WORDS OF FAMOUS MEN AND THE WORK OF THE PRESTON LABORATORIES.

By Dr. F. W. Preston.

(A Lecture to the Society at its Edinburgh Meeting, on May 25th, 1949.)

In the last twenty-five years or thereabouts it appears that I have written some twenty papers for the Society of Glass Technology, but this is the first time I have been present to speak to you in person. In the same time I have written perhaps a hundred and fifty other scientific papers, and the members of the Preston Laboratories' staff have also written a number. Your president, Dr. Hampton, and your editor, Dr. Turner, in letters I received before leaving America, have in effect indicated that they would like some account of the Laboratories and their work, more particularly for the benefit of those members who have never visited them (as both these gentlemen have), and some background of the American scene as it has affected that work.

Therefore I am to try to show you what the Laboratories are like, what they do and why they don't do something else. And this, I think, can best be done by tracing their history and the influence of men, many of them now dead, whose remarks have had a bearing on the matter.

Now the Preston Laboratories, while they have had to concern themselves with many phases of glass technology from time to time, have tended to concentrate their attention as much as possible on mechanical properties. For though glass is useful for a great variety of reasons, the most important reason is its mechanical strength and its permanence of shape. This, however, is not the reason, at any rate not the principal reason, why we have been interested in mechanical properties. What has interested us most is the problem of breakage; not simply the economic problem of breakage, which is important enough, but the physical problem of what goes on when glass breaks. It looks superficially very obvious and straightforward, but as soon as you begin to look into it, it becomes exceedingly peculiar and hard to understand.

I do not intend to-day to go into deep matters in this field, but since this is my first appearance before you in person, even though, as a Japanese once told me, " I have met you often in your writings," I thought I would rather trace a thread or two of the history of glass technology, and show you how things came to be, rather than attempt to assess just where we stand at this moment. So let us go back, for a few minutes, to those nostalgic days ahead of World War I, when there was freedom in the world, when travel was not fettered by governments, when it might be

folly, but certainly not a crime, to be a scientist, when government by income tax had not been invented and the population of the world was beautifully less.

The photographic process, on glass plates, had been invented, and lenses to go in the camera. The simplest lens is a double-convex " positive " lens, like a burning glass. It forms an image of sorts, but it is not " achromatic on the axis." Different wavelengths focus at different distances from the lens. This separation can be corrected, to a first approximation, by cementing a suitable negative lens to the positive one. Then we get an achromatic doublet. We now find, however, that we have trouble " off the axis ": a square appears in a photograph as a barrel or as a pin-cushion. This can be overcome by placing two doublets back to back, and putting the " stop," and usually the shutter, between them. The combination is known as the " Rapid Rectilinear ": rectilinear, because straight lines in a photograph are straight lines: rapid? well, not by modern standards.

This was the state of affairs a little before the turn of the century.

The problem of designing a lens is partly a problem of having available a variety of optical glasses with different optical properties, and partly a matter of having enough " disposable variables " in the form of free surfaces and separations. Now a Rapid Rectilinear has four pieces of glass, and hence eight surfaces; but two surfaces are not " disposable," because they are cemented, and must conform to the contours of their mates. Further, they are not separated, so that there is not an " air lens " between them. It occurred to Dennis Taylor of York that three pieces of glass, all separated, all with different curvatures, would give more disposable variables, and he designed such a lens in 1893. This was the first of the anastigmats. It was much more rapid than the rectilinear, and much more highly corrected. But his firm, who were more interested in telescopes than anything else, said: " You have invented a camera lens, not simply a telescope lens. It is a good lens, but it will do far more than a telescope is required to do. Further, it is a very difficult lens to make, and still more difficult to assemble with the required precision. This lens ought to be taken to William Taylor of Taylor, Taylor and Hobson: if anyone can make it as a commercial proposition, he can, and he is in the business of camera lenses."

So William Taylor bought the rights in the lens for camera purposes, and under the terms of the sale, it was to be called the " Cooke " lens, named after Dennis Taylor's firm at York; and the firm of Zeiss sat up and took notice that a phenomenal new lens was born into the world. Their reply came about twelve years later.

Zeiss's thinking was simple. The Cooke lens consisted of two positive lenses, with a negative lens between them, and all separated. This gave six disposable surfaces and two separations. Zeiss would take the simple rear component and make a doublet of it with a deep cemented contact, thus obtaining an extra piece of glass and one more disposable surface.

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This was the Zeiss Tessar, and it is still a famous lens. It soon began to make inroads into the Cooke business, and it was now Taylor's turn to sit up and take notice.

William Taylor was a scientist at heart. He said: "The mathematicians are doing all this. We have to employ a mathematician to improve the Cooke lens." The man he picked was Arthur Warmisham, who had just taken his Master's degree at Manchester, under Lamb, studying hydrodynamics. It is a long way from hydrodynamics to geometrical optics, but the step was not beyond Warmisham. He moved to Leicester, and began his studies, but he was ill, and became worse. The doctor said he ought to live in the country, so he moved to the Charnwood Forest. William Taylor said: "When you feel fit, do some computing. The object is to make a three-component lens as good as the Tessar's four. When you think you have something on paper, come in and see us, and we will make up a model and test it."

By and by Warmisham came in. "Here is a lens; it is better than the Cooke of the corresponding series, but not so good as the Tessar. You might try it." They did, and he was right. Then he came back with another design. "This is better than the Cooke of Series II; but I fear it falls well below the Tessar." It was, and it did. Finally, he said: "Give me four pieces of glass, and I can beat the Tessar." "Well, we'd like to see it, and we will finance the work. But we do not want a four-component lens, and shall probably feel unable to market it." Warmisham came back with his four-component design. It was a marvel. But it was extremely difficult to make. They made a model, and said: "Our manufacturing skill is not equal to your skill as a designer. From our point of view the lens is marvellous, but quite unpractical, and we ask you to improve three-component lenses to the limit of their capabilities." Then the lens was pigeon-holed.

