting papers. The papers that were rejected were judged on their feature of offering only "slight generalizations." Very often the essential features of an important mechanical problem were isolated and formulated in mathematical equations more than a century ago. Since then much work has been put into adding small corrections to improve the physical validity of the system. The result has been an immediate complicating of the mathematics. In general, there are two types of development in a certain field. Sometimes the problem is an old one, but treated now by a new mathematical technique that allows the properties of solutions to be illustrated. In other cases an unsatisfactory analytical treatment of an important mechanical problem has prompted questions normally considered outside the domain of engineering. There are also papers that have the distinction of encompassing both these aspects, resulting in new, original, problems treated with elegant mathematics.

A book of this kind is the result of many direct and indirect contributions from friends, correspondents, and the authors I have read. However, one person deserves special mention. Ernest Wilkes has been a continual source of support throughout the preparation of this book, and the extraordinary care with which he has offered suggestions for improvements to the entire manuscript has been decisive. I wish to express my warmest thanks to him.

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Introduction

What does it mean to solve a problem in classical elasticity? The question may appear trivial, but, if we ask scholars working in the field, we receive surprisingly different answers. Let us assume, in order to make the subject more explicit, that the problem concerns the impact between two elastic bodies. For an experimental physicist solving the problem means interpreting those crucial experiments that make it possible to decide which are the important variables in the phenomenon: in this specific case, the densities and elastic moduli of the materials are important, but the temperature and atmospheric pressure, for instance, are not. A theoretical physicist will say instead that the solution consists in formulating the general equations of the problem, having inserted all the significant variables. For a mathematician it will be obvious that solving the problem means finding an existence, uniqueness, and possibly regularity, theorem for the equations of elastic impact. Yet another answer will be given by an engineer, who will require an explicit formula giving the stress components within the two bodies at each point and at each instant.

Confronted with such a variety of answers, a typical student feels disoriented, being immediately aware of the basic ambiguity in the way in which the question itself has been posed. Ludwig Wittgenstein would say in explanation that the confusion arises from the vague use of the verb "to solve." For the same word has been used in different contexts with different meanings.

Though dissimilar, the four answers have a common characteristic. They represent four attempts at describing the same phenomenon by abstraction, setting aside the unessential details, with the purpose not merely of illustrating but also of predicting. We say that they propose four models for the elastic impact. At this point we immediately ask whether there is a rational criterion for deciding which model is preferable, provided that all satisfy the three necessary requisites of being realistic, logically coherent, and simple. It is evident that a model must not be in obvious conflict with the physical data, nor must it be self-contradictory or too complicated. However, unfortunately, there is no incontrovertible way of establishing that one model is better than another. Setting up a model means creating conceptual conditions suitable for posing a particular question about the problem. The choice of these conditions then depends on the kind of answer we wish to obtain. For instance, if the question we ask ourselves about the elastic impact is one concerning the qualitative interdependence of some quantities, then the rough tests of the experimentalist represent the right method; if, alternatively, we want to know the detailed distribution of stresses on the surface of contact of the two colliding bodies, then the formulae proposed by the engineer work better.

However, even if a rigorous science for formulating good models is lacking we have at least the comfort of returning to the traditional analysis of those models that have been most deeply involved in the development of continuum mechanics, as, for example, the Euler equations of motion of perfect fluids, or the Navier equations of classical elasticity. Besides the three necessary properties of realistic representation, coherence, and simplicity, these models possess two other qualities. The first is that there is a surprising harmony between the originality of the physical problem that must be described and the novelty of the mathematical method by which the problem is treated. Newton's second law is a model for explaining the motions of heavenly bodies, but, at the same time, it requires the solution of ordinary differential equations of second order, a problem which was just at that time beginning to be studied. On the other hand, there are several examples of physically interesting problems treated with primitive mathematics, or of insignificant problems accompanied by a brilliant mathematical manipulation. Using an adjective coined by Nietzsche, this first quality may be called the "Apollonian" attribute of a good model. But this is not all. The great models of classical mathematical physics are characterized by a second quality which may be called "reproductive," because we devise from them the methodological example for tackling new problems. A typical instance of this is found in the classical theory of elasticity, which, as Love (1927) says in the Historical Introduction to his treatise, is not only important for its contribution to the material advance of mankind, but also, and more importantly, for the light thrown on other branches of the physics of other areas, such as optics and geomechanics.

