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The Shoe on the Other Foot

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IT MAY be in order to take stock today of some problems encountered in the glass industry which turn out to be not the problems we think they are, and to show how strenuously some of us try year after year to put the shoe on the wrong foot, and don't realize that that is what is happening.

Variation of Strength of Glass with Size of Specimen

A great many people have measured the strength of glass fibers, and all of them have reported that the strength increases as the fiber diameter decreases. Here, for example, is a table taken from the work of A. A. Griffith (1920) at the time of World War I. The results are shown graphically in Fig. 1.

Table I. Strength of Glass Fibers

Diameter in thousandths of an inch	Breaking Stress, lb. per sq. in.	Diameter in thousandths of an inch	Breaking Stress, lb. per sq. in.
40.00	24,900	0.95	117,000
4.20	42,300	0.75	134,000
2.78	50,800	0.70	164,000
2.25	64,100	0.60	185,000
2.00	79,600	0.56	154,000
1.85	88,500	0.50	195,000
1.75	82,600	0.38	232,000
1.40	85,200	0.26	332,000
1.32	99,500	0.165	498,000
1.15	88,700	0.130	491,000

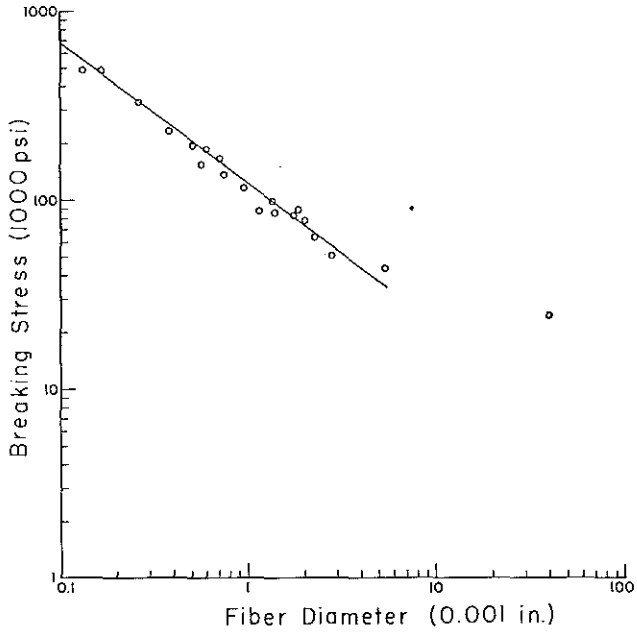

Fig. 1. Strength vs. fiber diameter.

Plummer (1938) obtained similar results: so did Anderegg (1939), and many others. There is no reason to doubt that so many excellent experimenters did obtain some such results and that the results mean something. A number of people have tried to account for their results. Some twenty-five years ago I indicated, in a letter to Professor W. E. S. Turner, that there might be some factor other than diameter involved. I did not know what it was, and suggested that, if he was experimenting on the subject (and he was), he should make sure that the *length* of specimen was held constant while the diameter varied. This was a bad guess. Bailey (1939) evolved a theory of strength taking into account the probability of a "flaw" being present in the highly stressed surface, the probability being greater when more surface was stressed. This is a good theory, and other people have used somewhat similar concepts, based on the idea that the strength of a chain is that of its weakest link, and postulating that the links have a statistical variation in strength. Littleton stressed the point, made also by Griffith, that whatever was wrong was something in the surface, and by preparing and protecting the surface he made sizable rods of great strength (see Shand' 1952). On the other hand, the data on fibers could be accounted for on the supposition that the surface actually contributed some element of strength (Preston, 1933): this as Condon was to say twenty years later (1953) might agree with the figures but seemed completely obscure as a matter of physics.

Orowan (1949, p. 198) in an extensive review of fracture and strength has a page devoted to the "Size Effect," and notes that Griffith's results could be expressed by a formula which had fitted Karmarsch's results (of 1858) on the strength of fine wires, and that Reinkober's experiments on thin silica fibers fitted Karmarsch's formula even better than Griffith's did. Orowan then adds parenthetically that Karmarsch's results on metals were not confirmed: they were probably due to differing amounts of cold-work on wires of different diameter, i.e., to different "constitutions" of the different wires. However, Orowan indicates no suspicion that Griffith's glass fibers or Reinkober's silica ones might differ in constitution: he treats their effects as genuine, though Karmarsch's may have been spurious.

And so we might go on, showing how a great many people made conscientious, and sometimes laborious, efforts to account for the phenomenon, with very indifferent success. The real trouble was that the phenomenon was not there to be accounted for. At one time I suspected that Corning knew this, but in view of Condon's Iowa lectures I now suspect that they didn't.

