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Recent Advances in Rational Mechanics

The search for underlying concepts and strict mathematical proof deepens our understanding of mechanics.

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I begin by answering the question that will occur to many a reader upon seeing the title: What is rational mechanics? It is difficult to define rational mechanics, but no more so than to define chemistry or physics or mathematics. However, a chemist writing a survey of his field is not expected to begin by defining chemistry. The difference is that in the United States, at least, rational mechanics is not a recognized science. Indeed, there are some who disbelieve in its existence (*1*).

Relation to Other Disciplines

First, *rational mechanics is a part of mathematics* (*2*). It is a mathematical science, and in its relation to experience, intuition, abstraction, and everyday life it does not differ in essence from other branches of mathematics. There is no need to offer here a general defense for mathematics; it should also be unnecessary to point out that mathematics, however abstract and however precise, is a science of *experience,* for experience is not confined to the gross senses. Also, the human mind can experience, and we need not be so naive as to see in an oscilloscope an instrument more precise than the brain. The experiential basis of the sciences is not limited to laboratory experiment.

That rational mechanics grew out of practical mechanics and cooperated with it, if sometimes unwittingly, to produce applied mechanics and mechanical engineering is obvious. In writing the first treatise on rational mechanics, Isaac Newton (*3*) established its standard of mathematical rigor as precisely that of geometry. Not always has this standard been maintained, but today, as in 1687, it remains the ideal. Newton's comparison with geometry is enlightening, for geometry, too, grew from physical experience. To those who scoff at geometry for its precise calculations when all measurements are subject to error, the geometer for millenia has replied: Geometry is mental, not instrumental.

The analogy to geometry is a good one. That rational mechanics speaks not only of space and time but also of mass, force, and energy does not make it any less precise. Since it deals with a greater number of physical concepts than does geometry, its applications to physical problems may be expected to be more frequent and more far-reaching, but physical applications are not its objective.

But does not rational mechanics deal with quantities of physical experience? Indeed it does; so does geometry, for lengths, surfaces, and volumes are equally related to physical experience. The geometer may visualize a surface in terms of a twisted strip of paper, as in mechanics we may think of a force as a manual push, but whatever these motivations, the symbols in the equations of geometry and mechanics are precisely defined mathematical quantities. Origin in broader experience should make mechanics more interesting but not any less exact.

Might not the same be said of any part of theoretical physics? Indeed, it might; each discipline couched in mathematical symbols is ultimately susceptible of independent mathematical existence. In his famous list of problems for this century to solve, David Hilbert (*4*) included that of forming a set of axioms for mechanics, but this problem, like all those he proposed concerning the relation between mathematics and physical experience, has been neglected by mathematicians. Some of the older parts of physics are well on the way toward fully mathematical treatment, but they lie outside my scope.

Finally, does not the emergence of rational mechanics as a purely mathematical discipline imply that we have the freedom to invent new laws of mechanics as the geometers invent new spaces? In fact, the geometers are not so wildly speculative as might appear; their inventions usually arise from some experiential need, though not always one easy to explain to laymen. Purely mathematical theories of space have been in existence at least 2500 years, while in mechanics, with some exceptions, we have a tradition of only four or five hundred years, and the rational mechanics of materials, where most modern researches occur, is little over three hundred years old. The properties of mechanical laws most immediate to our experience are not nearly so well explored as are the properties of the space most immediate to our experience. Moreover, mechanics is enormously more complicated than geometry. The ordinary system of mechanical laws continues to offer us mysteries and challenges. While indeed we have the liberty

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to explore other mechanical laws, usually we do not choose to exercise this liberty except in the limited and sober way I will explain later.

That rational mechanics is a mathematical science does not mean that its standard is one of mathematical difficulty. Indeed, some great researches in rational mechanics are difficult and intricate, but others are simple. An innovation that is simple and easy to understand is especially prized. The problems are set by the subject; hard or easy, they must be solved, and their mathematical difficulty is just as much an incidental as is their physical application. After all, in this, rational mechanics is not different from other parts of mathematics, for only to the novice is the seeming complexity or difficulty of a mathematical science a measure of its level.

Is rational mechanics a part of pure mathematics? To most mathematicians today, pure mathematics means topology, abstract algebra, or analysis in abstract spaces. These, most certainly, rational mechanics makes no attempt to imitate. While in spirit it is nearest to geometry, its problems, its aims, and its methods bear little evident similarity to those of other parts of mathematics. A theorem in topology is not evaluated in terms of its bearing on the theory of numbers. It is equally ridiculous, though unfortunately not infrequent, to disparage theorems in rational mechanics when they do not also contribute to the more popular branches of pure mathematics.

Is rational mechanics a part of applied mathematics? Most certainly not. While in some cases known mathematical techniques can be used to solve new problems in rational mechanics, in other cases new mathematics must be invented. It would be as misleading to claim that each achievement in rational mechanics has brought new light to other mathematical domains as to claim the opposite, that rational mechanics is a mere reflection from known parts of pure mathematics. It is a mistake to raise the issue of applied versus pure mathematics. Indeed, it is not the aim of rational mechanics to produce methods or results that rate per se as "new" in other parts of mathematics; neither is its aim to avoid them. Equally, it is not the aim of rational mechanics to produce applications, whether to physics, to engineering, or to other parts of mathematics; neither is its aim to avoid them. *Rational mechanics is an independent branch of mathematics.* When a research in rational mechanics predicts a new physical phenomenon or produces a new analytical method, as occasionally happens, this is so much the better. But such by-products, while very welcome luxuries, are not essential. Rational mechanics, like every other distinct science, has its own aims, its own standards, its own independent problems.

Role of Rational Mechanics

There is widespread belief that in physics the basic equations governing physical phenomena are established and then it is the duty of the mathematician, or the applied mathematician, to solve them. When this view is imposed upon mechanics, we are driven to conclude that the basic equations of mechanics are in the textbooks on classical physics and all that is left is for analysts to solve them, for engineers to apply them. This leaves no place at all for rational mechanics, since rational mechanics rarely concerns itself with theorems of existence and uniqueness or with calculation of numerical answers. While such a simple cooperative division of responsibilities between mathematician and physicist may be ideal, it is unreal. Surely we may admit that biologists study biology because they prefer it to physics; thus, that physicists nowadays study nuclear physics rather than classical mechanics need not imply that classical mechanics is a dead field, but may be explained on more subjective grounds, less unflattering to those persons who do cultivate mechanics. (In simple fact, mathematicians who study mechanics today have rejected older views and have formulated the subject afresh; a part of their work has included discovery of new basic equations defining and explaining new classes of physical phenomena.) Moreover, we often hear complaints from the physicists that mathematicians study the wrong problems, neglecting those the physicists have set and pursuing, instead, flights of their own imagination. Mathematicians readily admit this charge. But if the mathematicians have failed so miserably in their half of the ideal bargain, it would be unjust to expect the other end to be supported in full. Finally, the historical fact is otherwise: The great achievements in rational mechanics of two centuries ago, now imbedded in our instruction as "classical," were wrought by a small group of persons who, calling themselves "geometers," pursued the mathematical properties of their equa-

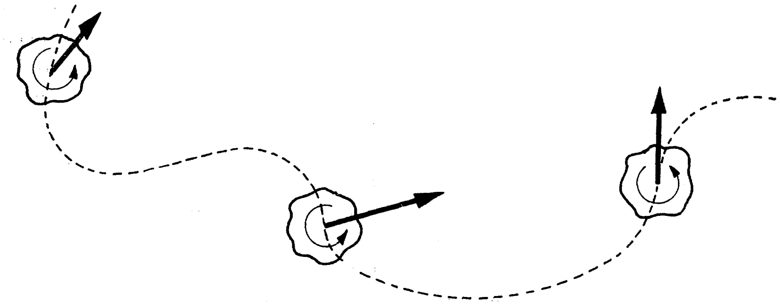

Fig. 1. Spin or "vorticity" of a fluid particle.

