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THE STRENGTH AND OTHER PROPERTIES OF MATERIALS OF CONSTRUCTION,

AS DEDUCED FROM STRAIN DIAGRAMS AUTOMATICALLY PRODUCED BY

THE AUTOGRAPHIC RECORDING TESTING MACHINE.

A Paper by Prof. ROBERT H. THURSTON, Member of the Society.

PRESENTED DECEMBER 31ST, 1875.

In a paper read before the Society in February and April, 1874,* the writer gave an account of a series of researches which he had made with a novel form of apparatus, and illustrated the work by *fac-similies* of a collection of automatically produced strain-diagrams. The new method of investigation adopted and the importance of some of the conclusions deduced from the autographic records have attracted much attention and the paper has been extensively republished.† It has recently been translated into the German for Dingler's Polytechnisches Journal, and its publication has been followed by a paper by a distinguished colleague of the writer, Prof. Kick,‡ of the Institute of Technology at Prague, who makes a number of criticisms§ which indicate that it may be advisable to consider some of the more obscure points in the original paper at greater length and to exhibit the sources of the errors which have been committed by the critic.

The first criticism made by Prof. Kick, as will be seen by a perusal of the paper, a translation of which is herewith given,|| is a statement that important discrepancies exist between the results obtained experimentally

* Transactions, Vol. II, page 349; Vol. III, page 1; † Journal of the Franklin Institute, 1874; Van Nostrand's Mag., 1874; Dingler's Polytechnic Journal, etc., etc., 1875; ‡ International Jury, Vienna, 1873 ; § Kritik Uber R. H. Thurston's untersuchen uber festickeit und elasticitat der constructions; Materialen Von Friedrich Kick, Bd. 218, H. 3.

|| CRITICISM OF R. H. THURSTON'S "RESEARCHES ON THE STRENGTH AND ELASTICITY OF THE MATERIALS OF CONSTRUCTION," BY FRIEDRICH KICK.—Translated from Dingler's Polytechnisches Journal ; Band 218, H. 8.

The results of Thurston's investigations of the strength and elasticity of materials, no less than the ingenious deductions therefrom, require the more thorough examination because of the important discrepancics arising between those results and the experiments instituted by myself for the determination of the relations of tensile and compressive forces as influenced by changes of form.

by the author of the criticism and by the writer. This difference is attributed to an assumed peculiarity of the apparatus and of the method of experiment adopted by the writer, which is asserted to produce serious errors. That such a difference does appear between the results obtained by the writer and the critic is undoubtedly the fact; that they are attributable to the cause assigned is less evident, and what follows may prove the assertion entirely unfounded. The critic makes an assumption of faulty manipulation without evidence of its existence and then claims to "prove" mathematically that the apparatus, which is asserted to be "dynamic" in its action, records its results statically and thus introduces fatal errors of record.

The mathematical portion of the paper is correct, and we will take advantage of that fact and will show how far the adverse element—the resistance due to the acceleration of weight—which is so boldly asserted to be the cause of "serious" errors, is likely to introduce such errors.

Taking an extreme case, supposing a perfectly rigid test-piece to be under test, the velocity of motion of the weight would be precisely equal to that of the handle and would be a *maximum.* Actually, the test-piece always yields and the velocity of the weight is invariably less than that of the handle. In the greater number of cases, the weight moves with much less rapidity than the handle, even when moving at its highest rate of speed, and during the greater part of the test, the rate of motion of the weight is so low as to be imperceivable and incapable of measurement, and at other times, the weight actually moves slowly downwards, as is seen by reference to the published strain-diagrams, on which the relative motion of weight and handle can be readily determined.

The motion of the weight is, in fact, independent of that of the handle and varies with the resistance of the test-piece, rising or falling as that resistance increases or diminishes, always slowly and almost invariably

The study of Thurston's treatise indicated that a certain error, sometimes insignificant, sometimes important, was inherent in the results, the source of which was to be found in Thurston's testing machine. This machine, acting dynamcally, records its results as if they were statical, the greater the velocity adopted, the greater is this error. This assertion is proven as follows:

If a weight be suspended by a spring, when equilibrium occurs—whether for rest or motion—the tension of the spring is proportional to the weight. As soon, however, as acceleration takes place, in the case of transition from rest to motion, as well as with varying velocities during motion, the tension on the spring must change; this variation may be expressed by the variation of S in expression

$$S = G + \frac{v\ G}{g\ t};$$

for uniformly accelerated motion, in which S is the tension on the spring; v is the velocity at the end of the time t, and g is the acceleration of gravity=9.808 M.

with very much less velocity than that of the handle. In making tests with this machine, the handle is always moved very slowly, and when attempting to secure diagrams for scientific purposes especial precaution is taken.

The following figures represent the rate of motion of the *handle*, measured alternately for a somewhat rapid and for an ordinarily slow motion. The motion of the weight is, as has been shown, very much slower.

	Time.	Angle.	R. Cos.	Space.	Max. Moment.
(*A*)	1 Min.	16°.00.	47.125 in.	13.54 in.	135.987 ft. lbs.
			1.197 m.	0.362 m.	
(*B*)	2 Min.	37°.66	38.75 in.	32.00 in.	292.755 "
			0.984 m.	0.813 m.	
(*C*)	1 Min.	16°.00	47.125 in.	18.54 in.	135.987 "
			1.197 m.	0.362 m.	
(*D*)	1 Min.	37°.06	39.00 in.	31.70 in.	291.70 "
			0.996 m.	0.805 m.	