It would appear that, at this point, the question of finding a good model does not necessarily depend on these terms. We cannot hope to find an infallible recipe for suitably formulating and treating all the problems proposed by physics, but, at least, we have inherited from earlier work a significant number of examples suggesting means of dealing with those that interest us. However, in spite of this, it is hardly likely that these examples will provide satisfactory solutions to fresh problems. We must also consider that the great men of the past, when unable to find a general closed solution, have often reformulated their problem in more manageable terms, even at the cost of convoluted approximations. This is the so-called path of "reasonable" approximations. When confronted with a problem of too great a complexity, simplification is necessary; we can achieve this, for instance, by eliminating the residual non-linear terms, which jeopardizes the application of classical procedures, or by changing the shape of the original domain into one of more regular form. All this implies the substitution of the primitive model with a new one, and means that the new model must be reconsidered *ab initio* from the point of view of physical completeness and mathematical consistency.

Models thus generate submodels, thereby creating the possibility of multiplicity, and deciding on their suitability can cause confusion. But here again examples from the past give us encouragement. Workers such as Kelvin (1848), Hertz (1881) and others, have passed into history for their contributions to the general principles of mechanics, but are not always appreciated for their formidable capacity in making compromises. In order to evaluate the effect of a force acting at a point in an indefinitely extended solid, Kelvin considered the case in which body forces act within a finite volume T and vanish outside it. He constructed the solution of the problem and then passed to a limit by diminishing T indefinitely and supposing that the resultant force with T has a finite limit. The procedure seems tortuous, but the final solution is very simple. Hertz, in his solution of the problem of the pressure between two bodies in contact, assumed that the compressed area, common to the two bodies, is an ellipse, and that, provided this area is small, each body may be regarded as a half-space loaded over the bounding plane.

We are tempted to believe that, in mechanics, reductions like those of Stokes and Rayleigh are the outcome of long reflections, but it has not always been so. To be more precise, it was so until the end of the last century, but after that, those working in mechanics, pressed by the need to achieve manageable solutions, have preferred to accept the technical artifices, without submitting them under critical scrutiny. The treatises of the nineteenth century, such as those of Clebsch (1862), Kelvin and Tait (1867), and Love (1927), are full of mathematical development, but still maintain a critical grasp whenever any new approximations are introduced. More recent books, such as those by Sommerfeld (1944) and Landau and Lifschitz (1971), only occasionally state the postulates introduced during the process of deriving the equations. Books written for engineers are even less punctilious and all this leaves the reader in a state of permanent disorientation. There are, of course, some brilliant exceptions: for instance, a paradigm of deduction of the linearized equations of the motion of a string, with exemplary justification of all assumptions, can be found in the book by Weinberger (1965), which is not a book on elasticity, but a course on partial differential equations!

Thus a sort of no-man's land has been created between the principles of elasticity and their applications. And this terrain is shaky, either because one fears having somewhere violated the basic principles of mechanics and thermodynamics in conjecturing some simplifying property of a solution, or, because, given the arbitraries of choice, one is constantly afflicted by the doubt that another much simpler but more elegant model ought to have been used instead. However, in spite of possible misapplications, the technical literature is rich in contributions proposing expedient methods of treating more general problems. There are theories of plates and shells derived from the three-dimensional equations of elasticity through expansions of the displacements as functions of the first, second and third order of the distance from the middle surface; theories of finite deformations with unjustified linearizations; and semi-inverse solutions in elastostatics for domains having noncylindrical forms. A great number of these attempts are mediocre, but some have contributed greatly to the development of technical mechanics. On the other hand, books on general mechanics are reluctant to supply the general equations with some practical application, as if the analysis of the motion of a toothed wheel or of a bicycle would compromise the elegance of the book. An attempt to fill the gap has been made by Szabó (1963, 1964), whose work ranges from celestial mechanics to plasticity theory with surprising versatility, and above all with a taste for applications. But the work is too fragmentary: it does not develop a method, but rather offers only a collection of smart artifices in solving particular problems.

The divisions in an intellectual discipline are always pernicious. They damage the foundations of mechanics because the objects being systematized, whenever they have lost any connection with experience, become the "golden mountains" of which Russel (1903) speaks. In applications, they are even lethal. For historical reasons, in continuum mechanics subdivisions have been created between, roughly, the mechanics of incompressible fluids, of compressible fluids, and of solids. This division is necessary because the specific problems of each sector are so particular that an *ad hoc* procedure must be employed in their solution, as, for instance, in the theory of surface waves or in that of elastic structures. But these methods are in some cases so cogent that they might well be employed in related sectors, where they are unknown.

An objection made to this observation is that nowadays the use of numerical calculus has overcome the difficulty of describing, in the limit, small technical detail by solving the general equations. Unfortunately, however, even precise numerical solutions are too restrictive and do give no idea of how solutions behave as data vary.

Many ambiguities arise from three tacit premises: that classical elasticity is a logical science; that logic can be raised to the level of awareness; and that thinking in mechanical terms can be refined by its intelligent application.