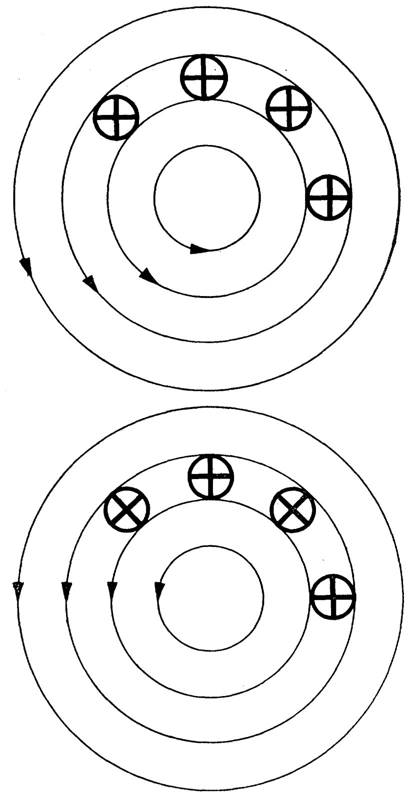

Fig. 2. (Top) vorticity zero; (bottom) vorticity not zero. In both cases, the circular paths of the fluid particles are the same. However, in the case shown in the top diagram, there is no spin, while in the case shown in the bottom diagram, the spin is the same as if the fluid were a rigid wheel.

tions as vigorously as they pursued the mechanical principles the equations embody. That mechanics has its experimental side is obvious, but the creators of the mechanics we learn today in physics courses resolutely and most openly refused to consider mechanics an experimental science. Had our forebears approached mechanics in the style and spirit often recommended nowadays as being "the scientific method," what today we call "classical mechanics" would have lain undiscovered.

Why then is rational mechanics so unfamiliar today? The term is not new. From Newton's generation until our grandfathers' time, it was in fairly common use. The great American physicist Willard Gibbs (*5*) described his book on statistical mechanics as a contribution to rational mechanics. In several countries of Europe, where academic tradition was fixed earlier, each university has its chair of rational mechanics. The neglect of rational mechanics must be laid to fashion. In this country, especially, about fifty years ago the main course of physics turned to the structure of matter; of mathematics, to abstract collections. Since matter is made up of many small particles, it is natural to expect that understanding of the physics of these particles will lead to understanding of the behavior of ordinary materials. Since the collections of ordinary experience are very special cases of the abstract collections encountered in the mathematics of the last fifty years, it is natural to expect that the new mathematics will explain ordinary phenomena. As is now well known, both these expectations are ideal, not real. The physics and the mathematics of the last fifty years have led to very absorbing developments of their own, but they have not brought us appreciably nearer to mastery of the basic problems of mechanics—for example, to the principles governing failure of metals under load or to solution of the equations which describe this phenomenon. It would be wrong to condemn the last half century for failure to solve the problems it did not really attempt. But the basic problems of mechanics remain, and they are being attacked again. It is some of the brilliant successes of the past decade that I wish to describe here. I say the past decade, but the list of references in a modern article on mechanics is likely to begin with some studies fifty or more years old and then skip to the last six or eight years. With relatively little injustice the discussion that follows could be presented as a summary of progress in the 20th century.

I have said that rational mechanics is a branch of mathematics with its own objective and that this objective is confined neither to the existence theorems typical of the theory of differential equations nor to the calculation of numerical answers for comparison with experiment. Rather, the objective is to *understand* mechanics. Understanding, after all, is the objective of every branch of mathematics. The measure of understanding in any field is partly aesthetic, and it is difficult to explain in general terms what a mathematician means when he says he understands or does not understand a given subject.

Fluid Motion

After these preliminaries, it is best to enter into cases. Let us begin with an old research whose value and quality are uncontestable, because from such an example one can most readily see the kind of result that is prized. No one example can illustrate every possible excellence in rational mechanics, but I select a single classic specimen before passing on to the most recent work.

In a fluid motion, each small part of the fluid may be considered as a body in motion. Such a body may or may not be spinning. If it is spinning, it spins about an axis at a definite rate, and thus its spin may be represented by an arrow (Fig. 1). The arrow, which points along the axis of spin and has length equal to the angular speed, is the "vorticity." In one fluid motion different particles may have different vorticities, and the vorticity of a given particle may change as the particle moves. On the surface of a fluid, the vorticity may be observed by following a cork marked with a cross (Fig. 2). If the arms of the cross do not rotate, the vorticity is zero; if they do rotate, there is vorticity. Now the theory of fluid motions in which the vorticity is zero is mathematically much easier than that for the general case, when the particles are spinning. While the case of no spin is appropriate to some applications, particularly for waves on water and for aeronautics, in other cases it is not. For example, without vorticity we could not have the variety of winds actually observed. For a century after the discovery of the fundamental equations for ideal fluids, the nature of spinning motions remained mysterious.

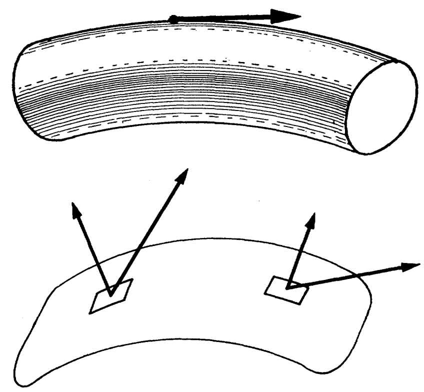

Fig. 4. (Top) vortex tube; (bottom) strength of vortex tube.

In 1858 an entirely new approach was created by Hermann von Helmholtz (*6*). At each point of the spinning motion, draw the arrow representing vorticity (Fig. 3). Then connect these arrows by curves tangent to them. Such curves are called "vortex lines." If we drop a loop in the fluid, the vortex lines through it will sweep out a tube, called a "vortex tube" (Fig. 4). At a given cross section, project the vorticity upon the direction normal to the surface and then add together these projections at each point of the cross section. The resulting quantity is called the "strength" of the vortex tube at that section. After introducing these new ideas, Helmholtz proved three great theorems about them:

1) The strength of each vortex tube is the same at all cross sections.