Not long thereafter, World War I broke out. The lights went out in the North Sea; then they went out all over Europe. Then trench warfare set in, and with it, reconnaissance by aeroplane. Soon the British War Office was advertising in the London *Times*: "Will any photographers who have Zeiss Tessar lenses please turn them in to the War Office: they are urgently needed." All the British lens manufacturers rushed to Whitehall to demand what was wrong with *their* lenses. The War Office pulled out aerial photographs taken with *their* lenses and with Tessars: "Now *you* tell *us* what is wrong." They went away with their tails between their legs.

Then Mr. W. B. Appleton of Taylors' thought of Warmisham's pigeon-holed lens. He dusted it off, took a number of test-pictures with it and departed for Aldershot. This lens and a Tessar of similar focal length were mounted side by side in two identical cameras, put in a crate which we dignified by the name of aeroplane and taken upstairs. Simultaneous pictures were taken with the two cameras, marked with cryptic signs, developed and printed by persons who did not know which was which. Then the

inspectors were told to choose the better prints. In all cases they chose Warmisham's. This christened the lens. It became the "Aviar." The War Office's problem was solved, but Appleton's problems were only beginning. The War Office gave him an order for vast numbers of lenses, but he had no idea how he was going to make them. He had had enough difficulty making even one.

After some wearisome struggles, William Taylor said: "Well, if skill cannot do this unaided, maybe science can help. We have to hire a scientist or two to find out how to make the thing." I was chosen for the job, and my admiration for William Taylor's insight into mechanical affairs, and Arthur Warmisham's genius, has never diminished. In later years William Taylor was elected a Fellow of the Royal Society and President of the Institute of Mechanical Engineers. Arthur Warmisham has astonished everyone with his unending stream of inventions in the field of lenses and with his insight into many other problems.

Some months went by. In the daytime I worked at Taylor's; in the evenings I worked at the Technical School. One dismal night, in the black-out of World War I, I was walking home, as I often did, with a young man named Wilfred Hampton, who was then a chemist at the Municipal Gas Works. "Well," he said, "I think I'll try once more to join the Army. To-day I finally quarrelled with my boss. From a scientific point of view he is completely hopeless, and there is no sense continuing this way."

"Do me a favour," I said. "Apply for a job in the research laboratories of Chance Brothers, West Smethwick, and make me some optical glass of more constant refractive index." "Glass? I don't know the first thing about glass." I urged him to try. He applied, and got the job. He has been stuck with it ever since. But he has never borne me any malice. He has tried to put it out of his mind, and has even denied that he remembers the occasion. I take this to be evidence that I have been forgiven. Be that as it may, it is I whom you must thank for the fact that Dr. Hampton is now your President

Before World War I began, three or four 'teen-age boys were sitting in the shadow of a hedge in Leicestershire, watching the birds and talking of scientific things. One of them said how wonderful it must be to do scientific research, and make new discoveries previously unknown to Man, like an African explorer in the previous century. Hampton replied with a matter-of-fact assurance: "No one who puts in a reasonable amount of time studying scientific matters can avoid making a new discovery. There is so much to discover, and so little known, that it is utterly impossible not to make discoveries." He was, in fact, already beginning to make them himself, and was completely without conceit about it.

So Hampton went to Chance Brothers. It was his job to worry about the glass while it was hot, and to deliver it to me, and others, in good condition when cold. Thereafter the cold-tooling of it was my problem—and Warmisham's, for he was much more than a first-rate mathematician.

None of us had had any formal training in our respective fields: we did the best we could with a basic faith that things are ultimately intelligible, though they may be very different from what we think at the start.

Now at approximately the same time another young scientist entered the glass industry by a curiously indirect route. His name was W. E. S. Turner. Since I have already, in a talk in America, recounted my first meeting with Dr. Turner,* I need not do so again. My next meeting with him was an accidental one, in St. Pancras Station in London, and I said, "Dr. Turner, I presume?" Then I met him, not entirely unexpectedly, in a hotel in Paris, and a year later I attended a musical recital one Sunday afternoon in Toronto, Canada. Looking over the front of my pew, or whatever you call such things in a concert hall, there I saw Dr. Turner. "Dr. Preston, I presume?" he said. So, whenever I get to some place where I never was before, I take a careful look around to see if Dr. Turner is there ahead of me.

And so, with Turner and Hampton in the glass industry, the stage was set somewhat more than thirty years ago for to-day's meeting; but in all that time, so far as I know, the three of us were never on the same stage together, or present at the same scientific meeting. Both Turner and Hampton have several times visited the Preston Laboratories in Pennsylvania, however; I have been on the same stage with Dr. Turner, with Hampton absent, and travelled much with Hampton, with Turner absent; and no doubt the two of them have had many experiences together, with myself absent. But it has taken more than thirty years to get the three of us into the same room, and I suppose I ought to thank you for achieving that feat.

Let us now praise famous men and the fathers of glass technology.

Two years ago, at a meeting in Columbus, Ohio, of the Glass Division of the American Ceramic Society, Professor Turner said, "During the war, we became aware that at every turn we were confronted by a lack of knowledge of the mechanical properties of glass, and I wish there were many more people working on the subject."

Now the only mechanical property of glass of which it can fairly be said that we are entirely ignorant is the mechanical strength thereof. We cannot say that there is any obvious difficulty in measuring Young's Modulus or assigning a definite value to it, though even that is less easy than most people believe. The same is true of Poisson's Ratio or any other elastic constant. But when it comes to strength it is another matter entirely. In fact, it is difficult to see if the term has any meaning. In the steel industry it does appear to have a meaning, and to have a useful meaning. You can use it to design a structure. But it is not easy to apply the same sort of thinking to an article of glass and design it as you would a steel article.

At school we are taught to design steel structures on the assumption that they have to stay within the elastic limit. They don't actually do so,

* F. W. Preston, "The Inventor of Glass Technology," *Glass Ind.*, 1946, 27, 498.

but we pretend they do. If they didn't yield locally in a non-elastic manner, we should have trouble. That is why we use "mild" steel, and eschew in general the stronger, but more brittle, alloy steels.

