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CHARACTERISATION OF BEATING THROUGH STUDY OF THE VISCO-ELASTIC PROPERTIES OF PAPER

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PART 1

A strong sulphite pulp was beaten in different laboratory beaters to a series of freenesses. Handsheets were formed according to the British standard method. In this method, the sheets are clamped during drying and, thus, shrinkage in the plane of the paper is prevented. Stress/strain curves were recorded using the apparatus developed by the Steenberg group.

It was found that sheets from pulp beaten to different freenesses in one and the same beater yielded stress/strain curves of approximately the same *shape* within the range of freenesses examined (Canadian standard freeness 600 — 250 ml). The shape of the curves, however, differed from one type of beater to another.

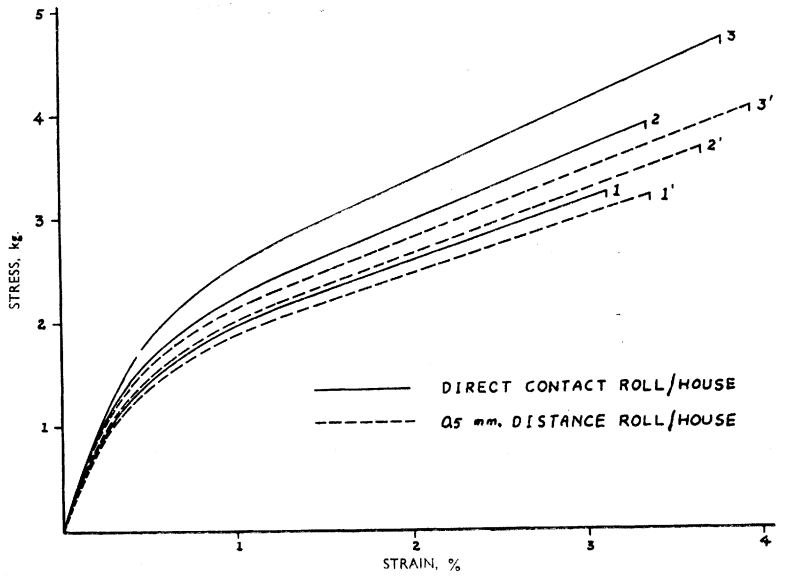
Fig. 1 shows a family of stress/strain curves for paper from pulp beaten in a PFI mill. Fully drawn curves represent runs made with direct contact between roll and housing, dotted curves are from runs with $\frac{1}{2}$ mm. distance between roll and housing. The numbers 1, 2, and 3 designate Canadian freenesses of 600, 500 and 350 ml., respectively.

The curves show that there is a difference between direct contact beating and distance beating. Moreover, both tenacity and extensibility increase with increased degree of beating; however, the curves have approximately the same *shape* — that is, the stress ratio between any two curves is approximately constant throughout the whole range of elongations.

In order to obtain a better picture of the curve shape, the curves were *normalised*, that is, they were all adjusted to go through one and the same empirically chosen point of normalisation near the point of rupture.

The result of the normalisation of PFI mill curves is shown in Fig. 2, upper curve. The lower curve represents normalised curves from a series of runs made with the same pulp to various degrees of beating in a Valley beater. As may be seen from the figure, the difference between the average PFI normalised curve and the average Valley beater normalised curve is considerably larger than the deviations within beating apparatus of single curves from average shape.

Fig. 1



The normalised ordinate at 3 — 4 per cent. elongation was chosen as a numerical value that could serve as a shape *parameter*. The shape parameter was calculated for each single stress/strain curve and the resulting material was treated statistically. An analysis of variance proved that the difference between the shape parameters of the PFI mill and the Valley beater was significant at the 99.9 per cent. level.

The logical following up of the above-mentioned results was to see whether the difference in shape of the stress/strain curves could be related to differences in the beaten pulps, also whether the difference in shape of the

stress/strain curves could be related to differences in other properties of the handsheets.

PART 2

The effect of fibre length on the shape of stress/strain curves was investigated. Used in this part of the work was a PFI mill, a Valley beater, also an old, small L. & W. beater of the Valley type in which a rather severe cutting effect was expected and obtained. Pulp was beaten in each of the three apparatuses to approximately the same breaking strength and in sufficient quantities to allow handsheets to be made from each fraction after fractionation in a Bauer-McNett fibre classifier. The distribution of fibre lengths was measured by means of a microscope. The pulp beaten in the PFI mill had the largest average fibre length, the L. & W. - beaten pulp the smallest average length.

Fig. 3 shows normalised curves for paper from whole (unfractionated) pulp beaten in the three apparatuses. The PFI pulp, having the longest fibres, produced a paper whose normalised stress/strain curve was located between those of the two other beaters. Thus, the curve shape is no simple function of fibre length. This was rather to be expected. Probably, the surface condition of the fibres is one of the main factors determining the degree and pattern of fibre-to-fibre bond forming and different beaters may well be visualised to give the fibre surface different degrees of bonding activity. Furthermore, the surface treatment of the fibres may or may not be accompanied by a shortening of the fibres.

Fibre length and its distribution may, however, *contribute* to determining the shape of stress/strain curves. In order to clarify the effect of fibre length upon curve shape, stress/strain curves were recorded for paper from the fractions of pulp collected in the four compartments of the classifier. In addition, some sheets were made from a mixture of the four fractions and stress/strain curves were recorded. As the quantity of each fraction in the mixture was determined by a fractionation analysis, the mixture should be equal to the whole pulp minus fines.

The curves shown in Fig. 4 are taken from experiments made with the PFI mill. The fully drawn curves represent the normalised stress/strain curves from the four fractions of the beaten pulp, the dotted ones show the corresponding curves from the unfractionated pulp and the pulp mixture. It will be seen that *the shape parameter increases with decreasing fibre length.* The curves from the two other beaters, also the curves from later experiments with a Lampén mill, displayed essentially the same picture of increasing