2) A fluid particle outside a vortex

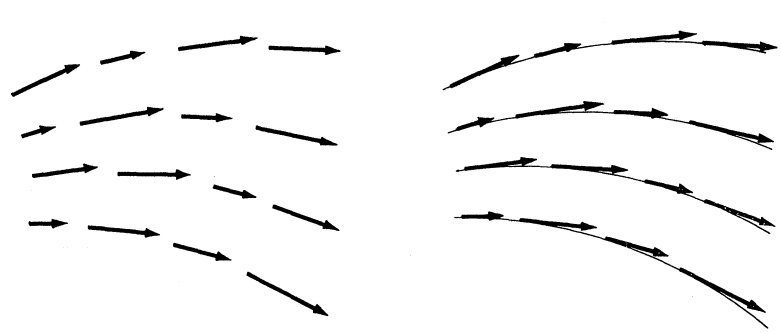

Fig. 3. (Left) vorticity field; (right) vortex lines.

tube never crosses into it, and a fluid particle inside never escapes.

3) The strength of the vortex tube remains constant during its motion. In these theorems, we observe that (i) no new physical laws are proposed; Helmholtz used only the equations of a theory established one hundred years earlier; (ii) the results are not based on experiment but are mathematical theorems; (iii) no equations are solved; (iv) no numerical predictions for comparison with experiment are obtained. Nevertheless, these theorems are universally accepted masterpieces of hydrodynamics. In publishing them, Helmholtz remarked that while complete solutions of the problems to which they are related remained possible only in a few of the simplest cases, his theorems made the entire class of these motions "approachable in concept." I think this is what we mean by saying we "understand" a subject mathematically. Understanding is just what Helmholtz' theorems give us. We picture the entire fluid composed of vortex tubes; as the motion proceeds, these tubes may be bent and twisted every which way, yet they continue to separate the fluid into distinct parts. Moreover, the strength of a tube is a measure of the amount of spin within it, and each tube preserves its strength unaltered as the tube itself is transported and deformed. That is, if the fluid spins faster, the tube must shorten, while if the spin decreases, the tube must stretch. Spin once started can never be lost entirely, nor can spin be created in a particle that was not initially spinning. While Helmholtz' theorems do not solve any special problem, they have proved treasures in subsequent study of spinning motions. They are taught in every beginning course on the mechanics of fluids. While they do not explain the phenomena of turbulence or atmospheric winds, they furnish tools that are used habitually by every student of those fields. In looking back on the theorems of Helmholtz, in addition to some qualities peculiar to them alone, we see also two that are necessary for any great work in rational mechanics: (i) new *concepts,* and (ii) strict mathematical *proof.*

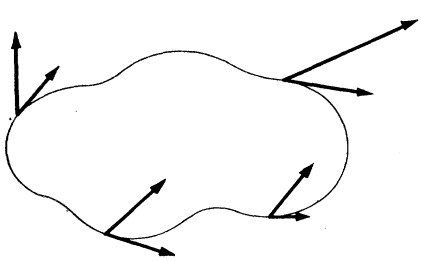

Fig. 5. Circulation of a loop.

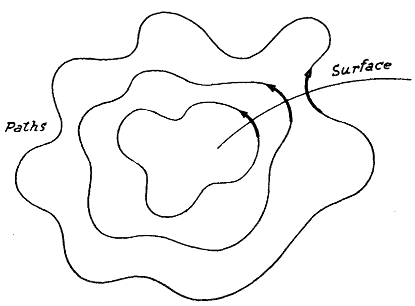

Fig. 6. Ertel's circulating motions.

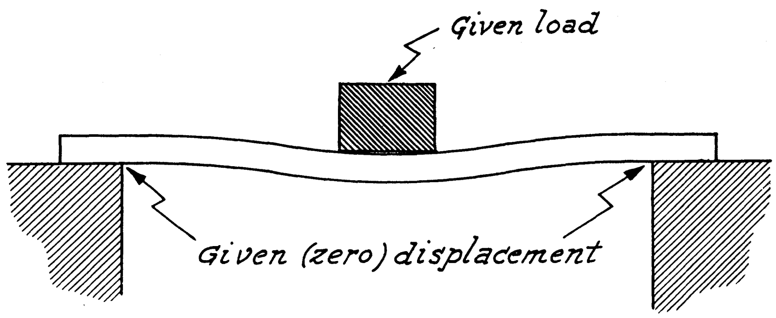

Fig. 7. Boundary of an elastic beam.

Helmholtz' theorems were cast into a new form by Lord Kelvin (*7*) in 1869. Kelvin introduced a still more important new concept. Consider a closed loop of fluid particles, and at each point project the velocity of the fluid onto the tangent to the loop. The sum of all these projections Kelvin called the "circulation" of the loop (Fig. 5). The circulation is a measure of the average rate of turning of the loop. Kelvin proved then that Helmholtz' theorems are equivalent to a single statement: The circulation of a loop of fluid particles remains the same throughout its motion. That is, however the fluid loop is turned, pulled, and twisted as the spinning motion proceeds, its circulation is unaltered. Kelvin's theorem plainly exhibits qualities i and ii above.

Helmholtz' and Kelvin's theorems refer to a problem that was one hundred years old when they wrote; these theorems are now themselves nearly one hundred years old. A beautiful addition to precisely this same subject—spinning motions of ideal fluids—was made in 1950 by the German meteorologist Hans Ertel (*8*). Consider a steady spinning motion whose paths are closed circuits perpendicular to a certain surface (Fig. 6). The character of this motion is assumed not to change in time. That being the case, any one particle on one of the circuits takes as long to go around it as does any other particle on the same circuit. Thus, each circuit has a definite time of travel, or *period.* For two hundred years it has been known that each path has a definite *energy;* Kelvin's theorem asserts that each path has a definite, constant *circulation.* Ertel had the insight to perceive that circulation, energy, and period must be related, and for two neighboring circuits he proved that

$$\text{Period} = \frac{\text{Difference of circulations}}{\text{Difference of energies}}$$

While the theorems of Helmholtz and Kelvin provide a geometrical picture, Ertel's theorem gives us the first insight into the time a fluid requires to execute a spinning motion.

Small Elastic Deformations

Next to fluid mechanics, the oldest branch of the mechanics of materials is elasticity, the science of the deformation of bodies under load. The basic equations for slight changes of shape have been established for 125 years. For about seventy-five years we have known two alternative ways of stating these fundamental laws in terms of economy: (i) Among all changes of shape consistent with given displacement on the boundary, that which occurs according to linear elasticity yields the least stored energy. (ii) Among all statically possible interior forces consistent with given forces applied on the boundary, those which occur according to linear elasticity yield the least complementary energy.

In the second theorem, "complementary energy" means stored energy less the work done by the surface loads in producing the surface displacements. Both these statements are what are called "variational principles." In comparing different conceivable states of a deformed body, they assert that the state actually occurring is such as to give to a certain quantity the least possible value. Variational principles are prized for four reasons: (i) they characterize an entire theory in terms of a single simple concept; (ii) they are equally valid for all methods of description; (iii) they may often be used to prove analytical theorems regarding the subject; (iv) they may often be used to calculate numerical solutions in special cases.

