

EXPERIENCE, THEORY, AND EXPERIMENT

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Science today is much like government or big business: scientists are specialists not only in a single science but even in a single problem, each a senior bureaucrat jealous of interference from the multitude of others whose actions are in turn as isolated as his own, and the multiplied sciences themselves are self-fecundating compartments which reproduce, if at all, by division. Everyone is an expert in something. We are accustomed to speaking of every scientist as a leader in a particular field, but often it is difficult to discern any following. That so many experts turn out so much research that no single person can know it all even in a single field is often brought forward as proving the progress of science. Rate of working, however, is the product of force by velocity and is not necessarily increased if velocity approaches infinity while force approaches zero. There are costly efforts to gather and review the totality of the literature in various fields, but it might be more appropriate to find out who, if anyone, reads the typical paper of today.

Fluid mechanics in its various aspects is divided among several types of engineers, a few physicists, and some mathematicians. Since first I began to study fluids I have had to lose time listening to wrangles among members of these cults, each defending his own while condemning the others. The engineer has the right and duty of knowing fluids as they are met in life for man's direct harm or use; to him, the physicist sets up situations whose only relevance is their ease of study for physicists, while the mathematician is lost in abstraction and arid brain games. The physicist drills out the true principles of fluids, above both the mere detail and empiricism of the engineer and the purism of the mathematician, who for rigor is ever ready to gloss over essential physical aspects. The mathematician has the assurance of correctness and finality; for him the results of physicist and engineer are alike suspect, mere conjecture and ever subject to possible revision. These views are not without their truth in the cellular science of our day. Permit me to reset them in words less apt to reassure the engineers,

physicists, and mathematicians in their respective complacencies. The engineer, blinded by the daily need for design or test of this or that device, will not pause to learn enough of the concepts of modern physics or the methods of modern mathematics to find out whether they can be applied to his problems. The physicist, blinded by the oversimplification and the raw guessing now in vogue, despises alike the phenomena which occur in natural, day-to-day situations and the logical standards of precise reasoning. The mathematician, blinded by a century of ever more abstract pure mathematics, has lost the skill and wish to read nature's book. Whether put as praise or put as blame, the foregoing argument, which each of us must endure at every meeting, is barren. I should like to lay before the community of those who study fluids a motion that we hear no more of it. For my part, I promise to try to speak to you not of one of the professions within fluid mechanics today, but of fluid mechanics itself.

Knowledge of fluids is gained through experience, theory, and experiment. Of these, the first and last are often confused. Experience is sometimes dismissed as the uncomprehending rules of thumb of mere artisans, while experiment is exalted as the foundation of science. The empiricism of some physicists of the last century has been embraced by many philosophers of science and educators of our day, particularly those associated with psychology and the biological and social sciences. Students in some of these doctrines are often given long instruction in the "scientific method," which is said to consist in controlled experiments and their statistical evaluation, while theory, if mentioned at all, is subsequent curve-fitting. Experience, the straight impress of nature that observant and rational man gains through his unaided senses as he daily encounters the world and which, by his nowadays all too slighted faculty of reason, he puts in verbal generality, is dismissed as primitive and below science. Many prefer to mine nature's darkest and deepest entrails, closed except to the dearest experimental apparatus or voluminous statistics, while leaving the smiling face of earth unheeded. That modern science is experimental science would follow also from any poll of the scientists themselves: in the biological sciences, little else than experiment exists, while in the physical sciences experiments are often so elaborate that large numbers of persons must be employed upon them. Such force of numbers need not be compelling. A poll of professional musicians would reveal that music today is neither hot jazz nor symphony but sugar stirred in soup. While scientists are more apt than musicians to idolize the means by which they must earn their daily leisure, nevertheless

many experimentalists will admit by their actions if not by frank confession that theory is the objective of science. The frustrating and so far vain struggles of biologists and social scientists to organize their subjects upon a basis of mathematical theory is apparent in every conversation among them, while it is the successful theories of the physical sciences that distinguish them from other human endeavors and sometimes cause their enthusiasts to put forward thinly disguised claims to the sole possession of knowledge.