Anderson (1953), at the New York meeting of The American Ceramic Society, was still trying to account for it, and Condon (1953) in his lectures at the University of Iowa was also trying. The answer had been known to a few of us for almost ten years, mainly as the result of work by Otto at Owens-Corning Fiberglas. Since Otto commented briefly on his findings in the discussion of Anderson's paper, and will pre-

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* References listed alphabetically on p. 358.

sumably present a full account of his work before long, I shall be rather brief in the present paper.

We became aware, during World War II, that it was not enough to define the composition of a glass and the diameter of fibers made from it. Nor was it enough to prepare the fibers in substantially the same manner. They had to be prepared in very exactly the same manner. Now when the early experimenters prepared fibers of different diameters, they usually drew the fine fibers faster, so that they cooled more suddenly and perhaps "froze in" a "constitution" characteristic of a higher temperature. Further, the finer fibers were often drawn from glass that was actually at a higher temperature, in order to permit their being drawn down so fine. Briefly, fibers of different diameters were produced under different "forming conditions," and it was this that was making the difference. Otto made up special apparatus that permitted the simultaneous drawing of fibers of different diameters from the same mass of molten glass, and when this was done there was no longer any effect of diameter upon strength. Coarse fibers were as strong as fine ones. This result is of great theoretical importance, and may be of great practical importance, but I shall leave it to Otto to tell his own story.

Orientation and Strength

It had been suspected by many that the high strengths commonly found in fibers might be due to a parallel orientation of elongated molecules or something of that sort. A number of people put considerable effort into this hypothesis; but once again it was effort put into trying to get the shoe on to the wrong foot. The evidence has always been plentiful that silicate glasses ought theoretically to be very strong; the energy of their chemical bonds calls for it, and no orientation is required to account for it. The calculated strength comes out at quite a variety of figures according to the theory of strength that you adopt, but no theory prophesies a strength *less* than the observed strength of fibers. Indeed, *every* hypothesis prophesies a greater strength, in all orientations, than we actually observe. Therefore, we do not have to account for the strength of glass, but for its weakness.

None the less, it seemed advisable to check whether there was any orientation of strength in fibers. This also was done by Otto (1950). The experiment consisted simply in breaking fibers of glass in tension and in torsion, and in computing the breaking strength in the two cases. It was found that they were equal and this proves that strength is isotropic, and not oriented.

Brannan (1953) confirmed the lack of structural orientation by another test. If there were strong bonds longitudinally in a fiber and weak ones crosswise, he ought to get a high value of Young's modulus in tension and a low value of the shear modulus in torsion. He ought to find fibers hard to stretch but easy to twist. He didn't. The relationship between the two was normal, which is consistent only with an isotropic material.

In view of this direct evidence, it seems unnecessary to try further to get the shoe on the wrong foot.

In my "Edinburgh Lecture" (Preston, 1949) it is apparent that I have had a lot of fun working with the research men at the Preston Laboratories: it will be apparent from what I have been saying here that I have had lots of fun also in collaborating with people outside the laboratories, people like Otto and Brannan, for instance.

Standard Deviation or Scatter of Strength Measurements

I now turn to a development that came about inside the laboratories.

In the ordinary course of testing glass specimens for strength, it is usual to find a considerable variation from specimen to specimen. Even when the specimen has a simple shape and all specimens are prepared with care in as identical a manner as possible, this variation remains large. It is sufficiently large that it adds greatly to the burden of routine testing, both by requiring more specimens to be tested and by requiring the use of statistical analysis to make sure of the results. This large variation in strength is quite unlike the almost minute variations in other physical properties of glass and looks more like the variations we find in the biological world than those we find in physical and chemical laboratories. Since this fact will be known to all of you who are in the glass industry, I need not elaborate on it. It is usually expressed in terms of a "standard deviation," and when the standard deviation is expressed as a percentage of the average strength, it gives a clear picture of the degree of variability. We may often find it coming out as 10 or 20% of the "mean" or average. This suggests that once again we have our thinking in reverse, and are trying to get the shoe on the wrong foot.

It is probable that the clue was given a generation ago by J. T. Littleton, when he said, "We do not measure the strength of the glass, but only the weakness of its surface." But Littleton did not explain the technique for measuring "weakness."