For the linear elasticity theory, the two classical variational formulations are different. One refers to displacements, the other to interior forces. If, as often happens (Fig. 7), on a part of the boundary the displacement is given, while on another part the loads are given, neither principle can be applied. For each principle, one half of the equations of the theory are regarded as known, while the other half are consequences of the principle; in the two principles, the two halves of the basic equations play interchanged roles. This is unfortunate, since a variational principle should express the situation in its entirety.

In 1950 Eric Reissner of Cambridge, Massachusetts, established a new variational principle in which nothing is presumed. On part of the boundary the loads may be given; on other parts, the displacement; on still other parts, a portion of the load and a portion of the displacement. In the comparison of different possible states, all six measures of internal force and all six measures of deformation are varied. In 1914 Ernst Hellinger (*9*) had defined what might be called a new type of stored energy, depending on all 12 of these quantities, and he had proved a variational theorem for it. Reissner rediscovered Hellinger's result and completed it by considering every possible type of boundary condition. He proved that giving this new energy the smallest value consistent with the given loads and displacements on the boundary is equivalent to satisfying the entire set of equations of elasticity theory. Thus, a fully general variational expression, including as special cases both the old ones, has been proved.

Just over a century ago Barré de St. Venant (*10*) created an ingenious method for finding the interior forces and deformations when a bar is twisted. In this method, the total torque twisting the bar may be assigned, but the detailed distribution of load on the ends of the bar is determined by the method and cannot be assigned at will. Thus, the method does not yield a fully general solution of the elastic problem. St. Venant reasoned that this should make no difference. When a bar is twisted in an experiment, it is usually impossible to ascertain the distribution of load. All that is known, usually, is the over-all torque (Fig. 8). Near the end itself, different loadings may produce considerably different effects, but far down the bar no difference is perceptible, provided that the total torque is the same. Thirty years later Jean Boussinesq (*11*) expressed St. Venant's suggestion essentially as follows: The difference between the effects produced by two different but equivalent loads applied in a given part of the body becomes very small at great distances from that part. Two loads are "equivalent" if they would have exactly the same effect on the body if it were rigid. The notion just expressed has been generally accepted and is called "St. Venant's principle."

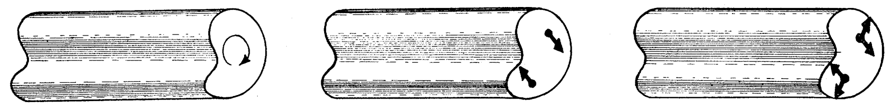

Fig. 8. Equivalent end loads twisting a bar.

In the attitude toward St. Venant's principle we may illustrate a difference between engineering, or applied mechanics, and rational mechanics. St. Venant's principle can be subjected to partial test by experiment. It has been tested and found good. Therefore it is used with confidence by engineers. Every time they design a bridge or ship they apply St. Venant's principle repeatedly, if often unconsciously. In rational mechanics, however, elasticity is a mathematical theory whose basic equations are fixed once and for all. If St. Venant's principle is true, it should be *proved* as a mathematical theorem. Indeed, there have been many unsuccessful attempts to prove it.

If proof of St. Venant's principle were a problem in pure analysis, I should not mention it here. Like other real problems of rational mechanics, it required a preliminary searching of concepts. My statement of it above is vague, and several different statements have been proposed. All these were reviewed in 1945 by the late Richard von Mises (*12*) of Cambridge, Massachusetts, and he showed all to be either trivial or false. Von Mises proposed to consider the case when loads are applied within a small sphere of radius r, which is then allowed to shrink (Fig. 9). At a fixed point within the body, we should try to show that, as r approaches zero, the effects of two equivalent loads in the sphere are of more nearly the same order of magnitude than are the effects of two nonequivalent loads. By examples of a special kind, von Mises showed that this formulation could not be correct, and he proposed a stronger one.

In 1954 Eli Sternberg (*13*) of Chicago put von Mises' views into general mathematical form and thus at last produced a definite enunciation, capable of proof or disproof. Notice that what was involved in the first 99 years of this problem's history was search of the *concepts* preliminary to use of the analytical tools usually associated with mathematics. Sternberg proceeded to construct a strict proof, and he proved St. Venant's principle *false*. However, he was able to show that additional restrictions, similar to those suggested by von Mises and realized in many cases of application, render it true. For example, if the loads are parallel to a fixed direction, and if the two loadings remain equivalent when rotated, St. Venant's principle is true.

Large Elastic Deformations

While the foregoing researches fall within the theory of small elastic deformations, that theory itself is insufficient to describe the behavior of materials such as rubber, which may be stretched severely yet springs back to its former shape. Some seventy or eighty years ago, a mathematically proper theory for large elastic deformations was formulated. However, although this theory is an old one, its status is different from that of the only-a-little-older theory of small deformation. University courses in

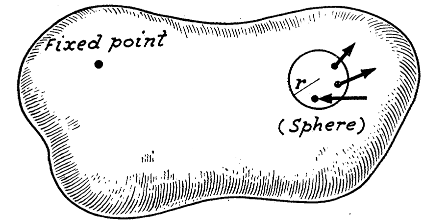

Fig. 9. Von Mises' formulation of St. Venant's principle.

the theory of small deformations are standard; thousands of papers concerning it have been published; for a century there have been textbooks and even reliable treatises upon it. The theory of large deformations, on the other hand, has been known by only a few; few have been the papers concerning it, and from 1900 to 1948 general knowledge of it declined to the point where many specialists in mechanics were partially unaware of its existence. It was not until 1948 that the first book concerning it was published, in Russian.

An exception to the foregoing statements is provided by the work of Antonio Signorini (*14*) in Rome. Of all the persons I shall mention in this article, Signorini is the only one whose position is that of "professor of rational mechanics." From the 1930's until the present time, Signorini and his pupils have sustained classical knowledge and have made important additions. First I mention a beautiful and simple theory of mean values, noticed by Signorini in 1933 and later extended. For any kind of material, whether elastic or not, this theory enables us to estimate the *average* internal forces if we know the loads applied. Hence result very simple bounds for the greatest internal force.

In elasticity itself, the approach of Signorini has been to conjecture the particular manner in which the stored energy depends upon the deformation. Adopting one or another form as a hypothesis, Signorini explores the properties of such a material and checks them against experimental behavior of real materials. Signorini has obtained some success with certain particular theories.