Fig. 2

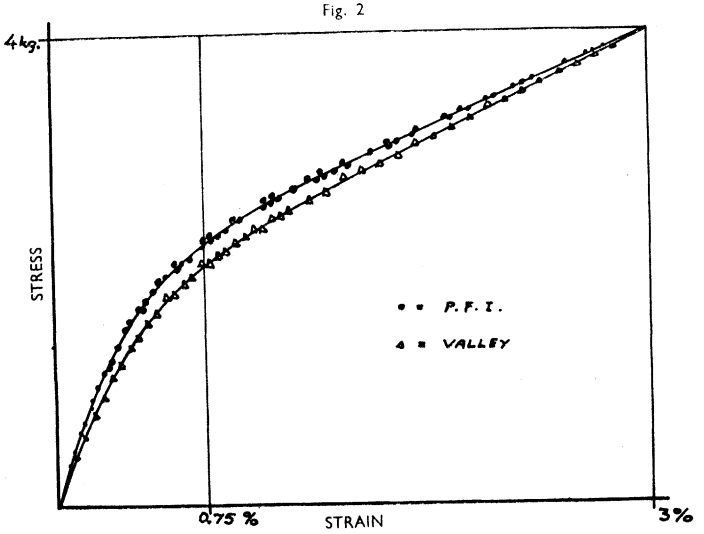
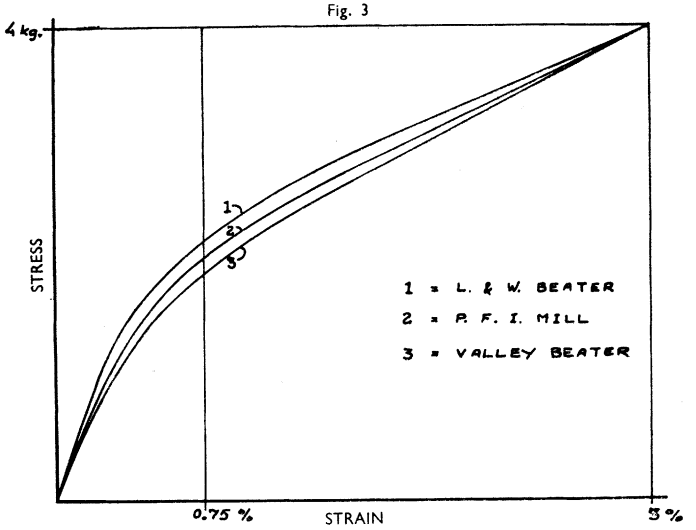


Fig. 3



shape parameter with decreasing fibre length, the only difference being of a quantitative nature.

As for the unfractionated pulp and the pulp mixture (dotted curves in Fig. 4), two effects should be noted. In the first place, a removal of fines produces only a minor change in the shape of the normalised stress/strain

Fig. 4

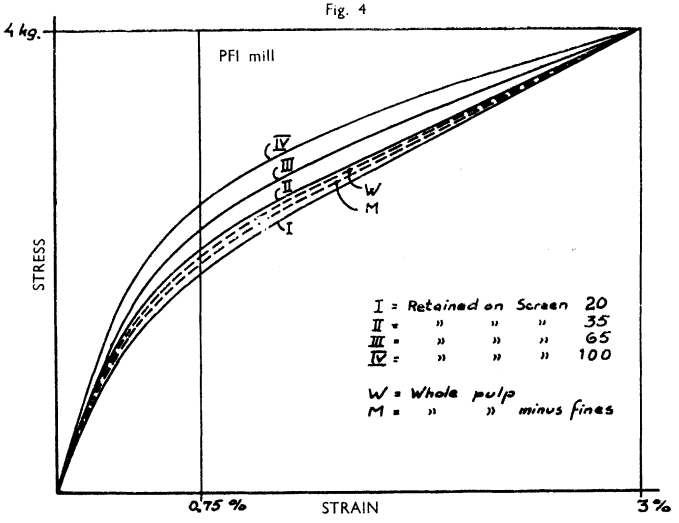
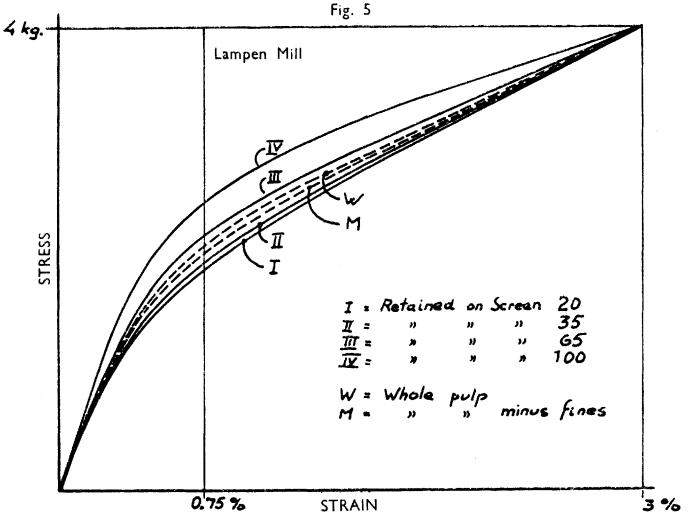


Fig. 5



curve. Secondly, both curves are located between the curves of fractions I and II (the two long-fibred fractions). Beating experiments with the Valley beater and the L. & W. beater gave concurring results. This seemed to indicate that the shape of a stress/strain curve is quite strongly influenced by the longer fibres in the pulp; however, experiments with a Lampén mill gave

a somewhat different result, which will be described separately (Part 3 of this paper).

Going back to Fig. 3, it was shown that fibre length alone cannot account for the differences in shape of the stress/strain curves from unfractionated pulp beaten in the three apparatuses examined. Knowing the influence of fibre length upon curve shape, it might be said that the shape parameter of the PFI curve has too high a value relative to the parameter of the Valley beater curve. It was suggested that the surface condition of the fibres is of importance to the degree and pattern of fibre-to-fibre bond formation, which in turn may influence the visco-elastic properties of the paper.

Now, we have several indications that the PFI mill makes the fibre surface more *active* than do the two other beaters examined —

1. The PFI unfractionated pulp and pulp mixture produce paper with the same apparent specific volume, whereas for the two other beaters the pulp mixture produces paper with a higher apparent specific volume than that of paper from unfractionated pulp.
2. The breaking strengths of paper from PFI pulp mixture and from PFI fractions I and II are as high as the breaking strength of paper from PFI unfractionated pulp, with the corresponding strengths of paper from the two other beaters at somewhat less than 90 per cent. (for Valley beater) or 80 per cent. (for L. & W. beater) of the strength of unfractionated pulp.
3. The PFI mill gives paper with a specific light scattering coefficient that is significantly lower than that of paper from pulp beaten in the two other apparatuses.