Now there are three ranges of "strength" that have been investigated in glass. One is the low-strength range, of the order of 5000 to 10,000 lb./sq. in. Most large glass articles are somewhere in or near this range: things like window panes, laths of glass, jars and bottles, and so on. A lot of practical information is available about glass in this range. Then there is the medium-strength range, typically 50,000 to 100,000 lb./sq. in., say. This is the range in which Tooley's (1952) carefully prepared rods tended to fall when he was investigating the effect of surrounding liquids upon strength. Finally, there is the high-strength range, typically, let us say, around 500,000 to 1,000,000 lb./sq. in. This is the range in which properly made glass fibers fall.

I do not mean to imply that glass never falls into levels intermediate between these, or that it is not sometimes reported lower than 5000 lb./sq. in. or higher than 1,000,000. All I am trying to do is to define for my own convenience, and yours, three ranges of strength differing by an "order of magnitude" one from the next.

Some years ago, at the Preston Laboratories, R. E. Mould began the attempt to see how high a strength he could find in glass. It was surmised that we might with advantage use fibers, that we ought to test as minute a surface area as possible, and it was practically certain that we ought to use the shortest time-duration possible. This combination of requirements, after much investigating, seemed to call for a cross-bending test of fibers about the size of a human hair, tested on a very short span and loaded electrically for a period of a fraction of a second. This is a rather coarse fiber by commercial standards, but the limitations of mechanical engineering, and of the structure of metals used for knife edges, have so far prevented our using still smaller specimens.

According to Griffith, fibers of this coarseness might be expected to give a strength around 80,000 lb./sq. in., but Mould began to find strengths around 700,000 lb./sq. in., and he also began to suspect that his fibers were not showing the usual "scatter," but his apparatus was a little temperamental, and we were not sure just what was going on. Dr. Lasday revised and remade the apparatus and Dr. Hansen devised a method of measuring the diameters of the fibers with very great accuracy. This last is an essential element in the problem, for it is not easy to measure fiber diameter to a tiny fraction of one per cent, and unless it is so known, the strength cannot be determined.

Lasday found the strengths of some fibers were in fact around 700,000 lb./sq. in.: other sorts were perhaps around 500,000. But the important thing was that for a given sort of fiber, specimen after specimen agreed to extremely close limits, the standard deviation having fallen to a fraction of one per cent. This deviation includes variations of diameter from specimen to specimen, which may quite possibly account

for most of the remaining variability. In other words, we were now apparently measuring some physical property of the glass and not the statistical variability of its injuries.

If this is the correct interpretation, when it is found on a larger and more imperfect specimen, that the strength is only 5000 lb./sq. in., and on the next specimen only 4000 lb./sq. in., what is being discovered is that one was 99% ruined and the next one 99.2%: or, measuring "weakness" down from 500,000, lb./sq. in. (say) instead of measuring "strength" up from zero psi, we should report that one was 495,000 lb./sq. in. "weak," and the next was 496,000 lb./sq. in. "weak." The measurements then take on the normal appearance of data from a physical or chemical laboratory. The average of our measurements of "weakness" might be, say, 495,000 lb./sq. in., and the standard deviation 1000 lb./sq. in., or 0.2%. This replaces the standard deviation of 20% if the figure is computed on *residual strength* instead of on weakness or *lost strength*. Once more it looks as if we have been trying to put the shoe on the wrong foot.

The Merit or Lack of Merit of "Stippling"

I think it was John Tyndall who commented that Scientific Research is a game you play against an extremely skilled but absolutely honest opponent. That opponent is Nature. You ask questions and she answers them. She tells no lies, but she always answers the questions you actually ask, not those you intended to ask or think that you asked. The skill of the research worker consists in asking the right questions and in being sure what the questions really mean. This involves a much higher order of skill than may appear at first sight.

In the humble field of glass technology, even of rather routine testing, we come across many examples of this. I have already given you some. I may add a few others.

Bottles are sometimes plain and smooth on the side walls, and sometimes stippled, that is, covered with tiny flattened pimples. Which is the stronger kind of bottle? There is some reason to think that the roughening of the surface might make some difference, though this is not absolutely certain, since we do not completely understand all the factors involved. So the best thing is to make some bottles of each kind and find out. Fortunately, this is easy. The bottles can be made of the same weight and same capacity and can therefore be made concurrently from the same tank and the same feeder. All you need are two molds (or more) on the machine, one having a smooth side wall and the other a stippled one. Then you collect a reasonable number of each, subject them to a pressure test, obtain your "means" and "standard deviations" and use your brains or the resources of statistical methods or both to find out whether stippling is good or bad.