But this reminds me of that other famous man, the late William Taylor, previously mentioned, who thirty years ago, at the time of World War I, said to me: "The schools tell you what to expect below the elastic limit, but nothing of interest happens below the elastic limit. Here is a piece of mild steel. Unless I exceed the elastic limit, I cannot drill a hole in it or turn it on a lathe. I cannot cut a thread on it or saw a piece off it. I cannot forge it, swage it or cold-work it. I cannot make it bigger or smaller or change its shape. It is good for nothing except to throw at the cat."

This impressed me considerably with the importance of exceeding the elastic limit. But if you exceed the elastic limit with glass, all you have left is cullet.

Some twenty years ago I was talking with the late L. T. Sherwood, chief technologist of the Pennsylvania Wire Glass Company. He said: "Broken glass is a *disgusting* subject. Let's talk about something else." Sherwood, incidentally, was one of the most far-seeing and original thinkers in this field of glass technology, and the first of my acquaintance to realise how very important the static fatigue of glass can be.

I was obliged to agree with him. In fact, I agreed with both William Taylor and Tom Sherwood. As long as glass was unbroken, it was of mighty little interest. Most glass articles are very commonplace, boresome, banal and of no consequence. So William Taylor was right. On the other hand, after the glass is broken it is just cullet. Nobody wants it, nobody loves it. It is henceforth good for nothing but to be cast into the furnace and remelted; and you are lucky if you don't get cut in the process.

Now between the time a glass article is whole and unbroken and the time it is broken into smithereens, only a few millionths of a second elapse, as a rule. And it is in those microseconds while the glass is actually in the process of breaking that it is really interesting.

That glass should break if you hit it hard enough seems to us perfectly natural and very ordinary. We have seen it happen so often that it never occurs to us to question it or see anything peculiar about it. But as soon as we do question it, everything becomes completely mysterious. It is worse than that enigma-wrapped oriental power of which Mr. Churchill spoke, and it leaves us in a worse predicament than the centipede in the poem. In fact, I think we understand slightly more about what happens when a plutonium atom splits, or an atomic bomb goes off, than we do about what happens when a piece of glass cracks.

I suppose many of you are interested primarily in the bottle-glass industry, and that you would probably subscribe to the dictum of your American treasurer, Francis Flint, some twenty years ago: "It wouldn't hurt us to know more about our bottles." That was a masterly understatement, because since then, knowing more about our bottles has paid very handsome dividends indeed.

The use that is expected of a bottle may not always be self-evident. I used to think that a beer-bottle was intended to hold beer. If pressed, I might have added that it should keep the beer in and keep ultra-violet light out. But George E. Howard, the inventor of the Howard Feeder, told me I was wrong. "That," he said, "is a secondary and a tertiary objective. The primary use of beer-bottles is as a hammer, to bang on the counter, to call for another bottle of beer. Therefore they should be made heavy."

I am beginning to think that this is a very practical viewpoint. At any rate I have begun to think that the real question with bottles is not whether they are sound bottles to hold their contents, but whether they can stand the ever-increasing abuse of the bottlers' high-speed filling lines, or the mischances of transportation by rail or road or sea.

Now the requirements here are sometimes quite different from what you might expect, and you can learn a lot more from the bottles that have broken than by a profound study, according to preconceived notions, of the bottles as you make them.

It is well known that in practice our glass articles do not exhibit the strength that is theoretically inherent in them. It is also well known that this deficiency of strength seems to reside in the surface layers. I quote again a famous man, Dr. J. T. Littleton of Corning Glass Works: "Glass, properly made, is strong—tremendously strong; it is only its surface that is weak." Unfortunately a glass article devoid of surface is not imaginable, and the utility of glass articles from bottles to 200-inch telescope mirrors lies entirely in the fact that they present a surface to something or other.

Now it has been very generally argued that if the defects are in the surface, the smaller the surface the fewer the defects, and therefore in minute surfaces, or minute areas of surface, we might reach or approximate the theoretical strength. Now the theoretical strength is a thing you calculate, and you have to make a certain number of assumptions, and on what assumptions you make depend what values you get.

At one time I had hoped to add Nelson W. Taylor, then at State College, Pennsylvania, to our staff. Mr. Hitler's misdemeanours prevented this, but Taylor has continued to take an interest in our work on the strength of glass. He came to see me once in my hotel in New York, to outline his then forthcoming paper on the strength of glass. I said, "No theory of strength appeals to me as reasonably complete unless it accounts for the limiting cracking-velocity of glass." Taylor replied: "I can account for that too, but it does not form an integral part of the present theory: it seems to be a separate matter. But here is the argument. The velocity of sound in the glass is given by $V_s = \sqrt{E/\rho}$, where E is Young's Modulus, having the physical dimensions of a stress, and ρ is the density." "I can see the rest of the story," I said. "I might as well finish," he replied. "We may

Fig. 1.
J. M. McCormick.

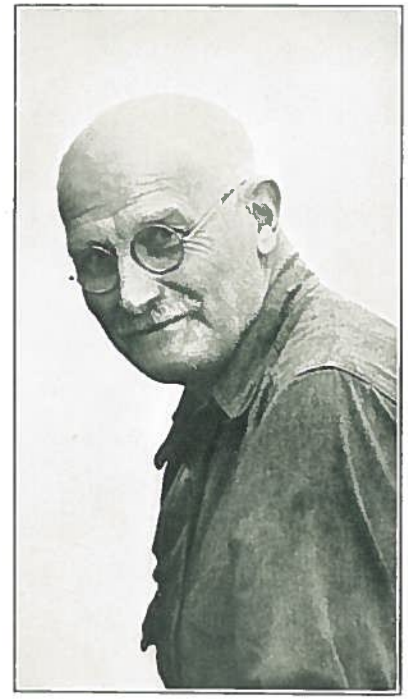

Fig. 2.
The late James Currie.

Fig. 3.
R. G. Hunter.

Past and Present Members of the Staff of the Preston Laboratories.

[*To face News*, p. 182.

FIG. 4.
H. E. Powell.

FIG. 5.
T. C. Baker.