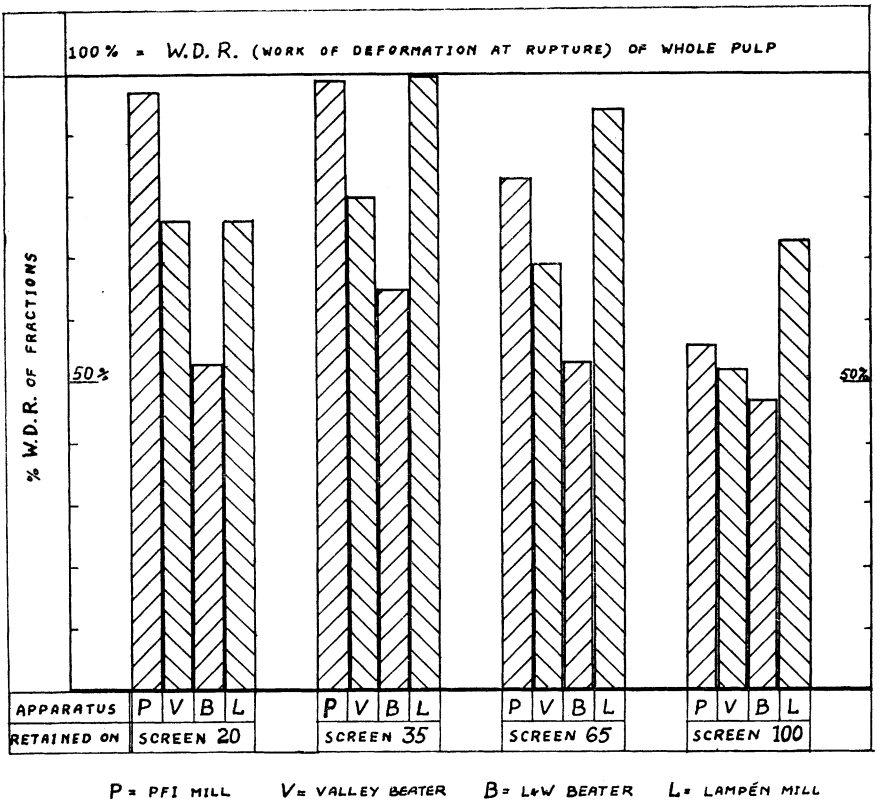
Thus, it looks as if the PFI pulp, having a more active fibre surface, gives a greater degree of cross-linking between fibres, an effect that in turn may be expected to reduce the effect of fibre length upon curve shape.

PART 3

In the experiments made with a PFI mill and with two beaters of the Valley type, it was shown (Part 2) that the shape parameter increases with decreasing fibre length. In addition, the curves from unfractionated pulp and from a mixture of the pulp fractions were located between those from fractions *I and II* (the two long-fibred fractions).

Fig. 5 shows the result of similar experiments made with a Lampén mill. Even for this mill, the shape parameter increases with decreasing fibre length. (Although the difference between fractions I and II is quite small, it was found to be statistically significant.) In this case, however, the curves from the unfractionated pulp and from the mixture of fractions were located between the curves from fractions *II and III.*

Fig. 6.



One of the reasons for this seemingly anomalous result may be that the Lampén-beaten pulp contains more short-fibred material than do the pulps from the other three beaters. Based on fractionation analysis, the ratio (by weight) of the two long-fibred fractions to the two short-fibred ones has been calculated —

Equation	*Values*
$R_Q = \frac{\text{I} + \text{II}}{\text{III} + \text{IV}}$	PFI = 2.03 Valley = 1.67 L. & W. = 1.44 Lampén = 1.27

Thus, if the *quantity* of short-fibred material in the pulp has any influence on the visco-elastic behaviour of paper from the unfractionated pulp — an

assumption that might not be too farfetched — the location of the normalised stress/strain curves from the Lampén-beaten pulp is at least partly explained.

Another factor possibly influencing the shape of stress/strain curves may be the contribution of each fraction to the *strength* of the composite pulp.

Fig. 6 shows the strength (total energy at rupture) of each fraction in percentages of the strength of unfractionated pulp. The short-fibred fractions of the Lampén-beaten pulp is seen to have a relatively high strength. The strength ratio of fractions I and II to fractions III and IV has been calculated–

Equation	*Values*	
$R_w = \frac{\text{I} + \text{II}}{\text{III} + \text{IV}}$	PFI	= 1.41
	Valley	= 1.29
	L. & W.	= 1.18
	Lampén	= 1.05

Thus, both the relatively large amount of short-fibred material in the Lampén pulp and the relatively high strength of this material may explain the results obtained in the experiments made with the Lampén mill.

Having determined experimentally the shape parameter of the normalised stress/strain curves of paper from a mixture of the four pulp fractions, it would be of interest to compare this value with a parameter calculated from the parameters of the components. Such calculations have been made by weighing the shape parameter of each fraction with both quantity and strength of the fraction. The ratio of the calculated shape parameter of the composite paper to the experimentally determined one was found to be as follows —

Equation	*Values*	
$R_{S.P.\ mixture} = \frac{S.P.\ calc.}{S.P.\ exp.}$	PFI	= 1.06
	Valley	= 1.05
	L. & W.	= 1.04
	Lampén	= 1.03

It is seen that the calculated shape parameters agree quite well with the experimentally determined ones, the deviations being in the order of 3 – 6 per cent. Furthermore, the parameter ratio of the Lampén mill does not differ appreciably from the ratios of the other three apparatuses. This seems to indicate that the above-mentioned theories on the influence of quantity and strength of the fractions upon the shape of stress/strain curves are valid.

Apparent specific volume of fractions I and II was found to be at least as high for paper from the Lampén mill as for paper from the other three

beaters, while Lampén mill fractions III and IV produced paper with lower apparent specific volume than did the corresponding fractions from the other beaters. For Lampén mill fractions, the specific light scattering coefficient *decreased* with decreasing fibre length, whereas for the other three beaters the scattering coefficient *increased* with decreasing fibre length. These findings are in accordance with the earlier mentioned findings concerning the relative strengths of fractions. In short, we get a picture of the Lampén mill as a beating apparatus in which quite an extensive shortening of the fibres occurs in such a way that the fibre fragments are left with a large bond-forming capability. Therefore, the fibre fragments in a Lampén-beaten pulp have a relatively strong influence on the pulp as a whole and on its papermaking properties.

DISCUSSION

MR. P. G. SUSSMAN: I have no contribution to make about the fundamental significance of bonds. I would only like to report on a small experiment that we carried out on the tensile strength testing of paper and that has some bearing on the stress distribution in paper.

Prof. Steenberg has had a few things to say in his paper about the usefulness of the normal tensile tester. Our experiment was carried out with an ordinary Schopper tensile tester. The distance between the jaws was 9 cm. and we varied the widths of the strips by 2 mm. steps from 2 mm. to 16 mm.

When the widths of the paper strips approached the width of the weak spots, we expected to see a large drop in tensile ratio, but always found a straight line within the accuracy of the experiment.

We tried this with hand-made paper, made in the old-fashioned way (which had a very even look-through) and with sack kraft paper (which had a very poor look-through). The result rather surprised us and we have not yet thought of a convincing explanation. These are some suggestions, of course.

There is another point in Prof. Steenberg's paper that I hope will be commented on later in the discussion — that is, the carrying out of tensile tests at high speed. Now, the speed of a multi-wall sack on hitting the ground is something like 5 m./sec., when dropped by an average size man. I wonder really whether one should extend this test to such high speeds that the inertia of the strip becomes important. I was hoping that Prof. Brecht might comment on that, as he has carried out a great number of tests on the ordinary load/stretch properties in relation to the strength of the multi-wall sack.

PROF. B. STEENBERG: May I answer this by asking another question? Have you studied how the breaks in the strip are distributed along the length of the strip at equal strip width? Since not, I will give some information about it. Dr. J. Kubát, in my laboratory, has studied this distribution. The breaks are not uniformly distributed over the lengths of the strip, although the average breaking strength is the same independent of the breaking point position.

