depends on ideas developed between pages 170, 188. The book is an important contribution to mathematical literature. At every turn one sees the care and ingenuity which the author has used to make his proofs rigorous and readable. The book is intended for the conscientious student, and it will repay him well for the hours that he may spend with it.

A. C. SCHAEFFER

Foundations of the nonlinear theory of elasticity. By V. V. Novozhilov. Trans. from the first (1948) Russian ed. by F. Bagemihl, H. Komm, and W. Seidel. Rochester, Graylock, 1953. 6+233 pp. $4.00.

Students of mechanics will be grateful to the translators and the publishers for making available the second of the three[1] existing monographs on the general theory of elasticity—the more so, since the Russian original is in this country at least a very rare book.

The translation is unusually good English (except for "compatability") and the translators have taken unusual care that the exposition of this elaborate subject shall make sense, although they are not always familiar with the terms used in mechanics (e.g. on p. 58 they use "components of a vortex" for "components of the curl"). Despite its being planographed, and thus repulsive to the eye, the text is readable.

The author's approach is straightforward, honest, and vigorous. There is little or no nationalism, rhetoric, or pedagogery. The author gives every evidence of his earnest competence and his respect for a difficult and important group of problems. The book is not scholarly, however; most of the some ninety items in the bibliography are not cited in the text, part of which presents material first published in important papers not listed in the bibliography. It is quite possible that many of the results in this book are rediscoveries by the author himself.

This is a serious work, deserving detailed notice. The author's preface is dated 1947, and the book is on the whole a careful, accurate, and reliable exposition of some of the mechanical aspects of the classical nonlinear theory of elasticity as it stood at that date. It was in 1948 that the numerous publications of Rivlin, which have enlivened the subject and changed the whole view of it, began to appear.[2] Thus

[1] The other two are *Théorie des corps déformables* by E. and F. Cosserat, Paris, 1909, and *Finite deformation of an elastic solid* by F. D. Murnaghan, New York, 1952. The latter was reviewed in Bull. Amer. Math. Soc. vol. 58 (1952) pp. 577–579.

[2] These are briefly summarized in Chap. IV of my paper, *The mechanical foundations of elasticity and fluid mechanics*, Journal of Rational Mechanics and Analysis vol. 1 (1952) pp. 125–300; corrections and additions, vol. 2 (1953) pp. 593–616.

the book, through no fault of its author, cannot be called a definitive exposition of general elasticity, nor even an adequate introduction to it. The term "foundations" in the title is justified by the usually careful treatment of principles: the author's objective is to set up the governing equations and various approximations to them, without any attempt at solutions in special cases.

Nearly two thirds of the text, comprising the first four chapters, is devoted to the basic equations of three-dimensional finite elasticity. In Chapter I the concept of strain is developed with especial care, and various levels of approximation are carefully distinguished (esp. §§13–15). In speaking of "small elongations and shears," however, the author fails to remark that shear is not an invariant concept; what he intends is "small principal extensions."

The treatment of stress in Chapter II is not only rather slipshod but also depressingly elaborate, while to follow the fifteen page derivation of stress-strain relations in Chapter III, even though it begs the main question at issue, would require an iron resistance to boredom not easily bred in temperate climes. It is in Chapter III, §29, that we find the author's most serious oversight. While he reduces the stress-strain relations for isotropic bodies to a material ("Lagrangian") form which is rather simple in appearance, he does not mention the possibility of using spatial ("Eulerian") strain measures [Finger, Sitzber. Akad. Wiss. Wien (IIa) vol. 103 (1894) pp. 1073–1100]. It is this possibility which renders problems of large strain manageable. It seems unfortunate that Joseph Finger, the first to notice this simple but centrally important fact and to obtain the stress-strain relations whose rediscovery has made possible the striking progress in general elasticity since 1947, is unknown in the history of mechanics.