The hydraulic engineer is favored in being alike in daily encounter with experience, theory, and experiment. His tasks and problems arise in common experience, which he dares not desert for more voluptuous realms deep hidden from human eye and touch. The flow of water he describes, understands, and controls in terms of the concepts of theory: velocity, pressure, and density, themselves mathematical ideas expressed in symbols and employed in equations. Either to check and correct the results of theory or to find answers to specific and detailed questions, he has recourse to experimental measurement. For this audience, therefore, I can refrain from further generalities on morals and philosophy. Instead I wish to present you two stories from the history of fluid mechanics. These concern the development of two fundamental concepts. The first concept is one you all use every day, the *static pressure* in a fluid in motion. Its origin is a part of the story of Bernoulli's Theorem, now more than 200 years old. The second concept, *cross-viscosity*, is one with which few of you are yet likely to be familiar; its story begins little more than ten years ago. I will tell you these stories, not in the fashion of those text-book writers who manufacture historical notices so as to bear out their own views of how science ought to have developed, but instead as they really did occur. Since one of these stories is from the earliest period of modern hydraulics and the other is still continuing today, and since despite the lapse of two hundred years between them the general outline is much the same, there will be no need for me to add comments or to draw a moral.

For the early development of hydraulics, I refer you to the excellent history by Rouse and Ince, the first installments of which have now appeared. From it we learn that hydraulic machines are of great antiquity and hence that necessarily man's observation of water flow begins with history. From many centuries of experience we have records of keen observation and reasoning on matters of principle. For example, Theophrastos realized that water waves transport motion, not mass, which suggested to him that sound is a similar undulation of the invisible air. Leonardo da

Vinci made a brilliant comparison between the waves on water and the waves which the wind sends travelling across a wheat field. He went far beyond these isolated remarks in asserting that in general the motions of water and of air are of the same kind. This assertion was based on experience. Leonardo traced streamlines in water by watching small objects cast into the flow, and from his pen we have sketches of waterfalls, river surfaces, and vortices as accurate as a photograph and more beautiful. To follow the motions of air he watched the leaves blown by the wind and even for gentler motions injected smoke into the current. It is now often claimed that Leonardo founded the experimental sciences, but I believe this statement is entirely misleading. Leonardo projected many experiments, some of them reasonable and some of them confused, but he has left us no record of ever having obtained any numerical value by measurement. Rather, he was an observer of undisturbed nature. Like none else he seized upon experience, but experiment lay many decades past his time. In fluid mechanics, as I mentioned, he devised methods for making an existing motion visible, but we have from him no numerical values of discharges or pressures. Leonardo recorded two quantitative statements in hydraulics. One of these is the principle of continuity in its simplest form. The other is the distinction between the rotation of a wheel and the rotation of a potential vortex. Leonardo's words, as always, are vague; if interpreted as strict proportions, his statements are correct, but it is possible that he intended only qualities and inequalities rather than equations. He gives no indication of how he obtained these basic principles, nor does he apply them in any way.

Whether Leonardo's notebooks through private and unacknowledged use influenced the numerous writers on hydraulics who came after him, or whether these discovered anew the facts described earlier by him, will always remain a question in debate. I have described Leonardo's work so as to make clear the keen and abundant experience available before Daniel Bernoulli's time. In addition to these fundamental observations, we must notice the increasing popularity of hydraulic machines from 1500 onward. Both for gainful work and for show or pleasure, pumps, presses, screws, fountains, wheels, conduits, and reservoirs were produced in greater and greater number.

After the principle of continuity, the next theoretical statement made by hydraulic writers was Torricelli's law of efflux, of little use for the design of a pump or wheel. Descartes was the first philosopher to assert that all nature is one great machine, governed by common laws. While nearly all of Descartes' physics is wrong in

detail, his grand attempt is the beginning of theory in the modern sense, and as a corollary it began in particular a search for a theory of hydraulics based on mechanical principles. This insistence on generality caused the physicists and geometers to reject all empirical rules and resulted in that separation of hydraulic theory from hydraulic practice, still apparent today, to which Professor Rouse referred in his opening remarks. In nature, the effects of friction, roughness, and turbulence can rarely be neglected and may altogether predominate, just as in ballistics neglect of air resistance would lead to wretched marksmanship. However, just as Galilei's mental abstraction of the medium in which all earthly bodies exist was a necessary forestep to the rational mechanics of solids, a similar abstraction of much of the daily circumstances of water was a necessary preliminary to the rational mechanics of fluids. This abstraction was made by Newton in his celebrated attempt to prove Torricelli's law. While Newton's fiction of the "cataract" is no more than a brilliant course of imagination and hypothesis, it is the first example of hydraulic theory, and as such, even though entirely faulty, it showed the possibility of the field and induced many other savants to attempt the problem. All these later trials were likewise failures. The forces exerted by fluids at rest were by now well known, but to consider the effect of motion on these forces seemed hopelessly difficult.