Thus, if the chance of a break a few centimetres from the lower clamp is, say, three times that in the middle of the strip, the average breaking length is the same. The stress distribution in the sample is important and every single tester and type of paper tested may have to be considered.

Thus, the basic theory of weak link distribution can probably not be studied by Mr. Sussman's method.

MR. L. G. COTTRALL: I am not going to tell you anything new. I am encouraged by the Chairman's remark that it is not essential to bring forward something very new to this discussion and the subject I am raising now has some bearing on the matters under consideration at this symposium. Prof. Brecht has touched upon this subject in his paper, but I should like to go into it a little further, because I feel that it has a bearing on the subject of bonding, which has been talked about quite a lot in the past few days and will probably be talked about considerably more later in this symposium.

I am additionally constrained to do this by one passage in Prof. Brecht's paper, when he says, "It is mainly the work of Campbell and Stamm that has contributed to an explanation of these observations." He is referring, by the latter, to the very small increased sorption shown by beaten pulp compared with unbeaten pulp. In fact, Campbell and his co-workers, L. M. Pidgeon and J. K. Russell, were really the first by very elegant, refined

TABLE 1

SORPTION OF UNBLEACHED SULPHITE PULP, BEATEN AND UNBEATEN

Canadian standard freeness — before beating 670, *after beating* 54; *temperature* 20°C

	Relative vapour pressure, %	*Sorption*, %		*Ratio of beaten to unbeaten*
		Unbeaten	*Beaten*	
First desorption	94.7	31.3	33.6	1.07
	83.6	19.5	20.2	1.03
	69.0	12.8	13.0	1.02
	60.9	11.1	—	—
	39.8	7.2	7.2	1.00
	25.2	5.6	5.7	1.02
	15.0	—	4.2	—
	0.0	0.0	0.0	—
		Average ratio		1.03
First adsorption	15.1	3.2	3.5	1.09
	25.2	4.4	4.5	1.02
	41.0	5.9	6.3	1.07
	60.8	8.9	9.1	1.02
	69.0	10.2	10.5	1.03
	83.7	14.1	14.5	1.03
	100.0 (wet)	38.6	47.2	—
		Average ratio		1.04

and precise experimental technique to make reliable determinations of the small extra bonding of water to the fibre substance brought about by beating and they published their experimental results long before the workers referred to by Prof. Brecht.

Campbell and Pidgeon published their work in 1930 (Campbell, W. B. and Pidgeon, L. M., Canadian Pulp & Paper Association, Technical Section, Sixteenth Annual Meeting: *Papers and Proceedings*, 1929/1930) and Campbell and Russell in 1931 (Campbell, W. B. and Russell, J. K., *Quarterly Review*, Forest Products Laboratory of Canada, No. 5, Part I, 41 – 64) — that is to say, before Seborg, Simmons and Baird in 1936, long before Wiedner in 1939 and Korn and Burgstaller in 1953, the authors quoted by Prof. Brecht.

Moreover, Campbell and his co-workers, unlike the work reported by Korn and Burgstaller in Fig. 6 of Prof. Brecht's paper, carried out both absorption and desorption determinations. They carried them out not merely at one stage of relative vapour pressure, but also at several points between 17 and 95 per cent. and were therefore able to plot more or less complete curves over the whole range of relative vapour pressures, thus enabling data of a high degree of accuracy to be attained.

TABLE 2

SORPTION OF KRAFT PULP, BEATEN AND UNBEATEN

Canadian standard freeness — before beating 720, *after beating* 80; *temperature* 20°C

	Relative vapour pressure, %	*Sorption*, %		*Ratio of beaten to unbeaten*
		Unbeaten	*Beaten*	
First desorption	96.0	31.6	34.0	1.07
	70.1	13.3	13.8	1.04
	62.3	11.8	11.8	1.00
	41.2	8.0	8.3	1.04
	27.2	6.2	6.6	1.06
	17.7	4.7	4.9	1.04
	0.0	0.0	0.0	—
		Average ratio		1.04
First adsorption	15.6	3.7	3.9	1.05
	27.4	4.9	5.0	1.02
	40.2	6.8	6.7	1.02
	61.0	9.1	9.8	1.09
	69.2	11.2	11.5	1.03
	94.9	21.6	22.8	1.05
		Average ratio		1.04

Campbell and his co-workers submitted wet samples of unbeaten and beaten pulps to atmospheres of relative humidities at constant temperature under carefully controlled and precise conditions; the equilibrium moisture contents of samples under these conditions were measured, so providing drying or desorption isothermal curves. They then reversed the procedure and submitted the same samples to decreasing steps of relative humidity and measured the equilibrium moisture content at each of these steps. When plotted, the absorption curve was, of course, some few per cent. below the desorption curve, owing to the well-known hysteresis effect, but the important point is that both the desorption curves and the absorption curves were practically identical for both beaten and unbeaten pulps.

To give you some idea of the results of this work, I would like to show you sets of graphs from Campbell's and Russell's work. Fig. L shows the desorption and absorption curves for beaten and unbeaten sulphite pulp. Both sets of points are practically coincident: it is difficult to distinguish between the beaten and the unbeaten. I have therefore shown the actual figures, the figures of absorption for the unbeaten being in column 2 and for beaten in column 3. Column 4 shows the ratio of the beaten to the unbeaten absorption. You will see that the highest percentage increase is 7 per cent.

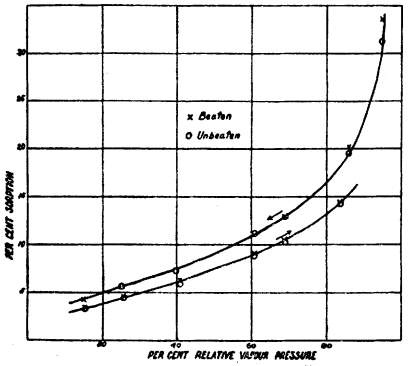


Fig. L

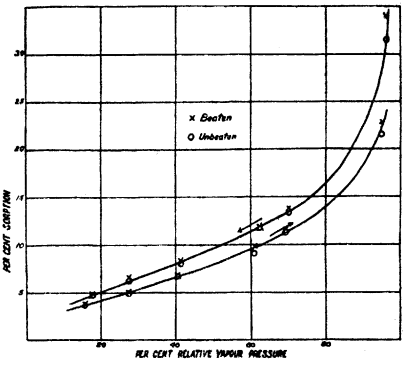


Fig. M

Fig. M shows the same thing for unbleached kraft pulp. You see the same thing here: as before, looking at the *absorption* curves, the additional amount of moisture taken up by the beaten pulp is only a very small percentage of that taken up by the unbeaten pulp.