On p. 113 the author states without proof the following invariant form for the "generalized" stress matrix S:

$$S = \Psi_2 \Pi_0 + \Psi_1 \Pi_1 + \Psi_0 \Pi_2,$$

"where Ψ_2, Ψ_1, Ψ_0 are functions of the strain invariants, Π_0 [is] the unit tensor, Π_1 [is] a tensor whose components are linear combinations of the [material] strain components, and Π_2 [is] a tensor whose components are quadratic combinations of the strain components." Here the author has found a portion of the basic invariance theorem established simultaneously by Reiner [Amer. J. Math. vol. 70 (1948) pp. 433–446]: S may be taken as the true stress matrix, Π_1 as any matrix whose proper values are analytic functions of the principal extensions and whose principal axes are the principal spatial strain axes, while Π_2 may be taken as $(\Pi_1)^2$.

In §31 the author writes "It follows from the above that for every material a range of small deformations can be established for which Hooke's law is approximately valid." If one wonders how a mathematical theory could ever establish such a result, one must turn back to §30, where one finds that the strain energy has been assumed analytic because "no negative powers can appear in the series." There is no statement that an assumption has been made, not even a discussion of why fractional powers, for example, might not be appropriate, although a considerable engineering literature devoted to this possibility exists [cf. e.g. R. Mehmke, Z. Math. Phys. vol. 42 (1897) pp. 327–338]. In fact, the only experimental justification of the assumption of analyticity is the *experimental* validity of Hooke's law for many [by no means all!] materials under sufficiently small loads—but this is the direct opposite of the author's reasoning.

The author's analysis (§32) of Hencky's theory of plasticity (in this country usually considered with respect to strain-hardening, and often called "the theory of Roš, Eichinger, and Schmidt") was obtained also by C. Weber [Zeitschrift für Angewandte Mathematik und Mechanik vol. 28 (1948) pp. 189–190; vol. 29 (1949) p. 256].

In this book all results are written out at length in rectilinear coordinates. Some sets of formulae cover most of a page. In the preface we find an explanation: "To make the book as accessible to as wide a circle of readers as possible, the author has attempted to carry out all deductions in the simplest and most intuitive manner, avoiding, in particular, tensor calculus . . . ." In fact, at the top of p. 67 the word "tensor" or the idea behind it is avoided with comical precaution: apparently the reader is assumed not to have studied the classical treatises of Voigt and Love. I believe that an intelligent student completely untutored in geometry on reading this page would set himself the problem of formulating and exploring the geometric concept which the two obviously connected results so forcibly separated by the author most plainly suggest. But on p. 111 the word "tensor" suddenly appears without explanation and is used several times later. It is somewhat similar with Green's theorem, which is carefully avoided in the creaking development of the properties of the stress tensor but appears later on p. 106 (where, however, it is regarded as so extraordinary as to need two of the five references given in the first 200 pages, the others being to works on orthogonal curvilinear coordinates).

It is not in disrespect to Euler and Cauchy that I say their methods in continuum mechanics are now unnecessarily elaborate; in fact, it is in their papers on continuous media that some of the earliest discoveries in the theory of differential invariants occur, and tensor analysis is in part an elaboration of their work. But I think the student who follows in this book the endless pages of dreary resolutions and projections in the Euler-Cauchy style could better spend his time learning tensor analysis, which would enable him to reproduce four fifths of the author's work in twenty pages, while freeing his attention for the important questions and ideas which are scattered through the remaining one fifth.

Although the author founds all his analysis in the fully general theory, his main interest is in the case next in order of generality past the fully linear one, when the extensions are small, but the displacements and rotations may be large. The cause of this restriction, on which he lays considerable emphasis, is his desire to furnish structural engineers with the basic theories needed for rational solution of their nonlinear elastic problems. Since typical structural materials, such as steel, fail to retain their elastic reversibility when subjected to extensions as great as 1%, there are essentially only two such nonlinear problems: (1) elastic stability, which the author interprets as determining the smallest load at which Kirchhoff's uniqueness theorem breaks down, and (2) bending of "flexible" bodies, such as thin rods, plates, and shells. The last two chapters, the most important in the book though occupying only about eighty pages, are devoted to these two problems.