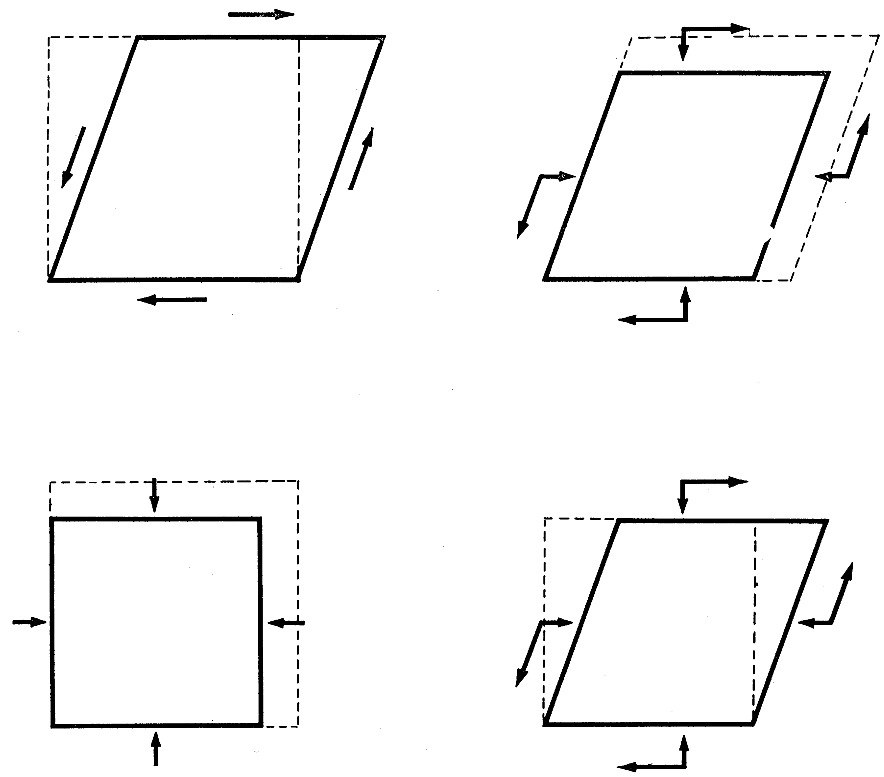

Fig. 11. Compression and shear in a linear theory.

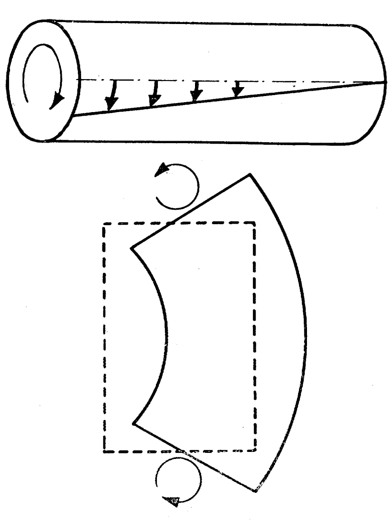

Fig. 10. (Top) twisting a circular rod; (bottom) bending a rectangular block.

While superficially this attack may resemble that of the many semiempirical engineering studies on large deformations, in fact Signorini's work is different in character and very precise. In my opinion, later researches show this approach to be inadequate, but there are by-products for which no counterpart is yet known in the more general work to be described presently. These by-products are restrictions on the manner in which the various measures of elasticity of a body may depend upon its temperature. Naturally such restrictions arise in part from the laws of thermodynamics, which are included in the general system of mechanics. In particular, restrictions upon the internal energy are shown to be consequences of the form of the stored elastic energy.

A basically new attack upon large elastic deformations was initiated in 1948 by Ronald Rivlin (*15*), then in London. For bodies whose volume cannot change, no matter how they are deformed, he sought exact solutions for simple but important problems. His fundamental departure was to leave the form of the stored energy function *entirely unrestricted*. The equations of the theory are so complicated that an approach of such generality would seem hopeless. In fact, only the idea had been wanting; the calculations turned out to be easy. Rivlin found the fully general solutions for twisting a circular rod (Fig. 10, top) for bending a rectangular block (Fig. 10, bottom) and for several other cases. These solutions were compared with experimental measurements on rubber, and in this way the form of the stored energy function for rubber was determined. The measurements showed that none of the forms which had been guessed at various times by various persons were physically correct. In particular, forms suggested by the common argument that the effects of certain "small" quantities may be neglected were shown to be inadequate. By substituting the experimental form in other particular solutions, Rivlin was able to predict the behavior of rubber in cases when it was stretched to twice and even three times its initial length, with an accuracy of a few percent. This is a remarkable achievement. Here I must add that "small" in the old theory of small deformations means "almost invisibly small": a change in length of 1 percent is usually too great for adequate description by the theory in books on elasticity. Second, it had been known for a century that rubber does not follow the predictions of the theory of small elastic deformations, and many researches of a physical or chemical nature had been devoted to attempts to get a proper theory for rubber from hypotheses regarding its molecular structure. The new researches in elasticity show that the mechanical behavior of rubber is fully and precisely predicted by purely mechanical principles, *providing they are not degraded by so-called "approximations."* In respect to application to physical phenomena, this was a triumph of the principles of mechanics and of generality. Out of Rivlin's work has grown a new branch of engineering which might be called "applied mechanics for rubber."

Other Nonlinear Theories

In 1945 a new pathway was opened by Marcus Reiner (*16*), in Haifa. Originally he dealt with fluids rather than solids, but it was soon realized that a new general method in the mechanics of nonlinear materials was available. Before going further I must explain the terms *linear* and *nonlinear*. In a linear theory, two causes applied at once produce the same effects as if first one, then the other, were applied singly. For example, if we first shear, then compress, a block, in a linear theory the result is the same as if we reverse the order or compress and shear simultaneously (Fig. 11). In a nonlinear theory, this need not be so. In fact, in a nonlinear theory it is generally necessary to apply compressive force in order for a shear without compression to be possible (Fig. 12). The compressive force appears to do nothing whatever, but if it is not applied, the body will expand. A more familiar effect of nonlinearity is seen in the gyroscope, which usually swings over in a direction quite different from that in which it is pushed. Mechanics as a whole is nonlinear; the special parts of mechanics

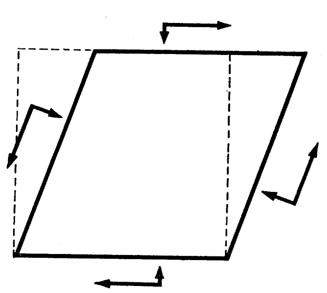

Fig. 12. Compression and shear in a nonlinear theory. Compressive forces are required to effect a shear without change of volume.

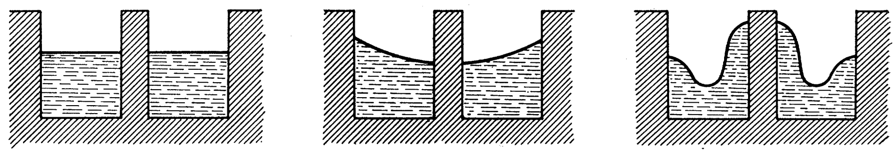

Fig. 13. (Left) rest; (center) rotation, linear fluid; (right) rotation, nonlinear fluid.

which are linear may seem nearer to common sense, but all this indicates is that good sense in mechanics is uncommon. We should not be resentful if materials show character instead of docile obedience.