Now these pulps were beaten to a very considerable extent, 54° C.S.F. for sulphite and 80° C.S.F. for kraft, which means that these pulps were

beaten to the greaseproof or almost greaseproof stage. Yet they only showed a difference of a few per cent. in water held at any particular vapour pressure. If more water were held by beaten pulp, whether it be bonded by hydrogen bonds or absorbed physically or even capillary held, the vapour pressure in the case of beaten pulp would be lower at any particular moisture content than in the case of unbeaten pulp. The fact that from the foregoing data it is apparent that at the same moisture content there is very little difference in vapour pressure between beaten and unbeaten pulp shows that the extra bonding with water in the case of the beaten pulp is very small. That is the point I really wish to drive home, that the extra rupturing of the lateral bonds by beating and the attachment of water molecules to the bonds thus released is extremely limited in extent compared with the bonds ruptured by the mere soaking of the pulp in water — it is only some few per cent. of the latter — and it does serve to show tha the very extensive effects of beating involve only a relatively minute proportion of the total bonds existing in the fibre and only a very small proportion of the actual bonds concerned when the fibre is soaked in water.

I am rather stressing this point, because the view seems to be prevalent in some quarters that considerable gel formation of the fibre substance occurs in beating, which, if it did occur and were even merely confined to the surface of the fibre, would increase the amount of bonded water to a much greater amount than Campbell's data would permit.

That brings forward another point, which I do not want to labour here, because it rather anticipates Dr. Van den Akker's paper tomorrow, but it does show that the beating process only exerts a relatively small mechanical effect on the paper. If we are only breaking a few bonds by beating, then the vast amount of power expended in beating must be used in other and apparently useless ways. I think that is confirmed by the fact that, with all our theories of beating that have been evolved in the last few years, we have not really touched on the most efficient way in which we can beat the fibre. We are still using the machinery invented many years ago. The Hollander beater was invented about 300 years ago. Next year is the centenary of the Jordan refiner and even the new adaptations of these refiners do not alter the principle by which such beating apparatus works. I feel that the fact that only a few of the bonds of the fibre are affected by the beater does show that we are using the beating process rather wastefully.

DR. S. G. MASON: Further to Mr. Cottrall's remarks, I should like to record that a collaborator with Campbell was Dr. Otto Maass (now retired)

who is in this room and to whom, I think, are due a few of the ideas elaborated in the papers cited by Mr. Cottrall.

MR. C. R. G. MAYNARD: Mr. Arlov showed a number of normalised stress/strain curves for the different fibre fractions. He did all his work on plate-dried sheets, sheets that were not free to shrink. Because this is a recognised standard procedure, I think many people use plate-dried sheets. It is rather a pity, because, as Prof. Brecht pointed out in his paper, shrinkage is most important and shrinkage does occur on the papermachine to a less or greater extent.

I think that one of the causes of this difference in shape of the standardised curves was not the difference due to fibre length, which Mr. Arlov was trying to show, but a difference in shrinkage potential. Fraction 4 will shrink during drying much more than fraction 1 and, if shrinkage is prevented, a far greater stress is generated in the sheet.

Such high stresses during drying will produce a stress/strain curve for fraction 4 that looks rather like a work-hardened curve for fraction 1. This shrinkage stress generated during drying may account for the difference in shape of the normalised stress/strain curves and the difference may not be due to the fibre length as such.

MR. J. A. S. NEWMAN: I should like to discuss the experimental method that Prof. Brecht used in the laboratory to simulate machine conditions in the light of the results usually found on a Fourdrinier machine, also in the light of some of our own experimental work.

On a Fourdrinier machine, the paper is first of all extended while going through the press section; during the drying section, it is sometimes allowed to shrink, sometimes held at a constant length and sometimes extended. This depends on the setting of the draws, but such is the power behind the machinery driving those different sections that, no matter what the paper might try to do, the extension is constant for any wetness or any amount of shrinkage potential. It is always extended or allowed to shrink by this one particular amount; whatever it tries to do will not alter that.

Therefore, the experimental set-up we used was to have two clamps, between which we put the strip, the separation of the clamp being preset to allow the strip to shrink by a certain amount and no more during its drying, however much it might want to.

In the cross-direction of a sheet on the papermachine, there is no extension whatsoever at any time during the run of the paper from the wire to the reeler. There is, furthermore, no restraint to shrinkage until that

shrinkage actually starts to occur. The restraint arises from the frictional forces between the cylinder and the paper and between the paper and the felt on top. To all intents and purposes, those frictional forces, again, are constant for any type of paper, whatever the wetness. They do change slightly, but in the main they are constant and, to simulate the cross-web condition, we therefore used two vertical clamps, between which the paper was put, the lower clamp also supporting a weight that was initially on a table, so that the strip was under no tension until it tried to shrink.

The results obtained from this — first of all from simulation of the machine-direction — were as follows. By plotting the permitted shrinkage against the expansion when wetted, the graphs for different wetnesses were as in Fig. N. In other words, for any given permitted shrinkage, there was very little increase in water expansion for a very large change in wetness. This is, I think, what might be expected from the only slight increase in water take-up of the wetter beaten sheets and is also what one finds on a paper-machine in the machine-direction. An increase in wetness does not have any great effect on the expansion.

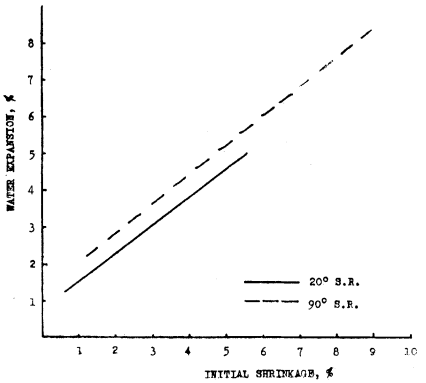


Fig. N

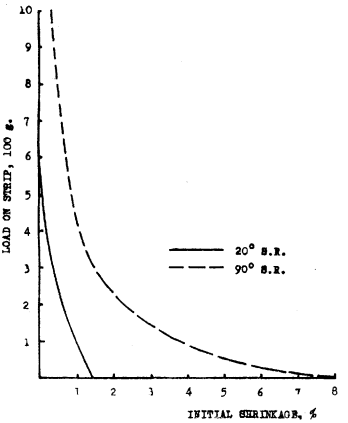


Fig. P

So far as the cross-direction sheets were concerned, we started plotting the shrinkage against the load applied to the bottom of the strip and curves were obtained as in Fig. P. Any one set of machine conditions where friction is constant is simulated by one particular load hanging on the strip. The strips thus shrink by very different amounts and, by transferring these shrinkages to Fig. N, we can see how much they would then be capable of

expanding. It can be seen that the increase in the water expansion in the cross-direction is very much greater, because of the increase in wetness in the cross-direction, than it is in the machine-direction. This confirms normal machine experience that the anisotropy of the paper is increased by increasing the wetness.