While the author's points are well taken, it is instructive to consider a simple analogy. Suppose we are to clarify the problems besetting horological engineers when their pendulums swing in a range beyond that in which the approximation $\sin\theta\approx\theta$ is sufficiently accurate. Doubtless then $\sin\theta\approx\theta-\theta^3/6$ is quite sufficient for all practical problems of this type. Accordingly, if we were to follow the practice which was nearly universal in nonlinear elasticity up to 1948 and is recommended by the author, we should devote ourselves to the differential equation

$$\ddot{\theta}+k^2(\theta-\theta^3/6)=0.$$

In so doing, we should lose all the simplicity of the linear theory, having to face at once all the complications of nonlinear mechanics—but even if completely successful, all we should have, at great cost, would be a somewhat better approximation. It is common knowledge that it is no harder to settle the whole matter rigorously by studying the exact solutions of the exact equation

$$\ddot{\theta}+k^2\sin\theta=0.$$

Now a very similar thing has happened in elasticity theory. The work of Rivlin has shown us that it is quite a feasible and practical thing to work directly with the *exact* equations for arbitrarily large strain of a material characterized by an arbitrary strain energy function. There is not only the scientific satisfaction of solving a really general problem for its own sake (cf. the last paragraph of the "Historical Introduction" to Love's *Treatise on the mathematical theory of elasticity*, 4th ed., Cambridge, 1927), but also the precision of a general analysis leads to simplicity and certainty in the end. While only relatively simple problems can be solved explicitly within the fully general theory, these particular cases are very important, and it was the light they cast upon the nonlinear theory which pointed the way to an approximate procedure valid for *all* problems of prescribed loading.[3] An example of the defectiveness of the approach usual in nonlinear elasticity is furnished by the author's formulation of the problem of elastic instability within an approximate nonlinear theory of elasticity. In the absence of a mathematical approximation theorem, we cannot assert with confidence that critical loads obtained from the author's equations approximate the critical loads which would be obtained from the general theory. But these remarks must not be taken as criticism of the author's work, which presents in a few pages a relatively simple and cogent development of the problem of elastic instability in the usually received sense.

There is some question also about the author's distinction between "geometrical" and "physical" nonlinearity (§34, and again on p. 197). For example, whether or not the rotations are large cannot be determined by "geometric considerations" *a priori*; the rotations result from loading, and (unless one is using an inverse method) one cannot know in advance whether for given loading of a material defined by a given strain energy function the nonlinear terms in the strain components will need to be retained or not. True, *after* the problem is solved the question becomes purely geometric, but if we have the exact solution then it is no longer very important whether we can neglect certain terms or not. The question of whether certain approximations are valid *in advance* is avoided by the author; its treatment would require a new type of approximation theorem for partial differential equations.

The excellent last chapter is summarized in the author's conclusion (§54):

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[3] R. S. Rivlin, Journal of Rational Mechanics and Analysis, vol. 2 (1953) pp. 53–81.

"Ordinarily, the theory of deformation of flexible bodies (plates, shells, rods) is developed by making certain assumptions which immediately reduce the problem to a two-dimensional one (in the case of plates and shells) or a one-dimensional problem (in the case of rods). However, with such assumptions one necessarily loses sight of the connection between the theory of plates, shells, and rods and the general theory of elasticity. In view of this, many people consider the theory of flexible bodies as a kind of hypothetical superstructure over the general theory of elasticity, as a foreign element in it.

"Only in this manner can one probably explain why most contemporary books on the theory of elasticity omit all mention of the problem of deformation of flexible bodies, which is of such practical importance. An attempt was made in Love's book to relate the 'hypotheses' of the theory of flexible bodies to the general theory of deformation.