Such was the scene when Daniel Bernoulli, a young mathematician of twenty-five and already world famous, took up the study of fluids. Throughout all his life Daniel Bernoulli performed both experiments and calculations; while when old he became almost entirely an experimenter, at the period we are discussing he was in the main a mathematician, working not only in mechanics but also in analysis and the theory of numbers. For about five years he gave occasional attention to fluids, and during this time he wrote two important papers on hydraulics before attacking the simultaneous determination of pressure and velocity. The Bernoulli Theorem itself he discovered shortly before 17 July 1730, on which date he wrote to Goldbach as follows: "For my part, I am entirely plunged in water, which furnishes my sole occupation, the some time past I have renounced all that is not hydrostatics or hydraulics. . . . In these past days I have made a new discovery which can be of great use for the design of conduits for water, but which above all will bring in a new day in physiology: it is to have found the statics of running water, which no one before me has considered, so far as I know. . . . The problem is to find the effort of water which is pushed with an arbitrary force in an arbitrary tube." He goes on

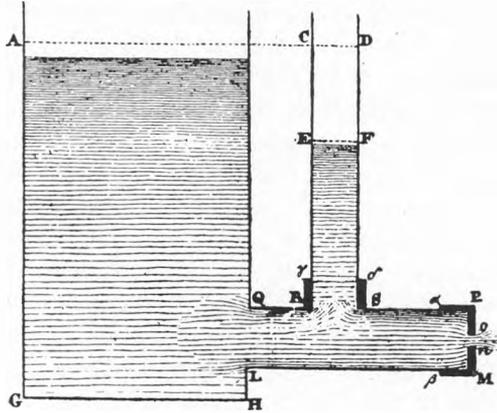


FIG. 1. THIS DRAWING IS NOT BERNOULLI'S ORIGINAL, BUT ONE MADE AFTER IT WHEN HIS LETTER OF 17 JULY 1730 WAS PUBLISHED IN 1843

to explain "one of the simplest cases," the example indicated by Figure 1. For the height SF' of the stagnant water over the hole in the tube of running water he obtains

$$SF' = \left(1 - \frac{1}{n^2}\right) a,$$

where a is the height of the reservoir and $1/n$ is the ratio of the area of the little hole to the area of the tube. This may not look familiar, but it is in fact the Bernoulli Theorem for this case, with the pressure replaced by the height SF' . The advantage of this form is that to test it no velocities need be measured, all quantities being geometric. Bernoulli wrote that an antagonistic senior colleague who belittled his work could not believe the result "... until I performed the experiment for him in the presence of other academicians. I made the experiments by means of a very polished iron cylinder which I had caused to be furnished with different covers which had holes of different sizes such as $aPM\beta$; in the middle of the cylinder was welded a little end of tube $\gamma RS\delta$ suitable for supporting a glass tube $CRSD$. All experiments succeeded perfectly."

Bernoulli's letter makes it clear that his theorem was discovered by theory alone, or, as he put it, *a priori*. Experimental test came afterward. You will note from the diagram that the experiment is devised so as to favor as much as possible the hypotheses under which we now derive Bernoulli's theorem.

Some explanation is required before we can recognize the equation written by Bernoulli as the modern theorem bearing his name.