Although mechanics is essentially nonlinear, it is little exaggeration to say that for 150 years only linear mechanics and its mathematics were studied. It became standard practice, after deriving the equations for a phenomenon, to replace them at once by a linear so-called "approximation." It would be wrong to regard this mangling as being in the original tradition of mechanics. In fact, only after a century of development was it generally recognized that linearity is synonymous with easiness. The linearizing set in about 1780 but did not gain undisputed mastery until toward the end of the last century, under the influence of Lord Rayleigh. Indeed, there are many physical situations in which a linear theory is adequate. This does not alter the fact that in many other situations linearization is unjustified; and both the mathematics and the mechanical principles associated with nonlinear phenomena are much more interesting.

While a century of linearization may have largely exhausted its capacities for explaining physical phenomena and have allowed its mathematics to be explored almost to the full, it has fostered a certain rigidity of approach which might be called "linear thinking." This has made a return to nonlinear problems more difficult in fact than perhaps a few years hence it may appear to have been. In particular, it has engendered an emotion somewhere between apathy and fear in the reception that nonlinear studies are accorded by the great majority of researchers, who today remain absorbed in conventional linear problems.

The work of Reiner in 1945 and similar work by William Prager (*17*) in 1945 and by Rivlin (*18*) in 1948 put into our hands a new tool for exploration of nonlinear phenomena in materials. Basically, their major result is a revival of an old but little-known theorem in algebra, having broad usefulness in mechanics. It is applicable to any material, whether fluid or solid, so long as the internal forces arise solely in response to a single measure of deformation. The example above concerning simple shear is an entirely typical consequence of this new approach. Another application, in essence the same, is this: When an elastic rod is twisted a little, it will elongate proportionally to the square of the angle of twist. Another application of the same principle is this: When a fluid is rotated about a fixed cylinder, it will climb up the cylinder, the force required to prevent such a rise being proportional to the square of the rate of rotation (Fig. 13). The former effect had long been known experimentally. While the latter had been observed specifically only at about the same time, it was quickly recognized as familiar. In the paint industry, for example, rotary stirrers were found to have little effect because the paint agglomerated upon them. Various physical and chemical explanations were given for this effect, but the theory of nonlinear fluids accounts for it easily and naturally on purely mechanical grounds. Rivlin calculated exact solutions for the configurations occurring in viscometers, and when viscometers which could measure the new effects had been constructed, experimental agreement was found.

It would be misleading to emphasize unduly the success of the nonlinear theory in predicting these phenomena. To form a particular theory such as to agree with any particular measurement is not nearly so difficult as it might seem. Indeed, *special* theories of nonlinear elasticity and nonlinear fluids had been constructed in some number during the 1930's and 1940's, and it is only the rule that such researches always include experimental data fully confirming the theory. What is new in the work just described is its *independence* on experiment. The new approach gives us a unified view, a grasp upon a whole class of nonlinear theories. The major result established, in respect to experimental application, is that virtually *any* reasonable nonlinear theory will predict these effects, so that their experimental occurrence, while indeed showing nonlinearity in real materials, does not confirm any of the numerous special theories.

You will notice that my tone has changed. While the advances I first discussed concerned specific problems in established theories, now I am surveying the discovery of *new theories* of mechanical behavior. This—the most absorbing aspect of modern mechanics—dominates the field today.

While I have praised generality, we must have a care of its dangers. It is easy to think of a material which, instead of using up work when it is deformed, gives it out. Such a material, indeed, we can think of, but, at least so far, it is not interesting in mechanics. This and more subtle misbehaviors we wish to exclude in the theories we create. In 1948 I showed (*19*) that, without surrendering the inclusiveness of the work of Reiner and Rivlin, we can classify nonlinear materials by dimensional analysis. The results enable us to discard at one stroke many materials that might otherwise seem reasonable. In regard to elasticity, in 1952 I proposed a more specific requirement (*20*): When a body has been deformed, additional deformation requires additional stored energy. This is not so trivial as it sounds, since what is meant by "additional deformation" is not obvious. This principle turned out to imply restrictions on the stored energy function. In 1942, M. Baker and Jerald Ericksen (*21*) of Washington replaced this idea by a more general one applicable to fluids as well as to solids. In 1955 I pointed out (*22*) that requirements of this type are closely connected with wave propagation: A body so unreasonable as to refuse passage to waves may also do other and more obviously objectionable things. At the same time, Ericksen and Richard Toupin (*23*) of Washington established a connection between these requirements and those of uniqueness. According to one of their results, a deformed body properly receptive toward waves is also unwilling to deform further in more than one way in response to given small additional displacements upon its boundaries. This class of problems, whose objective is to distinguish liberty from license, is still under study.

Here I pause to answer a heartfelt question of many a reader: Why don't we measure all these things? First, measurement in mechanics is not so easy as it might seem. Despite objections from philosophers of science, mechanics has always been expressed largely in terms of quantities that are themselves usually not measurable. Had there been operationalists alive in 1750, the design of a bridge in 1958 would proceed more awkwardly than it does in any engineering office today. Second, we have had more than a century of experimentation, often on a large scale and at great cost, regarding the mechanical behavior of materials. Had experiment settled these matters, I should not be writing about them here. Experimental mechanics is a recognized field which needs no defense; it has its own problems, its own methods, its own results, its successes, and its failures. It may, but need not, cooperate with applied mechanics and with rational mechanics; these, in their turn, may, but need not, cooperate with it. Without *experience* there would be no rational mechanics; but I should mislead you if I claimed that experiment, either now or two hundred years ago, has greatly influenced those who study rational mechanics. In this connection experiment, like alcohol, is a stimulant to be taken with caution: to consult the oracle of a fine vintage at decent intervals exhilarates, but excess of the common stock brings stupor.

Students of rational mechanics spend much effort thinking *how materials might possibly behave*. These thoughts have not been unproductive of information on how some materials do behave. Real materials are not naive; neither are they irrational.

In their behavior, materials always act in just the same way, no matter who happens to be looking. The response of a material is independent of the observer. Now this statement becomes less trivial than it might seem when we make two further observations: first, the laws of classical mechanics themselves change most noticeably when the same body is regarded by different observers; second, to express the response of a body in mathematical language we always employ the reference frame of *some* observer. The problem, then, is to find how to limit our ideal materials to those whose response is properly invariant with respect to change of observer.

For the old, linear theories, this problem is trivial and is solved automatically by using well-known principles of tensor analysis. For the nonlinear theories I have just discussed, the problem is relatively easy. But when the interior forces depend on more than one measure of deformation, or when the basic laws connect time rates, the problem becomes relatively difficult. For a very special case of time rates, the problem was set and solved by Augustin Cauchy (*24*) a century ago; in rather more general but still limited circumstances, by Stanislas Zaremba (*25*) in 1903. The nature of the general problem and a correct solution of it was first indicated in 1950 by J. G. Oldroyd (*26*), then working in Maidenhead. A more satisfying treatment was given in 1955 by Walter Noll (*27*), then in Bloomington. Noll's formulation is in terms of a principle he calls "the isotropy of space," by which the basic equations of any proposed ideal material can be tested for invariance. Similar methods, somewhat different in detail and scope, were introduced in two papers written by Rivlin (*28*) jointly with Ericksen and with Barbara Cotter of Providence; the method of Zaremba was rediscovered and extended by Tracy Y. Thomas (*29*) of Bloomington.