MR. A. P. ARLOV: Mr. Maynard raised the very important question of how to dry your handsheets. I should like to stress that I deliberately chose to use plate-dried sheets in this investigation. I did this because I knew from earlier experience that, if I permitted the sheets to shrink during drying, the effect I was going to study would become camouflaged by the greater effect of shrinkage upon the stress/strain behaviour of the handsheets.

I had the good fortune to work at the Swedish Forest Products Laboratory with Mr. Brauns and Dr. Ivarsson some five years ago. Working with the experimental papermachine, we examined the effect of draws and felt tension in the dryer section on the stress/strain properties of paper. The stress/strain technique proved to be a very useful method of examining the shrinkage conditions in the dryer section of a papermachine and, in later years, I have repeatedly used this technique for papermachine evaluation work. The idea behind the condensed report given by me at this symposium, however, was to try to extend the use of the stress/strain technique to a study of the beating process and I felt that I was more likely to succeed in this by applying drying conditions as constant as possible.

Glover, Pritchard and Ray have examined stress/strain curves of freely shrunken handsheets from a pulp beaten to various freenesses. They found a maximum in the post-yield slope of the curves at an average degree of freeness. I have done the same thing with plate-dried sheets and the shape parameter (which roughly correlates with the post-yield slope) is almost constant over the range of freenesses examined: however, the shape parameter is different for different beaters. In my opinion, this proves two things. In the first place, by using plate-dried sheets, you can obtain information about the type of beating not available when using freely shrunken sheets. Secondly, the near constancy of the shape parameter over a range of freenesses of pulp beaten in one and the same beater seems to prove that Mr. Maynard's theory on the effect of strain-hardening on the shape of my stress/strain curves is not valid.

MR. R. SÈVE: I wish to make a remark that, I think, may be of great importance on the subject Prof. Brecht was dealing with.

If a piece of paper is placed in a conditioned atmosphere, when the paper reaches its equilibrium, changes in its properties are observed —

for instance, its moisture content, dimensions, curl (especially if it is coated paper), etc. The variations in these properties decrease as the equilibrium comes nearer, then, after some time, stability is observed. It often happens, however, that at that time the humidity of the conditioned atmosphere is constant only from the statistical viewpoint, as discrete fluctuations of null average value occur.

Recently, Prof. L. Neel, the well-known specialist on magnetism, published (*Comptes Rendus*, 1957, **224**, 2240 and 2668) a very interesting study of the importance of these fluctuations in magnetic phenomena. I think that this study can be applied to the paper in the following way.

The hysteresis curve of paper properties (moisture content, dimension, etc.) as a function of the relative humidity has a higher slope at a given point when the humidity increases than when it decreases (Fig. Q). For that reason, from the equilibrium point, humidity fluctuations, equal in absolute value, but of opposed sign, cause property variations, even different in absolute value. In the case of a good number of fluctuations, due to pure accident, there is a cumulative effect that leads always to an increase in the property studied. The first consequence of this fact is that fluctuations in relative humidity disturb the stability of paper properties.

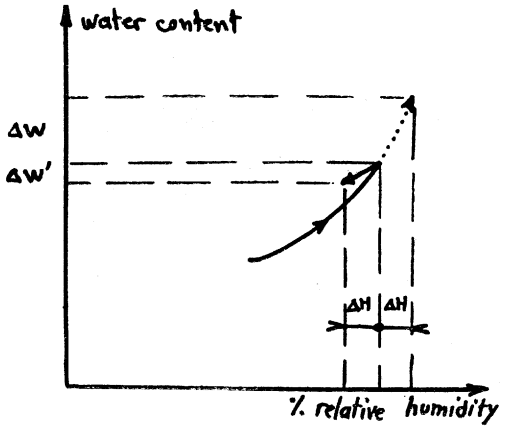


Fig. Q

A second consequence can also be envisaged. It is due to the local irregularities of the paper, causing for two different points of a sheet two hysteresis curves slightly different. When the paper reaches its equilibrium in a conditioned atmosphere, these differences are compensated by strains. If the point representative of the paper draws several hysteresis cycles, according to Prof. Neel's reasoning, there is a shifting of the hysteresis curve. For magnetic phenomena, hysteresis is studied in applying to the material first a magnetic field (+ H), then an opposed field (– H). For papers, it is not possible to change to an opposed humidity; the phenomena are therefore a little different and more complicated.

Some laboratory observations, however, seem to make good this application of magnetism to paper. If this were the case, useful consequences could be derived from the study of the hygrostability of paper.

MR. J. L. GARTSHORE: I should like to comment on one statement in the paper — that "the fines contribute little towards the ultimate strength of the pulp." In our experience, we think that they contribute considerably to its strength.

One difference between our work and that of Stephansen is that we used unbleached sulphate pulp, not unbleached sulphite pulp. The results of one set of experiments we did were published in 1935 (*Proc. Tech. Sect. P.M.A.*, 1935, **16** (1), 119). In this case, we used Lampén mill beaten stuff, fractionated it and, with different proportions of untreated fibre, fibrillated fibre and flour, we produced a synthetic stuff that gave the same laboratory sheet strengths (burst ratio). Stephansen has used breaking length. We varied the proportion of flour from 35 per cent. to 15 per cent. and the untreated fibre from 50 per cent. to 4 per cent., the rest being made up of fibrillated fibre. The freeness figure varied greatly, but the same burst ratio was produced.

A second set of experiments was carried out, still with unbleached sulphate pulp, but using the Aylesford laboratory beater. We fractionated through a 200 mesh screen. In the case of the unbeaten pulp, the removal of 3.7 per cent. of the fines reduced the burst ratio by 12 per cent. In the case of the pulp beaten to 160° C.S.F., the burst ratio of the +200 fraction was 80 per cent. of the original, 14 per cent. of the fines having been removed. The Canadian freeness of this fraction rose to 540°. The -200 fraction (14 per cent.) was made into sheets and produced a burst ratio that was 71 per cent. of that of the original beaten pulp.

We did a number of experiments varying the consistency in the beater, the speed of the cone and the clearance between the bars and the amount of beating after fractionating in all cases, the strength of the +200 fraction was reduced considerably, 25 – 30 per cent.

I wonder if the difference in the findings is due to the method of drying the laboratory sheets. Our sheets were dried between felts on a hot cylinder and were allowed to shrink. They were dried 3 – 4 per cent. moisture content and then conditioned up to equilibrium moisture content. Stephansen's sheets, I believe, were plate-dried.

PROF. STEENBERG: The experiments I have referred to were carried out by Stephansen in Norway. We have seen clearly today that the behaviour of fines from the Lampén mill, so far as ultimate strength properties are

concerned, is different from that of the fines produced in other laboratory beaters. Finally, I do not care for bursting strength as a criterion.

MR. P. E. WRIST: I should like to ask for Prof. Steenberg's interpretation of some interesting mechanical properties that can be imparted to paper by the new Clouet process (described in an American patent of 3-4 years ago). By this process, you can increase the expansivity of paper up to as much as 80 or 90 per cent. of the original length. I have seen examples in which a kraft paper that before treatment had a normal expansion in the machine-direction of 1-1¼ per cent. was increased up to 10 per cent. by the process. The ultimate breaking strength remained the same. Therefore, you have increased by about 600 per cent. the energy required to break it.