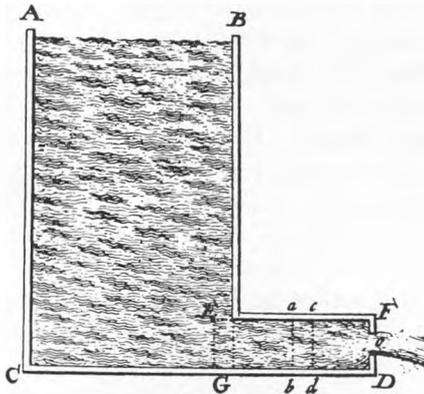


FIG. 2

When we turn to the derivation, even in the improved form published in his often cited but never read *Hydrodynamica*, still more explanation is needed. To repeat Bernoulli's words here would not be helpful. His method is to regard the element EG in Figure 2 as moving down the tube to $acdb$, where he wishes to find the pressure on the wall. The velocity there is related to the velocity at the small hole o by the principle of continuity. Bernoulli now imagines the tube downstream from ab suddenly to break off or dissolve. The element $acdb$, thus instantly released, suffers an impulsive acceleration. By using the principle of conservation of energy, Bernoulli calculates this impulse, which in turn he regards as proportional to the pressure on the wall when the tube is not broken off. The argument is intricate; the hypotheses on which it rests are questionable; and the details are confusing.

I have mentioned that Daniel Bernoulli set about at once to

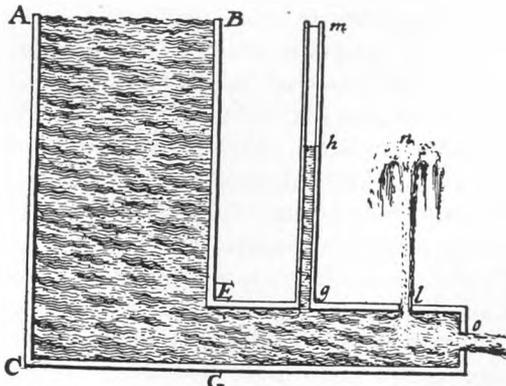


FIG. 3

verify his theorem by experiment. Figure 3 shows the experimental possibilities as presented in the *Hydrodynamica*. It is typical of Daniel Bernoulli not only that theory came before experiment but also that when the result was once confirmed by experiment, he regarded his work as finished. It is easy for us today to see that in fact the equation as written by Bernoulli is not very convenient; Bernoulli's contemporaries saw at once that his derivation was obscure and lacking conviction. What is missing from the equation itself and from the proof is the *internal pressure*, not yet invented. It was this same lack which had prevented Newton from giving an adequate proof of Torricelli's law. In his argument, Bernoulli used four different words, none of them defined, to express the forces exerted by the water upon itself and upon the walls of the tube.

The jealousy of the great Bernoullis among each other is well known. When the publication of Daniel Bernoulli's book in 1738 brought him redoubled fame, his father, the formidable John Bernoulli, who was then seventy-one years old, ill, and with but a decade still to live, grew furious with envy. The problem of proving Torricelli's law he himself had attacked many years before, in vain. All his remaining effort he put out upon the flow of water, and in 1743 he published his own treatise on hydraulics, dating it 1732 and embellishing it with florid and ugly boasting in an effort to steal priority from his son's masterpiece. The details of the controversy I cannot pause to repeat here; let it suffice that while the old man's outrageous procedure would suggest he was a mere thief, in fact his treatise created hydraulics anew. John Bernoulli had as great a talent for mathematics as any man who ever lived. While less successful in discovering the physical principles of a new field of experience, to derive by suitable new concepts and irreproachable reasoning a result already conjectured was something entirely suited to his genius. His innovations were profound. First, he separated the kinematic from the dynamic part of the problem. The principle of continuity and the principle of momentum he used consciously as separate basic postulates, as had none before him. Second, he created the idea of hydraulic pressure. In imagination, he isolated a thin slice of water in a tube and introduced a symbol for the force exerted upon it by the fluid on one side. In this way he achieved a differential equation and integrated it to obtain the Bernoulli Equation for a tube of arbitrary cross-section and position and for not necessarily steady flows. By using the internal pressure he was able to give also a correct derivation from the principle of energy.

The foregoing achievements of old John Bernoulli were not recognized as due to him until last year. Perhaps the reason is that

he never explained them with any clarity except in a series of letters to Euler. John Bernoulli lived in a world of challenges, enmities, secret methods, and anagrams. As he wrote to Euler, he derived everything from a certain "principle of the eddy," but even in his letters this is very vague, and in the printed treatise he abstained from expressing it from fear lest the English accuse him of borrowing the "cataract" of Newton. The progress of equations is clear, despite unnecessarily elaborate notations and a mathematical style which was by then obsolete, but mechanical principle is replaced by bombast and boasting.