The logic of mechanics is not a formal logic of deductive inference having the symmetric structure of Aristotelian syllogism, nor is it an inductive logic, like that of John Stuart Mill. Using a word introduced by Peirce (1931), it is a process of "abduction," that is, of formulating a hypothesis and of deducting what would be the case if the hypothesis were true. Usually the most important advances have been achieved by workers who had a particular problem in mind, the solution of which did not obviously follow by applying the general equations. They have then started to manipulate those equations and to extract some new ones, until the solution furnished by the latter conform to their expectations. The success of the method has been ensured by two ingredients: a prior intuition of how things should evolve and, consequently, of how the solutions must be; and a sort of esthetic taste in choosing the clearest route in working through the intermediate stages.

In these processes, as there are no definite prescriptions for reaching the result, it is easier to try to identify the common ways in which errors have been made. Fischer (1969), in his brilliant essay on the logic of historical thought, has endeavored to indicate the most frequent occasion of mistake. Essentially, these are the fallacies resulting from excessive motivation; that is, the neglect of relevant quantities simply because they disturb the desired result. On the other hand, there are the fallacies of overgeneralization, which arise from the belief that a model improves, the greater the number of its constituent entities. It is clearly forgotten in this fallacy that a good model can never be exhaustive, and must be simple to be effective. There are several examples of bad models, some of which originated with illustrious authors as, for instance, the celebrated equation of Föppl–v. Kármán in describing the large transverse displacements of a thin plate subjected to pressure. The defects of the theory have been pointed out by Truesdell (1978), whose criticisms can be summoned up in the following terms: unnecessary geometric approximations, and unjustified assumptions about the way in which the stress varies over a cross-section.

In the history of mechanics the separation between theory and application has never really existed. Workers in the nineteenth century have simply followed their seventeenth and eighteenth century predecessors, who could pass from abstract mathematics to technical solutions with extraordinary ease. It is known that James Bernoulli (1694) formulated the problem of the elastic line or *elastica* of a beam, and that Euler (1744) proposed a simple theory to explain the onset of buckling in a thin strut. But they are not exceptions, since eclecticism is a characteristic of scientists less famous than J. Bernoulli and Euler. For instance, in 1823, Lamé and Clapeyron were called on to assess the stability of the dome of the cathedral of St Isaac in St Petersburg, and on which occasion they virtually re-invented the technique of slicing the dome into lunes, each of which has to have an independent equilibrium.

At this point it might by naïvely asked which have been the most creative ideas in the development of the branch of classical elasticity oriented towards applications. The answer is clearly complicated because, in general, an ingenious result is not the outcome of a single author but the result of many refinements by a sequence of authors over a long period of time. Nevertheless, let us suppose that one is required to construct a list of outstanding results produced in the last three centuries, solely with the purpose of providing a picture of the evolution of interests over time. A tentative list might be as follows: James Bernoulli's (1705) equation of the *elastica*; Euler's (1744) integration of the equation of the *elastica* and Euler's (1757) definition of buckling load; Navier's (1827) equations of three-dimensional elasticity; Cauchy's (1829) definition of elastic material; Green's (1839, 1842) definition of hyperelastic material; Saint-Venant's theory (1855) of the torsion of prisms and the invention of the semi-inverse method of solution; Kirchhoff's (1850) formulation of the boundary conditions at the edge of a plate; Kelvin's (1848) solution for the elastic displacement due to a point force in an indefinite medium; Boussinesq's (1885) solution for the half-plane normally loaded by a point force; Hertz's (1881) theory of the contact between two bodies; Michell's (1904) theory of trusses; Geckeler's (1926) approximate theory for evaluating edge effects in shells; Kolosov's (1909, 1914) solution of the biharmonic equation in two variables; the three functions *Ansatz* of Boussinesq, Papkovič, and Neuber (Papkovič 1932; Neuber 1934); Vlasov's (1958) theory of thin-walled beams; Ericksen and Truesdell's (1958) director theory of rods; and Ericksen's (1973) concept of a loading device.

The list may suggest that all efforts to convert the problems created by practical applications into simple and precise terms have succeeded. But this impression is illusory, as there have also been numerous failures. For instance, Euler's (1766) theory of axisymmetric shells is not accepable; Coulomb's (1787) theory of twisting is wrong; Cauchy's (1828) theory of "rari-constants" has been contradicted by experience and thermodynamics; and Greenhill's (1881) solution for the buckling of a heavy column is incorrect, because the equation he solved neglects terms of the same order of magnitude as those maintained in the equation.

It is instructive to recognize that faults occur in the works of those who have decisively contributed to the progress of continuum mechanics: this means that mechanics is a perfectible science, and what seems a definite conquest today may be demolished tomorrow. An unknown Italian philosopher, Giovanni Vailati