FIG. 6.
L. G. Ghering.

FIG. 7.
Bernard Vonnegut.

FIG. 8.
J. L. Glathart.

Members of the Staff of the Preston Laboratories.

FIG. 9.
H. M. Dimmick.

FIG. 10.
R. E. Mould.

FIG. 11.
M. A. Knight.

FIG. 12.
John Turk.

FIG. 13.
Guy Clark.

Members of the Staff of the Preston Laboratories.

Fig. 14.
Entrance Sign.

Fig. 15.—*"Old Jack" Feeding the Peacock.*

Fig. 16.—*"Old Lab." from the Entrance Drive.*

Figs. 14—16.—*Scenes and Buildings in the Laboratories' Grounds.*

Fig. 17.
The Machine Shop (Sherwood Memorial Building).

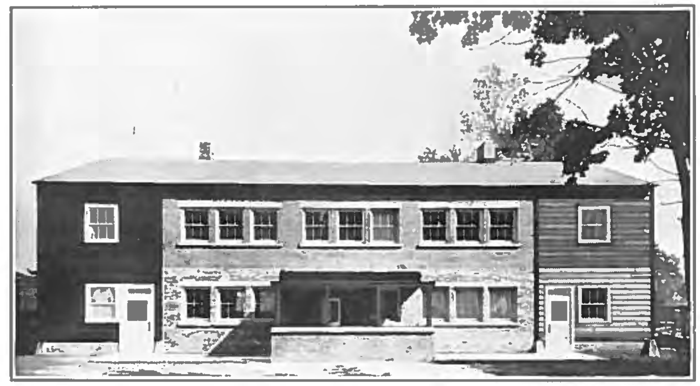

Fig. 18.
The Forrest Building.

Fig. 19.
McCormick's Dam.

Figs. 17—19.—*Scenes and Buildings in the Laboratories' Grounds.*

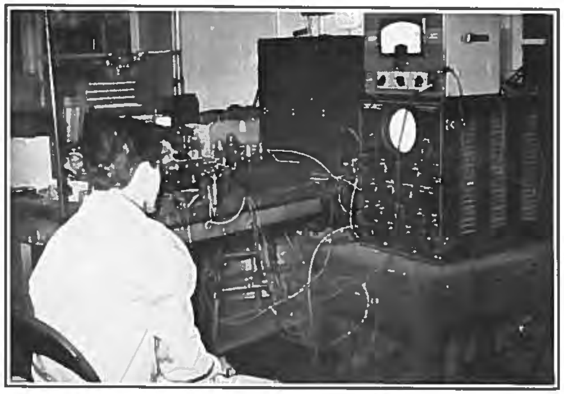

Fig. 20.
Microbeams.

Fig. 21.
Crossing Loops under the Microscope. Pauline Hemphill.

Fig. 22.
Hydrostatic Machine Operated by Mary Gamperle.

Fig. 23.
Museum of Broken Bottles.

Illustrations of Experimental Work in Progress in the Preston Laboratories.

Fig. 24.
Mousehole.

Fig. 25.
Internally Scratched Bottle.

Fig. 26.
Development of Structural Glass Plant.

Fig. 28.
Ghering Tries a Dangerous Experiment.

Fig. 27.
Density Comparator.

Illustrations of Experimental Work in Progress in the Preston Laboratories.

FIG. 29.
Cowbird Laying her Egg in the Nest of a Red-eyed Vireo.
(Photograph by Hal Harrison.)

FIG. 30.
"His Hownde and his Hawke and his Ladye Fayre."
Dr. and Mrs. Preston.

therefore expect that the velocity of crack propagation is given by $V_c = \sqrt{f/\rho}$, where f is the limiting tensile stress the glass will stand, since a crack is a response to a tensile stress. Now no one has succeeded convincingly in finding strength above about a million pounds per square inch, while Young's Modulus is generally about 10 million. It follows that the velocity of sound is roughly $\sqrt{10}$, or say 3 times, the cracking velocity, and this is what we find in practice."

At this moment it seems quite possible that Nelson Taylor has hit upon a good, if not too rigorous, argument, and that the point might be worth checking; and up to date we have nothing to the contrary. So far as our present knowledge goes, the limiting strength of one kind of glass used for making fibres should be a little over 800,000 lb./sq. in., and in practice we have not been able to find fibres with greater strength than this.

Now obviously if Taylor's theory of the relationship between cracking velocity and cracking stress is sound, we have to measure cracking velocities. This was first done by H. Schardin and W. Struth in Germany,* and shortly thereafter by F. E. Barstow and H. E. Edgerton † at Massachusetts Institute of Technology. Edgerton's work was started at the suggestion of myself, and initially financed through Dr. J. C. Hostetter by several American firms, but he finished it on his own initiative. Some years later (1947) H. M. Dimmick, Fig. 9, at the Preston Laboratories showed that the cracking velocity was the same at the temperature of solid CO_2 ($-78{\cdot}5°$ C.) as at room temperature, which is strikingly different from the values of technical breaking stress, as determined by B. Vonnegut ‡ (Fig. 7). At the Laboratory, Vonnegut found that strength was much higher at the lower temperature, but Dimmick found no increase in cracking velocity. If Taylor's hypothesis is correct, the true strength is therefore the same at both temperatures, but at the lower temperature we realise a larger fraction of it, though still only a small fraction.

In the light of the work of T. C. Baker and B. Vonnegut, Dimmick's results seem to mean that the initiation of a crack is typically a chemical phenomenon—a reaction with the water of the atmosphere adsorbed on the glass surface—but the *propagation* of a crack is a *physical* phenomenon, or, if chemical, then very different chemistry is involved in it. We hope to be able to enlarge on this in a more detailed scientific paper before long.

Let us, however, leave these rather difficult problems awhile, and consider the superficially simpler problems of making serviceable glassware with strengths far below the theoretical. Perhaps we can do this conveniently, and briefly, by making a quick tour of the Laboratories, and seeing some of the past and present workers there, what they are trying

* H. Schardin and W. Struth, "Breaking of Glass," *Glastech. Ber.*, 1938, **16**, 219.