For Euler, clarity was the hallmark of truth. He saw at once the core of old John Bernoulli's ideas and disrobed them of all vagueness. To him we owe the Bernoulli theorem in the form and terms today in use. To him we owe also the brilliant imagination of the internal pressure in full generality, the pressure field as equipollent to the action of the fluid outside any imaginary closed diaphragm upon that within. This concept, which has been the foundation of all further theory, he achieved ten years after his study of John Bernoulli's hydraulics. To discuss its formation would carry me afield from hydraulics, but I remark upon it in emphasis of the role of imagination and the importance of quantities which can only be thought of and cannot in themselves be measured. Neither is there time to discuss Euler's papers on hydraulic machines, where his grasp of the concept of internal pressure led him not only to detailed analysis of pumps and turbines but also to criteria for avoiding cavitation. These papers were neglected entirely by the hydraulic engineers of the day, and when Euler died in 1783 even a famous physicist of a younger generation characterized his work on fluids as useless in practice and merely exercises in pure mathematics. Euler did not perform experiments except before he was twenty, and thus he was unable to demonstrate the truth of his discoveries to practical men, who in that day despised calculus as being useless higher mathematics. However, Euler was intensely interested in machines, and in 1754 he not only invented the guide wheel for a turbine but even calculated a detailed design and gave a complete hydraulic analysis for the pressure in the rotating machine. Euler wished his turbine to be built, but the engineers at Frederick II's court only reflected the ways of the king himself in scoffing at all of higher mathematics. Euler published two papers, one in French and one in Latin, making his invention free to anyone who paused to read, but in fact it was 190 years before his design was tested. In 1944, long after Euler's guide wheel had been rediscovered and adopted in turbine practice, Ackeret found

that a model following Euler's plan reached an efficiency of 71%, as compared with 78-82% for the best modern turbines of similar capacity and head.

It is easy to praise or blame the actions of long ago, since we are free of responsibility in them. When we find parallel events occurring today, with no lesson learned, we are more ready to find excuses. I turn now to the recent discovery of cross-viscosity.

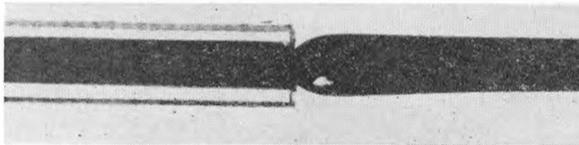


FIG. 4. AFTER MERRINGTON

There are various ways of telling the story, but I prefer to begin with a fact of experience which was a by-product of an experiment. In 1943 Merrington reported that in the course of some measurements of the discharge of rubber solutions or of oils containing metallic soaps he noticed that the fluid column swelled on emerging from the tube (Figure 4). Such an effect obviously cannot follow from any of the classical principles of fluid mechanics. Merrington himself asserted that the fluids in question are visco-elastic and that the swelling is due to their residual elasticity, being in fact recovery from the compression they suffered when forced into the tube. He identified the phenomenon with that observed by Barus in 1893. Barus had cut off perfect cylinders of marine glue extruded from a tube and had found that when left free of external load these cylinders continued to deform and in the end converted themselves into cups. Now Merrington's phenomenon occurs in steady flow. The spring of a portion of visco-elastic substance becomes poorer as time passes. Thus if we compare the swelling of steady flows in longer and longer tubes at the same rate of efflux, for a visco-elastic substance this swelling should diminish, since the portion emerging will then have suffered its compression at more and more remote times in the past. If, however, the swelling is independent of the length of the tube, the phenomenon is not visco-elastic. An experiment should not be difficult, but so far as I know it has never been proposed until today. To return to the story, apparently Merrington did not see any other possible explanation and did not pursue either theory or experiment concerning it.