The first time we tried it, many years ago, there was a distinct advantage to stippling. The second time, also many years ago, there was definite indication that the plain bottles were superior. This was disconcerting. It turns out that stippling, apart from marking the surface, affects the "distribution" of glass in the mold cavity, and this effect is probably in most cases more important than the surface marking. Thus, once more it is the "forming conditions" that count. The exact parison shape may be significant, favoring one type of blow mold or the other, and a change in blank mold design might reverse the favoritism.

But a funnier situation has often obtained in the factories of the industry, when they have tried for themselves to establish whether stippling is good or bad in a particular instance, using a fixed blank design. It has sometimes happened that though the stippling was confined to the side walls, all bottles actually broke in the base, so that differences in the bottom plates were responsible for all the variations that were encountered, and side wall design had nothing whatever to do with the case. Yet I am told that sometimes "fracture analysis" was not applied to the tested bottles, and the factory believed, for a time, that a definite superiority (or inferiority) had been scientifically established for stippled bottles.

Effect of Weight upon Strength of Bottles

A rather similar experience was ours some fifteen years ago, when Hartford-Empire set out to establish, with our collaboration, what the real effect of weight of bottles was upon strength and serviceability of "Steinies." This was a worthy project, well conceived, and credit must be given to Karl Peiler and J. C. Hostetter for undertaking so difficult an assignment. That the result was a fiasco was not their fault.

The idea was to take a Steinie mold of definite size, or a set of such molds, and to make a good substantial "run" of bottles in them, using "gobs" weighing, say, 8 oz. Then to make another run with 10-oz. gobs, and so on up to perhaps 12-, 14-, and 16-oz. gobs. The bottle walls would thicken, except at the "finish," and then the effect of wall thickness on strength could be tested.

Note that this is a more complicated matter than the simple stippling or nonstippling of bottles, for bottles of different weights cannot be made concurrently on the same feeder. In practice they had to be made on different days. They cannot be made at the same rate, so many bottles per minute. The heavier bottles have to be made more slowly, to give them time to set up. They cannot be annealed on identical schedules to the same degree of annealing, for strain-release varies rapidly with thickness. The task, therefore, was known to be formidable, but there was scientific manpower available in those days, and it was thought the problem was not insuperable.

We did not realize, perhaps, how serious was the problem of getting an ideal parison for each thickness of glass. In fact, I am not sure there is an ideal parison for anything but an ideal gob-weight. In any case, we finished up with the information that the glass in the tank varied in homogeneity from day to day, and that everything else was varying, too, but we did not get any clear indication of how bottle weight affected either pressure-strength or thermal-shock-strength. We found ourselves, through the limitations of the manufacturing processes of those days and through our own scientific limitations, quite unable to ask the right questions.

It is only very recently, by entirely different methods of approach, that Dr. Ghering and his assistants have answered that question, and I think it fair to say they now have a pretty good idea of the answer. This came about only after they gave up entirely trying to get the wrong foot into the shoe, trying to pose the question by actually making bottles of different weights and subjecting them to routine testing. The earlier method is really trying to solve the problem by brute strength and awkwardness: it has all the finesse of a bull charging a haystack. It was probably worth trying, for it made it clear that we had to try either the other foot or the other shoe, and I think we have now found the combination.

Relativity

I have given these illustrations, taken in some cases from new information not yet on the library shelves, and in other cases from information, which, if not strictly new, is not yet publicized, to illustrate a general principle, viz., that sometimes we study shoes in a very profound way and assume that we know all about feet. Hence, the possibility that we may be using the wrong foot does not occur to us.

The most famous example of this in scientific thought is connected with the name of Albert Einstein. Fifty years ago Sir Oliver Lodge, one of the pioneers of science, had felt able to say that more was known about the light-carrying ether than about any material substance. Yet all attempts failed to determine which way the earth was traveling through the ether. The earth behaved as if it were at rest with respect to

the ether, in spite of the fact that it is spinning on its own axis, revolving round the sun, and scurrying with the sun at an enormous pace across the sky. It took a young Swiss Jew to get the scientific world off its wrong foot. He postulated that motion through the ether is inherently unobservable. All that can be observed is the motion of one material body with respect to another, not with respect to the ether. Then he proceeded to work out the consequences of such a postulate, and so was born the Theory of Relativity.*

I do not know how many other shoes we are trying to get on the wrong feet. I have merely preached you a little sermon, hoping to encourage some of the younger men in my audience, perhaps, to be courageous enough at times to question the foot as well as the shoe.☆

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* Perhaps it is only proper to point out that although the entire Theory of Relativity is credited to Einstein, the way had been paved for this fundamental attitude by the work of Poincaré.

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