† F. E. Barstow and H. E. Edgerton, "Glass Fracture Velocity," *J. Amer. Ceram. Soc.*, 1939, **22**, 302; "Further Studies," *J. Amer. Ceram. Soc.*, 1941, **24**, 131.

‡ B. Vonnegut and J. L. Glathart, "Effect of Temperature on Strength and Fatigue of Glass Rods," *J. Appl. Phys.*, 1946, **17**, 1082.

to do, what apparatus they are using and why they are working on those particular matters.

At the entrance from the public road there is a sign, sandblasted in black glass by the Vitrolite Division of Libbey–Owens–Ford, indicating the way-in by means of the arrow formed by the V-formation in which geese fly. It indicates also that a visit to the Preston Laboratories may very well prove to be a wild-goose chase.

As we enter the grounds, the machine shop, Fig. 17, is on the left; it serves also at times as a pilot plant, and interesting experiments may frequently be found going on in there. And nearby you may see another sign, sandblasted in red glass, and rather different from Fig. 14 in method of execution. It is a giant tortoise from the Seychelles Islands, with the caption " Slow." This is intended to warn motorists not to go charging about our grounds and run over our peacocks, but it also serves to advertise the pace at which we work.

I suppose I ought to show you one of the peacocks, Fig. 15. Here he is with " Old Jack," our late caretaker. They were great friends, and the peacock used to insist on attention by flying up to his bedroom window and rapping on the glass.

On the approach to the old laboratory building, Fig. 16, we may find experiments going on out of doors. For instance, we may find that Dr. L. G. Ghering and Mr. K. H. Parikh are testing the decomposition of bleaching fluid in the ultra-violet of sunlight on the lawn at the base of a flagpole. They are collecting the gases given off, which are quite different in amount according to the colour of the glass used. Sometimes you may find them decomposing beer by the same process, in sunlight, even with snow on the ground. The Laboratory mascot, my dog Kip, may be supervising operations.

Some twenty or thirty yards in front of the Laboratory building we have two signs of the zodiac, Sagittarius and Taurus. Originally we intended to have twelve, but by a freak accident we got these two first, and the combination caused such uproarious laughter that we decided we did not need the other ten. The combination bears the legend " Shooting the Bull," which is American slang for delivering a super-sales-talk, or for ridiculous boasting. We tell our visitors that that is how we spend our time.

Beyond the signs, we come to a circle of lawn which we call the geography lesson. If you wish to know where the Laboratory is located, these signs will help. It shows you how far we are from the North Pole, and hence gives the latitude. Other signs indicate the distance to Capetown, Zanzibar and Tokyo. So that makes it easy to tell where you are.

Still farther over we come to the barn, which houses inactive apparatus, and also snow-ploughing machinery, tractors, lawn-mowers and other things; and beyond that again is the Frank Forrest Memorial Building, which housed a lot of our war-time activities, Fig. 18. A little to the east is a small pond that we call McCormick's Dam, Fig. 19, which is often

visited by wild ducks and other water-fowl, though they prefer our larger sheet of water, which we call the Carrie Dam, or Loch Carrie, in memory of our beloved Scottish master-mechanic, the late James Carrie * (Fig. 2).

Let us for a few moments go inside and consider testing procedures. Once upon a time there was considerable talk of testing glass " as glass," that is, finding out about its mechanical properties as a substance, and not as a piece of glassware. The properties of the glassware, it was thought, should be deducible from the properties of the substance, combined with a knowledge of geometrical considerations. Thus, in a somewhat analogous field, we may say that the optical properties of a lens, or of a lens system, can be described in terms of the optical properties of the glass as a substance, and of the geometrical properties defined by the radii of curvature of the surfaces, and the positioning of those surfaces. Therefore it did not seem unreasonable to think of understanding the mechanical properties of a bottle in terms of the mechanical properties of the glass as a substance, and of the geometry or " architecture " (to use the term preferred by Urban Bowes) of the bottle.

Up to date this approach has not been very fruitful as applied to problems of mechanical strength, because what we are normally concerned with is not the strength of glass as a substance, but the weaknesses of surface abrasions or structural surface imperfections of some sort or other. We cannot see the man for the clothes : the man is known to be a remarkable creature of high integrity, but his clothes are shabby, torn and ragged. For most purposes, however, the man must be interviewed with his clothes on, for clothes maketh the man, and the glassware must be taken along with its damaged surface. It would, none the less, be interesting to try to strip away these adventitious garments and study the underlying material. It is not easy, but we can try.

We have a standard type of engineering testing machine, the Riehle. We can use this for making tension, compression or cross-bending tests. It is, however, more useful for testing rather complex structures, such as light-weight " sandwich " structures intended for aeroplane components, than for finding out about the fundamental properties of glass. It was used by H. E. Powell, Fig. 4, in some of his investigations.†

Most of you will know Powell best by his work on microstrength,‡ although his confidential work on aeroplane structures may have been equally important, and his work on the structure of glass surfaces as elucidated by the electron microscope may shortly prove important.

This method, as we have seen, has its limitations, and so here we see another microstrength device, Fig. 20. It involves the cross-bending of a minute portion of a glass fibre which constitutes a microbeam.

* F. W. Preston, " One Extra Tooth," *Glass Ind.*, 1942, **23**, 258.

† F. W. Preston, " Significance of New Data on Combinations of Plastic and Glass Fibers," *Glass Ind.*, 1944, **25**, 266.

‡ H. E. Powell and F. W. Preston, " Microstrength of Glass," *J. Amer. Ceram. Soc.*, 1945, **28**, 145.