In trying to explain the effects from restricting shrinkage, we have talked about built-in strains, but we have in this new paper much larger energies stored in paper obtained by making the paper shrink more than it wanted to during the drying process.

Have you any explanation for these enhanced properties?

PROF. STEENBERG: As a matter of fact, I have no comments, because I have not been able to study this material in detail. I think we can, in essence, compare the case with what occurs in the multi-wall sack crepe papers. It has been known for many years that you can store a considerably larger quantity of energy by the use of crepe paper.

MR. MAYNARD: I have one question that I would like to ask Prof. Brecht concerning the mechanism of the expansion of the paper when it takes up water. He mentioned in his paper that drying tensions diminish the effective shrinkage and thereby the water expansion. When I first entered the paper industry, it was a common explanation that, if the fibres were close together and the sheet were dense, when they expanded in their cross-direction, they pushed one another apart and so the whole sheet expanded. If the sheet were not dense and there were much air, the fibres expanded into the air and the sheet did not expand. This does not seem to hold any longer, because we can make dense sheets by plate-drying them without any shrinkage. If we make plate-dried sheets from stock of high wetness, they have a high density. We can obtain increased density by calendering and by wet pressing. All these three methods increase the density, but do not seem to have very much effect upon the expansion of a paper when it is moistened. The important thing seems to be the shrinkage that the paper has undergone.

I wonder if anyone has any explanation how these pieces of paper seem to remember how much they shrank a year ago when they were made, so that they will know how much to expand today.

PROF. W. BRECHT: In so far as dried-in strains are concerned, I do not think there is any possibility for a paper to lose them unless conditions are appropriate. It does not matter whether the paper has been stored one year, two years or just a few days. I think it has been demonstrated that the paper tends to lose these dried-in strains if it has been allowed to take up moisture. On subsequent cycles of drying and remoistening, the paper is able to shrink to a greater extent.

There are two factors that determine the rate at which paper will come into equilibrium with the humidity in the surrounding air — (*1*) the rate of circulation of the air and (*2*) the density of the paper itself. If the surrounding air is circulated rapidly as it was in our experiments, a more porous sheet will reach equilibrium more quickly and will experience the corresponding expansion (*see* Fig. 20). If the surrounding air is stagnant or circulating only slowly, the denser sheet seems to come to equilibrium more rapidly (*see* Fig. 4). The explanation of this appears to be that the denser paper, because of a higher hemicellulose content, will have a higher equilibrium moisture content and therefore a higher vapour pressure gradient. In short, in rapidly moving air, the relative surface exposed is the determining factor; whereas, in stagnant air, the vapour pressure gradient dominates.

MR. G. F. UNDERHAY: The answer to the problem of the difference in properties between sheets of the same density might be in the way the sheets are formed, in that the dried down sheet has its fibres pulled together by the binding forces developing between the fibres, so that eventually you end up with strong bonds. In the sheet that is calendered, although there is some water present that helps to make the fibres plastic, there is no guarantee that, under the pressure of the calender, you are going to make those same strong bonds that have occurred in the drying of the sheet.

PROF. STEENBERG: May I take up another matter in this connection? It is not sufficient for the barometric pressure to fall to move the aneroid barometer needle; you must tap the barometer. You have the same phenomenon in paper. It is not only a matter of getting the water into the paper, because there may be stiction in the system. I will call it stiction, although I do not know if it is so in a real mechanical sense. Take board, for instance; you can put water rapidly into the board, you can also prove that it occurs as adsorbed water, but the sheets of board might not have changed their

dimensions. Mechanical treatment of the sheets increases the rate of dimensional changes.

I think this proves that there are frozen-in stresses or strains, whatever you like to call them.

In Sweden, a couple of years ago, when a printer's conditioning plant did not have sufficient capacity, I suggested that the paper should be run through an offset press with water. Handling of the sheet would probably do a lot of the business that water alone could not do in a short time. The procedure turned out to be extremely successful.

I think this will illustrate that a paper can remember what happened a long time ago, just as the barometer definitely remembers what the barometric pressure was yesterday. A piece of steel that is hardened will remember for many thousands of years that it was chilled. I think we should not be afraid about the long time factor.

MR. MAYNARD: There is one other point that I should like to take up. I have never personally understood this question of dried-in stresses or strains. In a sheet of paper, free from external force on it, any compressive force in the paper must be partnered by an equal tension force. When the sheet is wetted and dried, it changes in length, most probably contracts. If the tension and compressive forces are equal, why should the tension force win and contract the sheet? Why is the compressive force, which should expand the sheet, of no avail?

DR. J. A. VAN DEN AKKER: May I propose that the dried-in stresses are highly localised? There is no necessity for general force, because (as Mr. Maynard has pointed out) the net force is zero. On a microscopic scale, there can be a compression stress at one point and a tension stress at a closely neighbouring point. Certainly, the individual fibres are held in configurations by other fibres that of themselves alone they would not assume. So I would think that the stresses are highly localised and that their existence is not incompatible with a net force of zero.

PROF. STEENBERG: May I put in one word here? How do you explain the same thing in a piece of glass plate that is too rapidly chilled? Do you agree that there are dried-in stresses there, too and how do you explain them?

MR. MAYNARD: How do *you* explain it? I do not, I am asking you. I have no explanation. I do not say that Dr. Van den Akker is wrong: the forces could be locally distributed. I can appreciate that; but why does the tension force win or the compression force win when they are released? As they exist at the moment, they are equal; they must be.

MR. WRIST: May I suggest to Mr. Maynard that he takes a piece of cane and bends it around then ties the two ends together with string. In that state, the system is balanced and there are no external forces. If he now cuts the string — analogous to breaking a bond — he should stand back, as rapid expansion will occur.

MR. G. F. GLOVER: I think it is not really necessary to suppose that there are frozen-in stresses in a sheet while it is not in tension.

It is possible to visualise that certain fibres and their bonds are left at the moment of drying the paper in such a position and in such a condition that, when the paper is again put in tension, these fibres are ready to take the tension, while the remaining fibres are not. It is not possible to see what is there in the paper, but you can say that the paper knows what to do when you begin to put a load on it.

There is another point — the density of the sheet. There can be said to be a ghost at the party here, for nobody appears very ready to consider what happens in the thickness direction of the paper.

When by any means you bring about a change in the density of the paper, you also change the thickness. Changes in thickness also take place according to the manner in which the paper is dried. If you dry a sheet under tension, shrinkage in the plane is prevented, but there will be a greater shrinkage in the thickness direction. A tension-dried sheet will not expand so much in the plane, when wetted, as will a sheet dried free to shrink, but will expand in thickness.