"But special work in this direction was carried out by B. G. Galerkin, in whose papers the classical theory of shells and plates truly became a branch of the general theory of elasticity.

"The basic idea championed by B. G. Galerkin was that the problems of the bending of plates and shells must always be examined in the context of the general theory. This simple but profound idea was responsible to a large extent for the successful development of the theory of plates and shells in the Soviet Union and turned out to be fruitful not only in the case of thick plates and shells, but also in the case of thin plates and shells.

"It is natural to extend this idea to the nonlinear theory of elasticity, since one can expect that many results of this theory may be systematized by starting out from the general equations. The present chapter was an attempt to give a uniform method for investigating the deformation of flexible bodies on the basis of the general nonlinear theory of deformations. It was our aim to clarify, with the aid of the general equations, those 'hypotheses' on which the theory of plates, shells, and rods is ordinarily based, and to examine, from a uniform point of view, all these problems, which are ordinarily treated separately in spite of their common features."

The "basic idea championed by B. G. Galerkin" goes back to Cauchy and Poisson for the theory of plates, while for slight bending of shells it is the author himself [C. R. (Doklady) Acad. Sci. URSS. vol. 38 (1943) pp. 160–164] who has given us the first adequate treatment based on the three-dimensional theory.[4] In the present work he

[4] Similar treatments were constructed independently by R. Byrne (1941) [Sem. Repts. Math. Univ. Calif. (n.s.) vol. 2 (1944) pp. 103–152] and in my Princeton

carefully derives from nonlinear three-dimensional elasticity several of the nonlinear theories of rods, plates, shells, taking pains to show that the special hypotheses used are consistent to the degree of approximation considered. The reader not already familiar with this subject, where in the past outright inconsistent assumptions have often been made, may not realize that the author's treatment deserves the description "simple but profound."

C. TRUESDELL

The higher arithmetic. By H. Davenport. London, Hutchinson's University Library, 1952. Text ed. \$1.80, Trade ed. \$2.25.

This book is an introduction to the theory of numbers which is suitable for a very wide class of readers. On the one hand, no extensive mathematical knowledge is required of the reader; in fact, a good high-school training in mathematics would be sufficient. On the other hand, the author discusses subjects of real mathematical interest and treats them in a very readable way, so that a person of considerable mathematical maturity would find much enjoyable and profitable reading in this work.

The titles of the seven chapters are as follows: Factorization and the primes, Congruences, Quadratic residues, Continued fractions, Sums of squares, Quadratic forms, Some Diophantine equations. As can be seen from the list, a fairly wide range of material is covered. No attempt is made to treat each topic exhaustively, but the author goes far enough to enable the reader to get some appreciation of the main ideas and problems in each area. A few of the more noteworthy things to be found in the book are as follows: (1) a good presentation of the method of mathematical induction and a proof of the unique factorization theorem by this method, (2) a proof of Chevalley's theorem that an algebraic congruence in several unknowns to a prime modulus always has a nontrivial solution if the constant term is zero and the degree is less than the number of unknowns, (3) a proof of the theorem on the number of positive integers n between 1 and $p-2$ (inclusive) for which n and $n+1$ have prescribed quadratic character modulo the odd prime p, (4) a rather thorough treatment of the continued fractions of quadratic irrationals, (5) a presentation of various constructions for the two squares into which a prime of the form $4k+1$ can be decomposed, (6) a discussion (without proof) of Dirich-

dissertation (1943) [Trans. Amer. Math. Soc. vol. 58 (1945) pp. 96–166], and W. Z. Chien has asserted in a letter that the similar material in his paper [Sci. Rep. Tsing Hua Univ. vol. A 5 (1948) pp. 240–251] derives from his Toronto Thesis (1942). The idea does not appear to have taken hold in this country.