The fibre about the diameter of a human hair is placed across two "knife edges," literally knife edges this time, for they are pieces of safety-razor blades, separated by a few thousandths of an inch. The load is applied through a third piece of razor blade. In the figure the operator is watching the deflection of the tiny beam under a microscope. The load was originally applied relatively slowly by a chainomatic device, fed at a constant speed by a small electric motor, but the apparatus has now been modified to conform more nearly with the earlier work of T. C. Baker, Fig. 5, whose work on the fatigue of glass * and on hydrodynamic breakage † is still constantly quoted. The load is now applied electrically through a solenoid, and the test can be completed in a hundredth of a second or less. In a thousandth of a second, or not much more, the stress can be raised from negligible values to 800,000 lb./sq. in. or more if needed. It is held constant for a fraction of a second, and then as suddenly removed. At these speeds it is not possible to observe deflections in the microscope, but it is possible to get an electrical record of them from the screen of an oscilloscope. With this apparatus then, not only do we test an almost inconceivably small surface area of glass, much smaller than has been tested by any such means before, but we also test it for an extremely brief duration, and yet under "static" and not under "impact" conditions. In this way we hope to approximate the Taylor stress.

These microbeams have another useful feature. Whereas in testing bottles and other sizeable articles, we often accumulate some tons of cullet in a few weeks, the boys figure that it will take them between 500 and 1000 years to accumulate one pound of cullet in the form of broken microbeams.

There is another method of testing fibres that was used by A. A. Griffith ‡ thirty years ago. It consists in forming the fibre into a loop, and tightening the loop down to smaller and smaller dimensions till the fibre breaks. The form taken by the loop is the "elastica," § and is independent of most outside circumstances. Knowing the fibre diameter, the loop diameter and the value of Young's Modulus for the glass in the fibre form, the breaking stress can be calculated. This method has been used by others, for instance, by William Otto of the Owens–Corning Fiberglas Corporation, in a somewhat modified form, and was elaborated by John McCormick, Fig. 1, of the Preston Laboratories so that loops could be formed by an automatic device using microscopic fibres, much finer than a human hair, and the loops, immersed in oil, are tightened on a

* T. C. Baker and F. W. Preston, "Fatigue of Glass under Static Loads," *J. Appl. Phys.*, 1946, **17**, 189; "Wide Range Static Strength Testing Apparatus," *J. Appl. Phys.*, 1946, **17**, 162.

† T. C. Baker, "Water Hammer Breakage in Glass Containers," *Glass Ind.*, 1941, **22**, 430, 469, 485, 521, 534.

‡ A. A. Griffith, "Rupture and Flow in Solids," *Phil. Trans. Roy. Soc. (London)*, A, 1920, **221**, 163.

§ A. E. H. Love, "The Problem of the Elastica," *Theory of Elasticity*, 1944. Dover Publications, New York, p. 401.

microslide and watched in the microscope, Fig. 21. The fibres are so fine that once or twice the girl has mounted two of them simultaneously, thinking she had but one. Note that it is not necessary to measure any applied force; only Young's Modulus and the geometry are needed to arrive at the stress. By this method we can test fibres of smaller diameter than with the microbeam, up to date at least, but we cannot yet test quite so short a length of specimen. A serious difficulty, with fibres as fine as this, is to determine the fibre diameter with any high order of accuracy; that problem has not yet been licked.

Now I am not sure that a glass fibre represents glass, "as glass," any better than other test objects. It is of geometrically simple form, though its cross-section may not be accurately circular, and it is small. But its "constitution" is extraordinarily variable, and not anything like the constitution of massive glass. Some of these facts are known, but most of them are still in process of investigation. A lot of the work is falling on the shoulders of William Otto above mentioned, and on those of Richard Mould, Fig. 10, of the Preston Laboratories.

We must pass on from fibres to other things; for fascinating as fibres may be, they cannot give us the whole story of glass, and, after all, what we are interested in most of the time is the strength of glass articles. The most fascinating of all glass articles, to some people, is a bottle. Let us therefore talk briefly about the mechanical properties of bottles. Fig. 6 is a photograph of Dr. L. G. Ghering, head of our Glass Technology section, and Fig. 23 shows his museum of bottles, most of them broken, illustrating what can happen to bottles in a great variety of circumstances. The analysis of fracture patterns has been the subject of a number of scientific papers by myself, and the practical applications of the underlying theory and the experimental study of the results has been a large part of Dr. Ghering's life-work.

One of our tests is carried out by a thermal-shock machine which gets the bottles hot in hot water, then transfers them, with a given time-lag, and immerses them in cold water. This is a recognised standard test, required, in some cases, by American law courts, and it has some merit, but its findings have to be understood in combination with other tests.

Fig. 22 shows a hydrostatic internal-pressure testing machine. This test is a very good one, and is by no means without merit even when applied to bottles that are not required to withstand internal pressure in service. This also is a standard American Society for Testing Materials (A.S.T.M.) test method, and the test is sometimes required by the law courts.

However, it requires very great internal pressure to break a typical bottle if hydrostatic pressures are used. Curiously enough, with certain products, the bottle is in much greater danger of breaking from internal pressure if the internal pressure is very low than if it is very high. Some products are packed under vacuum, frequently for the purpose of preventing oxidation. Such vacuum-packed products, when suddenly jolted, may deliver hydrodynamic blows against the inside of the bottle, and will sometimes knock out a small piece of the side-wall of the bottle, leaving a "mouse-hole," Fig. 24. Sometimes the breakage takes other forms. The classic work on the subject is that of T. C. Baker, above mentioned, when he was with the Hartford-Empire Company, after leaving the Preston Laboratories. The subject has also been investigated to some considerable extent by R. E. Mould and J. M. McCormick at the Laboratories.*

Hydrodynamic breakage is facilitated if the outside of the bottle is locally weakened by external abrasions; but the converse, weakening of the bottle against external impact by internal abrasions, is even more serious. In Fig. 25 is a bottle internally abraded, and it is as brittle as a carrot. Such scratches may be produced by worn brushes † in a commercial bottle-washing machine. Without the abrasions it would take a sledge hammer to smash it; with the abrasions, the most trifling impact is likely to break it. Drs. R. G. Hunter (Fig. 3), Ghering, McCormick and Baker have all contributed to our understanding of this matter. So has Dr. J. C. Turnbull.