It is necessary to look at all three dimensions of the sheet, as well as the density to find a more complete story.

There is one other point. I think Mr. Arlov was tending to suggest that he dried his sheets on plates, because he wanted to eliminate shrinkage as a factor. If sheets are dried on plates, some fibre stocks will have a small tendency to shrink and therefore will generate quite a small tension force, whereas other combinations of fibres will tend to shrink more and generate more force if prevented. Hence, the various sets of sheets in an experiment will in fact have been dried under different tensions and one does not know the whole story.

MR. ARLOV: May I comment on the latter point? Pulps beaten to a series of freenesses will certainly have different shrinkage potentials, but, as shown by my first slides, a difference in shrinkage potential seems to have only a very small influence on the shape of the stress/strain curves, provided one and the same beater is used and that the sheets are plate-dried. As I

have said before, however, the shape of the curves varies from one beater to another. Thus, I have in my hands a tool that can be used in a study of the beating action of different beaters. I agree that by eliminating shrinkage you do not learn the whole story about the beaten pulp; on the other hand, by using plate-dried sheets, you can get information about the type of beating not available when using freely shrunken sheets.

DR. VAN DEN AKKER: I should first like to ask Prof. Steenberg about the independence of a test he refers to on the dimensions of the test specimen. I think we all agree that, if we could measure mechanical properties of paper in a way that is independent of the dimensions of the test specimen, we would be much happier, because then we would feel that we were measuring something fundamental. Prof. Steenberg was referring to the interesting work by Andersson on the critical velocity of paper.

I really have three questions. Did you investigate the effect of the width of the test specimen in this work? In general, the paper near the cut edge of a strip is a little weaker than that in the body of the test strip. Accordingly, one would think that the critical velocity would be a function of the width of the test strip. I am sure that what you were referring to was the independence of the critical velocity on the *length* of the strip.

My second question relates to the way in which a quantity that we measure depends upon basic properties. I think that many people consider as anathema the bursting strength test because of its many complications and difficulties, in spite of its great simplicity, as you pointed out this morning. From my point of view, however, I see a similarity between critical velocity and bursting strength. The bursting strength is a function of the tensile strength and the ultimate strain of the paper. On the basis of theory, one can relate bursting strength with the mean tensile strength and the stretch or ultimate strain of the paper in the machine-direction. This is one of the better correlations. It is an old one and many people are familiar with it.

It seems to me that the objection to the bursting strength test is not just the fact that the specimen opening must have a particular diameter, but rather that the test depends upon basic properties in a *peculiar way*. If we were interested in a correlation based on the mathematical product of tensile strength and the square root of the ultimate strain, then this would be a very appropriate thing and we would be interested in improving bursting strength instruments, I am sure. Do we escape this *peculiar* dependence on basic properties when we deal with critical velocity? I might recall that for a Hookian material the critical velocity is the ultimate strain of the material times the velocity of the pulse in the material.

For those of you who have not studied this particular subject for some time, in principle one has an indefinitely long strip of the material and, at the right end, there is a weightless clamp that, at zero time, is given some velocity V. The acceleration is infinite for zero time and the velocity springs up from zero to velocity V. If the velocity V is a very small fraction of the pulse velocity, there will be a very small strain developed in the material. If the critical velocity is attained, then we have the critical strain and the material fails.

The difficulty that I see with the critical velocity concept, in addition to the possible dependence upon the width of the test specimen, lies in this relationship —

$$V = e^* c = e^* \sqrt{E/\rho},$$

in which e^* is the breaking strain, c is the pulse velocity, E is Young's modulus and ρ is the density of the material. Therefore, in the case of the most ideal material to which we could apply the concept of critical velocity, we have the critical velocity equal to the rupture strain multiplied by E raised to the one half power divided by the density to the one half power. I would apply the same criticism to this assemblage of more basic quantities as that already directed against the peculiar collection of quantities for the bursting strength test. Paper is not actually Hookian, but behaves in a way that is highly non-linear, as described by Prof. Steenberg and others, so that the critical velocity is not actually so simply related to basic properties. Nevertheless, the fact remains that, even though a more complicated expression is required, the critical velocity is a *peculiar* function of basic quantities. If we are interested in some use — for research or otherwise — that happens to involve this particular combination of factors, I would say fine! — but how often do we want to combine our basic quantities in this particular way?

This is my third question — Prof. Steenberg has made a valid criticism in pointing out that, in many of our tests, the action depends upon the specimen itself: in measuring critical velocity, what is the difficulty involved in moving the clamp from zero velocity to V ideally in zero time? Is there not some apparatus effect involved in achieving the critical velocity?

PROF. STEENBERG: My co-writer of that paper, Mr. Andersson, is sitting here and I am sure he can give more information than I can.

In the first place, we have not investigated the influence of the width of the sample. You will recall that the critical speed is so high that you have to build a new machine for practically every test carried out. The method is impractical, as pointed out. It would be nice if we had the variables separated

from each other as far as possible. Of course, we will always have an instrument effect, but we should have as little influence by the specimen on the test as possible.

If your approximation formula for the Mullen test were correct, it would prove the Mullen test to be superfluous. It is not applicable for machine-made paper, however: even the formula for the critical rate test is over-simplified, as Dr. Van den Akker is well aware.

The two e^* in your two formulas can, of course, not be the same, because the process depends on the rate of elongation in the Mullen tester. This is unknown, different in different directions in a machine-made paper and entirely dependent on the paper. In the critical rate test, e^* is the rupture strain at a test speed determined by the properties of the material. I think that a comparison of those two e^* is not correct, because they are not identical.

DR. VAN DEN AKKER: I had no intention that they should be.

PROF. STEENBERG: If we are interested in the stress/strain curve and assume that it contains a time factor, we may be interested in the stress/strain curve at very slow rates.

DR. VAN DEN AKKER: There remains the important point — are we interested in the particular combination of basic factors underlying the critical velocity? We should be more interested in some other combination, depending on the requirements.

MR. O. ANDERSSON: I think that no specific combination has any preference. If so, why should not this one have it? We have just been told why it was chosen. On the other hand, I have a feeling that the argument is irrelevant.

PROF. STEENBERG: As a matter of fact, I hope that some day we will not be interested in any combination at all; we will be interested in the elementary basic concepts.

DR. VAN DEN AKKER: I agree with that.

PROF. STEENBERG: The only way to solve it when we have many unknown entities is to have as many equations as we have unknowns, because then we can take each one out; but, if we stick to one thing, we have and will have everything left unsolved.

MR. ANDERSSON: One of the beauties of this critical speed was that it was just one quantity with a particular physical meaning. Any quantity can be expressed in terms of several other quantities.

DR. VAN DEN AKKER: I should like to combine the basic quantities in a way that suits my purpose. The important point here is that we should be able to get at the individual basic quantities, as Prof. Steenberg has just said, so that we can put them together in any way we like.

MR. ANDERSSON: The critical speed is basic enough to me.