About the same time several English experimenters noticed a group of new phenomena occurring in rotating fluids. The simplest of these, and the one which really explains all the rest, is that if a

rod is rotated in a cup of certain high-polymer solutions, or oils, the fluid climbs up the rod. The results of these experiments were collected and represented schematically by Weissenberg in a diagram published in 1947 (Figure 5). They were not connected with Merrington's phenomenon. Weissenberg proposed an elastic theory which appears to neglect the usual properties of fluids entirely, and some of the other investigators gave semi-quantitative explanations of a chemical nature.

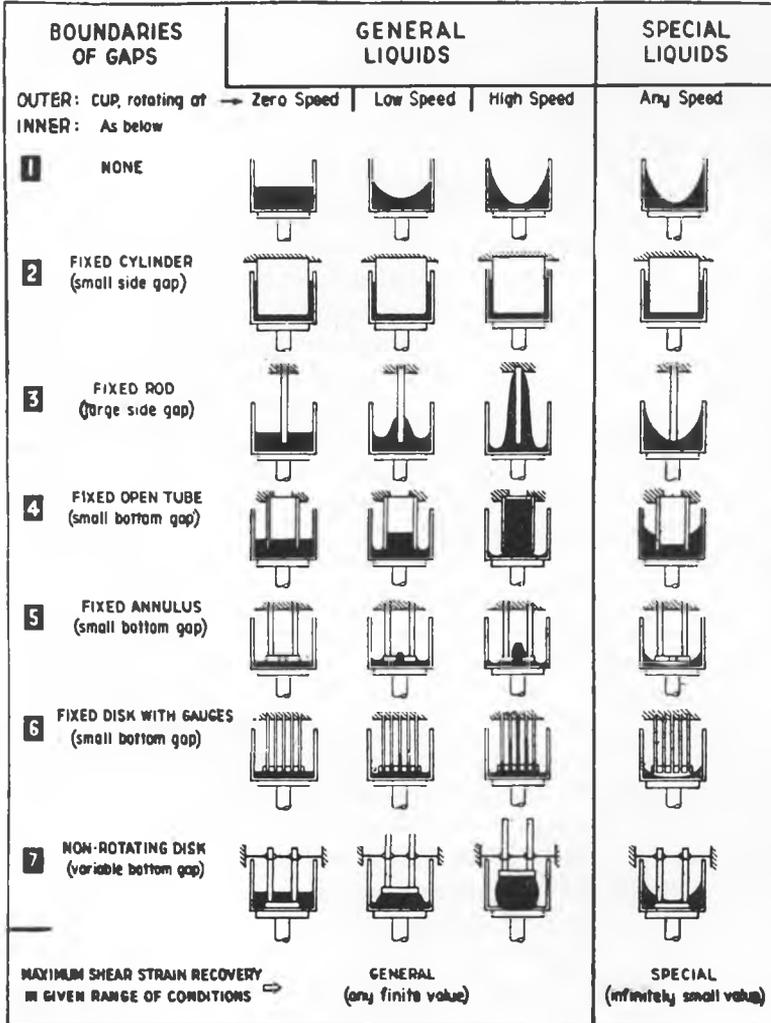


FIG. 5. AFTER WEISSENBERG

Between these two publications, three theorists began to study a subject which turned out to be related to these phenomena of experience. The subject is non-linear viscosity, and the theorists were Reiner, Rivlin, and I. Non-linear viscosity had been studied before, in fact long before, from two different points of view. Some theorists of the last century proposed some general equations as being reasonable, but in fact they did not investigate them sufficiently to get any definite conclusions. More recently a professional group called rheologists had measured departures from linearity in viscometric measurements. The rheologists were accustomed to one dimensional theories in which a single stress component is taken as a non-linear function of a single rate of deformation, but none of this literature could reveal a new phenomenon of a kind not included in the classical theory of viscosity at all.

Reiner is one of this rheological group and has published much work, both theoretical and experimental, of the type described. In 1945 appeared a paper of his of an entirely different character. In it he attempted to apply to fluids the mathematical methods and mechanical concepts used in the general theory of three-dimensional finite elastic strain, an old though little understood branch of mechanics which had been simplified in the previous decade by the introduction of tensor analysis. Reiner had in mind the phenomenon of dilatancy, which Reynolds long ago observed in granular materials: "a definite change of bulk, consequent on a definite change of shape." For example, when one shears wet sand in walking upon it, the footprints are dry, since the volume of the sand mass has increased and thus opened greater voids for the water to sink into. Reiner was able to show that this phenomenon is predicted by the general theory of non-linear viscosity. For his elegant and perspicuous formulation of the general theory itself he acknowledged the assistance of the theoretical physicist Racah.