We have recently developed a special pendulum-type of impact-testing machine. With this and other similar machines Henry Dimmick and Jay Last have for several years past been investigating in much detail the factors that affect impact strength. Some very striking conclusions have emerged, which will be the subject of scientific papers in the near future.

We have another impact machine which drops a succession of steel balls upon a bottle while the latter is rotating around and also when travelling endwise. The impact points on the bottle are therefore arranged in a close spiral, and practically all parts of the bottle get tested. This has proved useful for some sorts of work, but no single impact machine seems to fulfil all requirements.

Another impact tester drops a steel ball, by electro-magnetic release, from a great height, while the impacted object is supported on a very massive cast-iron V-block. This was found useful for testing the impact resistance of glass tumblers. It was used by Dr. J. L. Glathart, Fig. 8, in his work on the Service Life of Glassware.‡ Unfortunately I do not have a picture of his "dummy-restaurant," and the little waitresses running in and out with trays of tumblers. These are not by any means the only tests, or the only pieces of testing apparatus we have applied to bottles or to tumblers; somebody is always getting new ideas about testing.

Mr. U. E. Bowes, when Director of Research of the Owens–Illinois Glass Company, said, "The objective of the glass-manufacturer should be to provide the maximum of food packaging for each dollar of the customers'

* R. E. Mould and J. M. McCormick, "Practical Aspects of Hydrodynamic Breakage of Bottles," *Glass Ind.*, 1949, **30**, 381.

† L. G. Ghering and J. C. Turnbull, "Scratching of Glass by Metals," *Bull. Amer. Ceram. Soc.*, 1940, **19**, 290.

‡ J. L. Glathart and F. W. Preston, "Theory of the Behavior of Glassware in Service," *J. Amer. Ceram. Soc.*, 1948, **31**, 153 and 331.

money." It seemed a worth-while ambition, and he certainly contributed his share of progress to that end. But latterly all of us, I think, have come to the conclusion that even that high ambition is not enough. The glass container is not an end in itself. We have to deliver, as a rule, a paper-box container, inside which are a number of glass containers or bottles, inside which is food, medicine or industrial substances like paint or bleach or oil, to the ultimate consumer. It has become increasingly clear that the bottle is only part of the story, that it cannot be considered as something detached from the paper box or the filling lines of the bottler, and we have to study its serviceability beyond the point where it leaves the glass plant.

It accordingly became necessary to find out something about paper, paperboard and paper boxes. The man in charge of this department is Dr. John G. Turk, Fig. 12, an outstanding paper technologist. He and his staff work in a somewhat elaborately air-conditioned laboratory, where the temperature must be held 24 hours a day, and all the year round, at $72° \pm 2°$ F., and the humidity must be held to $50 \pm 2\%$. We have not found any commercial air-conditioning that can assure so close a control, and have had to develop improvements of our own.

Most of Turk's pieces of apparatus are more or less standard types of equipment. However, new and much more powerful instruments of research are apparently on their way in our own Laboratories, and we may have more to say about them some other time. He is equipped to make boxes to all sorts of designs, of all sorts of paperboard. Then he can take them, air-condition them, and give them all the tests he wishes. Paper, we find, is just as hard to understand as is glass. Therefore he has as second-in-command a physicist, Guy B. Clark, Jr., Fig. 13.

Although Turk and his staff spend a large part of their lives in a pleasant, if monotonous, artificial climate, they are by no means as cloistered as this might indicate. They undertake at times, with the assistance of a considerable number of men from interested firms, a very detailed survey of the contents of freight-cars arriving, with all sorts of glass-packed products, in big industrial centres from New York or Philadelphia to New Orleans or San Francisco. This work sometimes has to be carried out in extremely unpleasant climatic conditions, but it has taught us a great deal about practical requirements for the safe transport of our products. It also takes a great deal of time in the "field," and thereafter involves a great deal of statistical analysis. We have been fortunate in having been able to borrow, from the Hazel-Atlas Glass Company, R. F. Ferguson to take charge of the statistical analysis, and some of the field work, in the two principal car-surveys to date. Powell was in overall charge of the first one (1947), and Turk of the second (1948). This work was paralleled, or complemented, by the extensive laboratory researches of Dr. Julian Toulouse of the Owens–Illinois Glass Company at Toledo.

We shall not here go into the problems of the testing of flat glass, whether drawn window glass or polished plate, whether heat-treated, wire-reinforced or laminated. However, Fig. 26 shows a machine under development for making new forms of structural glass.

In the field of refractories I may refer to Turnbull's work on the physics of upward drilling.* This subject is broader than the field of refractories. It may be regarded as a problem in pure physics, but may very well have a bearing on geology also.

In Fig. 27 we illustrate a singularly useful little device known vernacularly as " the Sink-Float," or officially as a " Density Comparator." This device, vastly more sensitive and much more rapid than chemical analysis, has largely replaced the latter for controlling the uniformity of glass discharged from tank furnaces. It is, in great measure, an outcome of the work of Dr. M. A. Knight, Fig. 11, on cords. Knight's work on this subject has proved too voluminous to publish, incorporating as it does some of the work of Turnbull on the subject, and some of Ghering's. A summarising report has been circulated in mimeograph form.†

As we go outside the Laboratories again, we may take another look at Dr. Ghering, and perhaps find him looking as he does in Fig. 28. Here in heavy clothes and a fencing-mask he conducts some experiment, perhaps on seriously over-charged bottles or on bottles internally scratched. Our trial lawyer, B. W. Hendrickson, is amused at this attire, but takes a keen and continuous interest in Ghering's researches and their outcome. For many years he has insisted that the industry must get all the technical facts that can be got, and that lawsuits involving glassware must be tried on the basis of what the technical evidence shows; and unlike so many lawyers, who deal only with " testimony," that is, with what other people say, Mr. Hendrickson is not infrequently in the factories or the Preston Laboratories to see the first-hand evidence, that is, how the glassware itself behaves.