By use of his principle, Noll was able to construct the first adequate theory of the *continuity of the solid and fluid states* (*27*). In this unified theory appear materials which show the responses both of solids and of fluids, the nonlinear theories of pure elasticity and pure fluidity being included as extreme possibilities. The idea of such materials had been put forward a century earlier by James Clerk Maxwell (*30*) and had been explored with partial success by Zaremba (*25*), but Noll's work is the first to combine exactness and completeness. Noll obtained exact solutions for certain special deformations and showed that his theory implies breakdown of smooth motion at high speeds. A possible theory of continuous transition from ordinary behavior to ultimate failure or breakage has thus been initiated.

In 1953 I proposed (*31*) a more modest but at the same time more definite theory of elastic behavior. My idea was, instead of relating the interior forces to the deformation, to connect the rate of change of the interior forces with the rate of change of shape. The resulting theory, called "hypoelasticity," agrees with the old linear theory for small changes of shape but for large changes is entirely different. This new theory has turned out to be fairly general; in 1955 Noll (*27*) proved that it includes the classical theory of large elastic strain, and in 1955–56 Albert Green (*32*) of Newcastle and T. Y. Thomas (*33*) proved that, apart from an assumed condition of yield, it contains all the usual theories of plastic flow, provided those theories are corrected in several respects. The reservation "apart from an assumed condition of yield" is important. For decades, theories of plasticity had employed a specially assumed condition to express plastic flow—a semiempirical

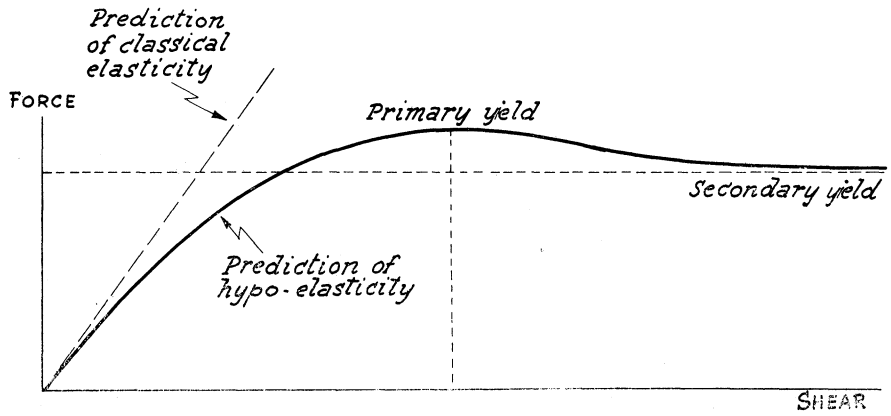

Fig. 14. Primary and secondary yield predicted by hypoelasticity.

compromise between experiment and theory unrelated to other parts of mechanics. I had long felt that this was unfortunate; that in fact yield is a phenomenon which should be *predicted*, not assumed, by a proper theory; that a true mechanical theory should include plastic flow not as a primary condition but as the *result* of previous circumstances leading up to elastic failure. In 1955 I showed (*34*) that, in certain cases, hypoelasticity furnishes a smooth and simple theory of just the type desired. For certain hypoelastic bodies in shear, the force required to effect the shear has been proved to follow a curve such as that shown in Fig. 14.

Statistical Mechanics

To conclude the list of advances, I wish to return from exploration of new theories and consider progress in an old, established theory: statistical mechanics. In statistical mechanics, apparently continuous matter is represented as a very numerous assembly of little points called "molecules," which are in rapid and largely independent motion. The object is to relate quantities of experience, such as force and velocity, to *averages* over the molecular assembly. Averages of this kind are called "phase averages." The conditions defining a particular motion of the molecules are not known. Rather, a whole class of such motions is laid down as being possible, and each motion within this class is assigned a *probability* of occurrence.

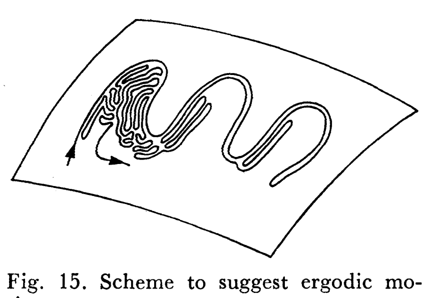

Fig. 15. Scheme to suggest ergodic motion.

In introducing such averages about eighty years ago, Maxwell (*35*) remarked on the difficulty of convincing oneself that such purely mathematical quantities are appropriate for comparison with physical measurements. In the particular kind of phase average he recommended, every motion admitted has the same energy, and all such motions are equally probable. He suggested that a complicated mechanical system, if left to itself, would eventually assume nearly every configuration consistent with its assigned energy. If we think of the motion as a curve, this curve would be so tortuous as to occupy, at one time or another, nearly every point on the surface consisting of its possible configurations (Fig. 15). For such a motion, the average behavior of a given system over a long time would equal the average over all possible motions at any one instant:

Time average = phase average

Motion of the type Maxwell suggested was quickly shown to be impossible, yet the possibility remained that his conclusion could be true.

The "ergodic problem," as the problem of proving this conjecture came to be called, drew the attention of many mathematicians, and in the 1930's it was solved. The conclusion, as far as mechanics is concerned, was negative. In order that the time average be exactly equal to the phase average, for *every quantity* and for *all but utterly improbable ways* of setting the system in motion, a condition emerged which was inappropriate to the mechanical problem and not reasonably to be expected. While an extensive and interesting part of pure mathematics grew from this idea, no progress in mechanics resulted.

In any case, Maxwell's type of average (which came to be called "microcanonical"), for reasons of mathematical difficulty usually could not be evaluated. Even before Maxwell's work, Ludwig Boltzmann (*36*) had introduced another kind of phase average, later called "canonical." No mechanical principle or idea lies behind this kind of average. Rather, its reason for existence is *mathematical* simplicity: With canonical averages, a patient person can get answers, and virtually all explicit statistical mechanics as applied to chemistry and physics has rested upon canonical averages from that day to this. Nowadays many scientists are willing to compare canonical averages with measurements and, finding them valid, to accept the theory based upon them as being justified directly by experiment. This was not Boltzmann's idea at all. Boltzmann attempted to prove that, as the number of molecules in the system becomes very large,

Microcanonical average $\rightarrow$ canonical average

In several attempts, Boltzmann claimed to give analytical proofs (*37*) of this conclusion, which is called "Boltzmann's law." One of these proofs is often reproduced in courses today, but, like the others, it is unsound. Such was the insecurity of the conclusion that, in his book on statistical mechanics (*5*), Gibbs did not even mention it. In the 1920's, Darwin and Fowler achieved a satisfactory but rather difficult proof.