Rivlin was employed by a laboratory investigating the properties of rubber solutions, and it is possible that his work was motivated by the phenomena published by Merrington and Weissenberg. In any case, he successfully and simply explained them by a theory of incompressible fluids with non-linear viscosity which he published in 1947. This theory is a special case of Reiner's, but Rivlin's work is distinguished not only by greater definiteness and clarity but also by explicit and general solutions for the flow in torsion, tube, and parallel plate viscometers. Particularly surprising is the fact that Merrington's swelling, which Merrington himself regarded as recovery from compression, follows from Rivlin's theory of incompressible fluids. To get the idea behind all these effects it is

easiest to consider simple shearing flow. To produce such a flow, according to the general theory, shearing stress is not enough: normal pressures on the shear planes must be supplied as well. This phenomenon is called *cross-viscosity* and is a property altogether independent of shear and bulk viscosity.

Once one has the idea of the general theory of viscosity and a little experience in tensor analysis or matrix algebra, the explanations are not difficult. Each individual phenomenon can also be explained by physical reasoning, but since these physical arguments came only after the really rather simple mathematics was all worked out, I doubt if they are in fact very enlightening, and I will not try to present them.

My own first crude memorandum on non-linear viscosity was issued in 1947, when Reiner's basic paper was already two years in print and Rivlin was far ahead of me. I knew neither their work nor the phenomena of Merrington and Weissenberg. Explanation, though no excuse, for my ignorance may be found in the apathy if not hostility of the world of fluid mechanics toward the subject of non-linear viscosity: all my mentors and colleagues, at that time as now intent upon practical problems and calculations, took no more notice of the work of Reiner and Rivlin than of my own attempts. My formal publication was delayed by the rejection of my paper by a journal of applied mathematics, on the grounds that no one was interested in the subject, the paper would be costly to print, and in any case my work was physics rather than applied mathematics. The terms used by the anonymous referee were so harsh that the only logical alternative to suicide was to give up science forever. While my happening to discover the identity of the referee prevented me from resort to either of these extremes, before arranging for publication abroad I spent eighteen months reviewing the fundamentals of mechanics and trying to learn the processes by which the classical theories had been derived by their discoverers. Priority for non-linear viscosity belongs unquestionably to Reiner and Rivlin, but I speak of my work as well because I have a better knowledge of my own motives and circumstances than of theirs.

For my part, my trouble was that I was employed to study fluids but I could not accept the so-called derivations of the Navier-Stokes equations given in textbooks. It seemed as unreasonable to suppose viscous stress a linear function of rate of deformation as to replace every curve by a straight line. I was aware of claims of departures from the Navier-Stokes equations in certain extreme conditions, but I was more surprised that the Navier-Stokes equations hold at

all, and I set out to find the reason. Being naturally both slow and obstinate, I resisted the pressure to calculate or guess useful approximations within accepted theories and to the great annoyance of my superiors and the disgust of my senior colleagues insisted on stopping to think. Textbooks hurry the reader on to accept their conclusions as quickly as possible, often replacing a logical gap by asserting that the result is established by experiment, without a reference. Such evasion is not found in original memoirs dealing with matters of principle. The discoverer or first proponent not only has the task of convincing a skeptical public but also often is close to his struggles to convince himself. Moreover, usually there are no relevant experiments at the time when the theory is first formulated as a plausible model of experience. This statement always surprises believers in the "experimental method". In reply to it they often suggest that if in the history of mechanics theory has usually come before experiment, there must have been many wrong theories proposed. In fact, there were few. Without experience, no explanation definite enough to be considered a mathematical theory is likely to be given; with experience, to expect theorists to propose wholly wrong models suggests a rather limited appreciation for the brains of theorists. Analysis of the dust pile of mechanics reveals few wrong theories but a host of "approximate" or numerical solutions and experimental measurements concerning details and special cases which have lost their interest, as well as many mathematically erroneous "solutions" within correct theories. Today we are piling up this scrap heap so fast that it is difficult to keep the rare cases of fundamental work out from under.