There was a time when R. T. Norris and his assistant might have been seen measuring the " strength-to-weight " ratio of the stem of a peacock's train-feather, or of his wing feather, or the still more remarkable strength in the wing-bone of a sparrow. Nature has some very solid achievements to her credit, in both the animal and vegetable world, in this field; in fact, the struggle for existence has often revolved around this problem. We did not make much of a dent in it, I suppose, and Norris is likely to be known best by his work on the cowbirds of Preston Frith,‡ work which has met with some acclaim and is perhaps comparable, in a mild way, with Edgar Chance's remarkable work, twenty-five years earlier, on the English cuckoo. The cowbird is not a cuckoo, but it *is* parasitic and has some remarkable habits, which we are only now beginning to understand. In Fig. 29 we have a picture by Hal Harrison and Norris, taken at earliest dawn, as the cowbird lays her egg in the nest of a red-eyed vireo. A number of outstanding pictures of similar nature studies have been taken by Norris and his collaborator, Hal Harrison, on the Laboratory grounds or in the immediate neighbourhood. They constitute a remarkable achievement, as the events thus recorded had hardly even been witnessed before.

* F. W. Preston and J. C. Turnbull, " Physics of Upward Drilling," *Amer. J. Sci.*, 1941, **239**, 92.

† M. A. Knight, " Problem of Cords in Glass," *Report to Testing Procedures Committee of Glass Container Manufacturers Institute*, 1947; see also L. G. Ghering and M. A. Knight, " Properties and Diagnosis of Cords," *J. Amer. Ceram. Soc.*, 1944, **27**, 260.

‡ R. T. Norris, " The Cowbirds of Preston Frith," *Wilson Bulletin*, 1947, **59**, 83.

Not far to the north of us, where the hemlock-trees grow thicker, the porcupine roams the woods. His gnawing abilities are phenomenal. He will eat your boots, your aluminium pots and pans or outboard-motor, the handles of your oars, or your barn in its entirety—not the contents simply, but the whole structure. He will eat the fallen horny antlers of the deer, and will come on to your porch to get those of your trophies. But what most people did not know is that he will also eat glass bottles. We have a catsup bottle which a porcupine has gnawed at more than one place, but the spectacular thing is a hole gnawed completely through the side of a bottle.* This and a number of glass bottles and fragments gnawed by porcupines were obtained for me by John Hopkins of Clarendon, Pennsylvania, when I was marooned in the mountains one spring, trapping beavers, while the great flood of St. Patrick's Day went through Pittsburgh and washed away the Allegheny bridges.

The Laboratory grounds are fairly extensive, comprising almost a hundred acres inside a nine-foot fence. This spacious area permits us to carry out experiments involving conflagrations and explosions without alarming our neighbours. For instance, it has been important to know as accurately as possible what risks are involved in storing explosive and inflammable liquids or pastes in glass bottles or tin cans, if there is any risk of their becoming overheated or otherwise subjected to fire-hazards.

Out in the centre of our arboretum therefore you may come upon a strange-looking shack, and Dr. Ghering or some of his staff with cameras and thermometers testing these materials. Such experiments have been useful to the authorities in New York City and elsewhere in drawing up their safety codes.

I may add that amusing results sometimes follow, and that I get credit for explosions that I do not originate. A spectacular one occurred on a snowy night during the war, and although I shortly tracked it down to a mischance of another firm's workman a mile or two away, and proved the point, most of my neighbours still believe I did it; and when in 1946 I went to Bikini to attend the atomic bomb A test, they considered their point proved.

The large area inside the fence and its remoteness from the city or seriously built-up areas ensure us relative freedom from smoke, dust, vibration and electrical interference which we did not enjoy at our former location twenty years ago. It has also permitted us to make some

* F. W. Preston, " Porcupines Gnaw Bottles," *Journal of Mammalogy*, 1948, **29**, 72.

biological and ecological studies.* We have, for example, trapped great numbers of moths in our light traps, seen many species of birds, including some rarities not before observed in our area, and obtained quantitative data for mathematical investigations. However, our outdoor interests usually take us farther afield. One of our extra-curricular activities was the tracking down and reconstruction of its flight through the heavens of fragments of the famous Chicora meteorite, which fell in Butler County on June 24th, 1938.† That was a triumph for Ghering, McCormick and Baker, who did most of the field work. Jobs of this sort, which cannot be tackled by routine procedures, help materially to sharpen the wits of the staff.

The same thing is true of our researches in the field of local geology, especially the history of the ice-age in Central Western Pennsylvania and South-eastern Ohio,‡ and of our wider ecological studies.§

Reference has been made to Fig. 15, which shows one of our peacocks; but we have other pets. For instance, there was my wife's little falcon that used to sit on her fingers and eat raw meat, carving it carefully from between thumb and fingers till never a trace remained, and never grazing her skin. Then there is Tiny Tim, the flying squirrel, who, likely enough, is reposing in my wife's pocket as she works in the office, or if not, he is probably asleep in my secretary's pocket. Then there is my own hawk, a red-tail, much larger than my wife's, but not nearly so well behaved or intelligent. My secretaries and most of the staff are a little afraid of him, but my wife can push him around freely enough. He likes to fight with me, and for months he had a standing feud with my dog, which was often very funny.

Finally, Fig. 30 is a group portrait of most of my immediate family, my hownde and my hawke and my ladye fayre, all of them completely unconcerned about the breaking strength of glassware.

August 25th, 1949.

* F. W. Preston and R. T. Norris, "Nesting Heights of Birds," *Ecology*, 1947, **28**, 241.

F. W. Preston, "Cowbird and Cuckoo," *Ecology*, 1948, **29**, 115.

F. W. Preston and J. M. McCormick, "The Eyesight of the Bluebird," *Wilson Bulletin*, 1948, **60**, 120.

† F. W. Preston, E. P. Henderson, and J. R. Randolph, "The Chicora Meteorite," *Proc. of U.S. National Museum*, 1941, **90**, 387.

‡ F. W. Preston, "The Glacial Foreland," *Ruffed Grouse* (*J. Audubon Soc. of Western Pennsylvania*), 1948, No. 2, p. 6.

§ F. W. Preston, "The Commonness, and Rarity, of Species," *Ecology*, 1948, **29**, 254.

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