This was the situation when the Russian mathematician A. Y. Khinchin (*38*) took up the subject in 1943. First, he gave a new, somewhat simpler, proof of Boltzmann's law along lines nearer to the ideas of the classical theory of probability. If this were all, I should not be mentioning it here, but this was an essential preliminary to his solving a much deeper problem—the true ergodic problem of statistical mechanics.

In looking back on the ergodic problem, Khinchin realized that the mathematicians of the 1930's had asked too much. First, they had required that the time average be exactly equal to the phase average, while for mechanics it would be enough that the error be small. Second, they had required that equality hold with only utterly improbable exceptions, while it would be enough that the exceptional cases be rather improbable. Third, they had required that the equality hold for every sort of quantity that might be averaged, while in fact only a limited class of quantities are appropriate for averaging in a mechanical system. The third idea gave the real clue. A quantity that has nearly the same value for all possible configurations will certainly have a nearly constant average in time for a given system. It is almost trivial to remark that, for such a quantity, the phase average and the time average are almost the same. But what Khinchin was apparently the first to realize is that Boltzmann's law itself implies that the quantities which are to be averaged are indeed nearly constant over most possible configurations! He then went back over his proof of Boltzmann's law and sharpened it sufficiently to estimate the average error. With this estimate, he was then able to establish the ergodic principle along the lines just indicated. In the sequence

Time average →
microcanonical average →
canonical average

proof of the second stage was shown very elegantly to yield proof of the first stage as a by-product. Khinchin's result, in full, may be put as follows: The cases when the time average differs very much from the canonical average become, as the number of molecules in the system is taken larger and larger, of arbitrarily small probability. In this statement we perceive another difference from ergodic theory, which is just as applicable to two marbles in a box as it is to a solid body with trillions of molecules; Khinchin's result, like Boltzmann's law, is a theorem appropriate only to complicated and numerous systems.

Khinchin's brilliant success has been little appreciated. On the one hand, most physicists have lost interest in the problem. On the other hand, mathematicians trained in ergodic theory find the case too special to be interesting. In my opinion, Khinchin's analysis is a masterpiece in rational mechanics, fully justifying the original view of Boltzmann. It shows that every time a chemist calculates a canonical free energy, he is calculating in fact the average free energy in a system of fixed total energy left to itself for a long time.

While the foregoing analysis refers to systems in equilibrium, the statistical mechanics of deforming media furnishes even more interesting problems. Until recently the statistical mechanics of fluids, for example, was regarded as different and presumably more accurate than theories representing fluids as continuous. That this is not the case was shown in 1950 by Jack Irving and John Kirkwood (*39*) of Pasadena. They contributed the basic idea, but their proof was not entirely satisfactory and their approach was not as general as it might have been, and in 1955 a better treatment was given by Noll (*40*), then in Berlin. His form of the result will now be summarized. Given any assembly of molecules, be they alike or different from one another, few or numerous, free or subject to any outside forces or forces of interaction, consider a selection of probability according to any admissible rule. Then, by appropriate phase averages, it is possible to define mean velocity, mean internal force, mean internal energy, and mean flow of energy in such a way that *they satisfy exactly the field equations for continuous materials*. Thus, statistical mechanics and field mechanics are united. By considering a fluid or solid as an assembly of molecules, it is impossible to derive anything that is in contradiction to the view of matter as continuous. Conversely, there is nothing in the continuous view of matter that is not also in accord with a molecular picture. With this beautiful unification of the two apparently opposing views of matter, I close the list of examples.

Apology

What I have presented shows the power and versatility of modern rational mechanics. You may expect me to say now that these achievements are only a small part of the great things that have been done, but that would be misleading. Indeed, in a longer article I would describe the work of Eberhard Hopf on turbulence, of T. Y. Thomas on the paths of dynamical systems and on the stability of shock waves, of Otto Ringleb, Walter Tollmien, and Kurt Friedrichs on transsonic gas flows, of Paul Neményi and Robert Prim on rotational gas flows, of Bruce Hicks, Prim, and Ericksen on the streamlines of gas flows, of Lavrentiev, David Gilbarg, and James Serrin on water flows past cavities, of Einstein, S. N. Bose, and Vaclav Hlavatý on the new unified field theories, and of Toupin on elastic dielectrics; I would describe some further studies of vorticity, and some other things. However, the list would not be very long.

Indeed, I must conclude with an apology for rational mechanics. It is not likely to become a popular field. No prizes are awarded for rational mechanics, and it would make a poor showing in a poll of the public or of scientists in general. The monetary cost of a century of rational mechanics will not equal the hundredth part of what is spent this year on computing machines. The work I have described was done slowly, by individuals working alone or with a single other individual of like tastes. The great teams that produce bombs and vaccines would not have multiplied or deepened the output here. In an age and country where numbers, cost, and statistics count, rational mechanics will never gain much notice. In itself, notice is not what we need, but nowadays he who is not noticed is not likely to survive. In rational mechanics the financial need is on so small a scale that often it goes entirely unrecognized, in favor of more costly and more glittering projects. Whether in universities or outside them, several of the persons whom I have named lack a proper library, secretarial help, and even adequate stationery, not to mention a reasonable allowance of time for work free of teaching or other community responsibility. There is no society for rational mechanics, nor are its individualist students likely ever to be numerous enough to afford one or cooperative enough to establish one. For at least twenty-five years no one has been elected to membership in the National Academy of Sciences for achievement in rational mechanics.

As far as costs and numbers go there has been little change. It would be quite incorrect to assume either that the "classical" mechanics we learn today as the first step in physics and engineering was produced by a cooperative effort of organized science or that, in those old-fashioned days, no large projects existed. Indeed, two hundred years ago much money was spent on science: on the calculation of great numerical tables, on extensive experiments for the betterment of mankind, on the design of warships, on the establishment of boards and committees to organize science. But these efforts did not produce the "classical" mechanics; this was the work of a handful of men, scattered over a continent and a century—men who were willful, uncompromising, quarrelsome, arrogant, and creative.

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Economics of Nuclear Power

An analysis of when the falling costs of nuclear power will meet the rising costs of conventional power.

John E. Ullmann

It is generally believed that the costs of nuclear power will decrease, and hence it is necessary to attempt a prediction of the time when nuclear power will begin to compete economically with power from conventional steam plants. This is of interest not only to utilities engaged in long-term planning of facilities but also to power users and to equipment manufacturers, who have to assess the competitive pressures exerted by atomic power developments upon their established lines of products. The specific variables of interest are capacity costs and bus-bar costs. Capacity costs are given in dollars per kilowatt of installed generating capacity. Bus-bar costs are the costs of power at the generating station—that is, the costs exclusive of transmission and distribution costs; they are usually given in mills (0.1 cent) per kilowatt hour.

There have been many predictions about the costs of nuclear power. The reasoning behind the growth rate they propose is not, however, generally set forth. The predictions of costs, and hence of break-even points—that is, the time when nuclear and conventional power will cost the same—usually assume that the present conventional power plant capacity and bus-bar costs will remain stable within rather narrow limits. It follows from this view that the price reductions of the future will have to

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