Going back to the great memoir of Stokes on viscosity, I found none of the dogmatism of the modern texts, but instead an honest hesitancy and search for principle. Taking up Stoke's definition of a fluid, I sought by the aid of tensor analysis to put into mathematical form precisely what Stokes had said in words rather than the mere approximations which were all that the mathematics available in his day could easily handle. My work was influenced also by a study of modern general elasticity theory and its mathematics, but while Reiner had attempted to maintain as close a similarity as possible, to me it was the basic conceptual differences which seemed more important. Some earlier writers had spoken loosely of the Navier-Stokes equations as being valid approximately for "small" rates of deformation, just as classical linear elasticity is valid approximately for small strain. This is plain nonsense. Strain is dimensionless and hence can indeed be small, so that the position of linear elasticity with respect to finite elasticity is clear in this

formal sense. Not so for fluids, for rate of deformation is of the dimension T^{-1} and hence cannot be absolutely small. It can be small with respect to another rate, but what this standard of comparison should be for a fluid is not obvious. My work on general fluids began at this dilemma, faced it, and resolved it by proposing a theory in which no material parameter of the dimension T can occur. When I learned of the work of Reiner and Rivlin, I found that they had not considered the role of the time, and I was able in some cases to show differences between fluids having a natural time and fluids which do not.

I mentioned the phenomena published by Merrington and Weissenberg, which were soon taken up as evidence for the existence of cross-viscosity. After awhile it was realized that cross-viscosity was not really new. Everyone knows that you cannot stir paint with a rotary motion, for the paint climbs up the rotor. In the paint industry, other methods of stirring were devised and the phenomenon itself apparently regarded as chemical. Here the experience lay before us all, but we were blind to its meaning.

Finally came the time for experiment. All old measurements on non-linear viscosity were made obsolete by the theory, since they measured only small corrections to classical effects and offered no means of detecting even the existence of the new phenomena. The old viscometers have walls supplying lateral pressures of any desired amount with no means of measuring their magnitude. Moreover, it follows very generally from the theory that while departures from the classical first order linear relation is an effect of third order in the rate of deformation, the new phenomena are effects of second order. Thus it is quite possible that fluids previously believed linear in the range tested are in fact non-linear. Precise tests in new instruments designed to show the new effects are necessary, first to measure the modulus of cross-viscosity as a function of the rate of deformation, and second to test the consistency of experiment and theory. Such measurements are now coming into print. Some of these appear to confirm the theory of non-linear viscosity and others do not. In any case we must remember that the classical theory of linear viscosity is a first approximation to several different more general theories, so that not all fluids which obey the classical laws for slow motion can be expected to obey any one particular theory for rapid ones.

The phenomenon of cross-viscosity is altogether typical of non-linear continuum mechanics. Every more or less plausible theory predicts something of this kind. While a few years ago this phe-

nomenon and others like it seemed* outside the domain of mechanics, with the recent development of many new theories we are faced with the opposite difficulty of being unable to use these phenomena as confirming any one theory rather than another. In fact, as I say, these cross effects are typical of mechanics. Everyone knows that if you push a gyroscope, it refuses to move in the direction you push it. This illustrates the general case in mechanics: only when a material is barely stirred out of its sleep will it answer to your wishes and move approximately as you impel it. A century of unquestioning acceptance of linear theories in mechanics has lulled us into expecting response which is not typical. It appears likely that non-linear effects will be discovered in increasing number and may eventually have a greater practical importance than is now foreseen.

My two stories are finished, and I have promised to draw no moral. I hope you will not consider my promise broken if I let Daniel Bernoulli draw a moral. In the quotation which follows, the word "mathematician" occurs, but it comes from late in Bernoulli's life when he was absorbed in experiment, and it obviously refers not to a professional group but to a habit of mind. Here is the quotation: "...there is no philosophy which is not founded upon a knowledge of the phenomena, but to get any profit from this knowledge it is absolutely necessary to be a mathematician."

*Excepted to the very few persons who knew of certain special results of Poynting (1909-1913) and later writers concerning shear and torsion of elastic materials.

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