Where the effort was made to attain greater rapidity of motion, the following results were obtained:

	Time.	Angle.	R. Cos.	Space.	Max. Moment.
(*E*)	1 Min.	33°.66	40.75 in.	28.78 in.	267.02 ft. lbs.
			1.035 m.	0.761 m.	
(*F*)	1 Min.	47°.66	33.00 in.	40.63 in.	350.98 " "
			0.838 m.	1.032 m.	

Let $t = 1$ second, and let v have the following successive values for the corresponding values of S, and the value of G will be:

v.			*S.*			*G.*		
0.1 M.	0.4	2.0	0.05 M.	0.20	1.00	1.01	1.04	1.20
0.2	0.5	3.0	0.10	0.25	1.50	1.02	1.05	1.30
0.3	1.0	4.0	0.15	0.50	2.00	1.03	1.10	1.40

Thus, even when the increase due to uniform acceleration amounts to but 1 M. per second, the tension will be 20 per cent. greater than in the case of equilibrium.

Let us apply this investigation to the Thurston machine in which tests are made by torsion. By depressing the lever *C*, *B* rises, the axes of both being united by the test-piece lying in the jaws. (For description of the machine, see Transactions, Vol. II, page 350, &c., *Translator.*)

We may liken this apparatus to a balance to which has been added an automatic recording apparatus which records the constantly increasing pressures exerted upon the lever *C*, and which are transmitted to the weighted lever *B* by means of the test-piece. These records can only be based upon statical laws; for Thurston actuates a movable recording-pencil, attached to the weighted arm by a fixed and invariable guide-curve which can only be constructed by reference to the statical moments of the weight *D*.

Neglecting unavoidable sources of error which are present in all automatic recording apparatus, this has one great defect.

Prof. Kick states correctly the resistance due to acceleration of the motion of the weight as equal to $\frac{v\,G}{g\,t}$, and the total amount of stress as

$$S = G + \frac{v\,G}{g\,t} \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(1)$$

in which expression, S = the total stress, v = the acquired velocity at the end of the time t, G=the weight and g = the acceleration of gravity $= 32\frac{1}{6}$ feet $=386$ in. $=9.8$ m.

$$\frac{S}{G} = 1 + \frac{v}{g\,t} \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(2)$$

Then, for the several cases just given, assuming the velocities to be those of the weight, as improperly asserted by Prof. Kick, we get :

$$\text{(A.)}\quad \frac{S}{G} = 1 + \frac{v}{g\,t} = 1 + \frac{13.54 \times 2}{386 \times 60} = 1.001212\,;$$

$$\text{(B.)}\quad \frac{S}{G} = 1 + \frac{32 \times 2}{386 \times 120} = 1.001401\,;$$

$$\text{(C.)}\quad \frac{S}{G} = 1 + \frac{18.54 \times 2}{386 \times 60} = 1.001212\,;$$

$$\text{(D.)}\quad \frac{S}{G} = 1 + \frac{31.70 \times 2}{386 \times 120} = 1.001347\,;$$

And for those cases in which the rate of acceleration was made as great as could be obtained by the exertion of all the strength of the operator :

$$\text{(E.)}\quad \frac{S}{G} = 1 + \frac{28.78 \times 2}{386 \times 60} = 1.002481\,;$$

$$\text{(F.)}\quad \frac{S}{G} = 1 + \frac{40.68 \times 2}{386 \times 60} = 1.006301.$$

It is constructed upon the basis of statical relations. In using the machine, equilibrium does not exist, but motion. The diagrams must therefore deviate the more from the truth, the more suddenly and the more rapidly the lever C is moved. If, therefore, the greatest care is not taken in experimenting, using low velocities and a steady hand, the diagrams will be incorrect and entirely untrustworthy for strictly scientific researches. No proof is required to show that the same force is required to produce the same acceleration, whatever the position of the lever carrying the weight at the beginning of the acceleration.

The moment of the force producing acceleration must be added to, or subtracted from, the moment of the weight D, according to the direction of the motion. Only the latter is graphically recorded—the former is not. The error thus arising would be constant for a uniform acceleration and could be corrected by drawing a line parallel to the curve, were it possible to move the lever C with a uniform acceleration. But, since this cannot be done by hand, such a rectification of the graphical record of the Thurston machine cannot be made.

Further, we have : that the above-given quantity which is to be added or subtracted is relatively of greater influence, the less the torsional moment of the weight D; in other words, the errors of the diagram due to the movement of the weight are of most importance at the initial portion of the diagram, within the elastic limit.

It is possible that the peculiar forms of the diagrams 6, 10 and 85, Plate B (*Plate II, Vol. II, page* 378), which are convex to the axis of abscissas, are a consequence of the more rapid motion of the hand-lever during those experiments, and, as well as the irregularities of the lines of the diagrams, may be partly explained by this dynamic action of the machine.

When, therefore, Thurston says : " (1.) To determine the homogeneousness of the material, examine the form of the initial portion of the diagram between the starting point and the

It is seen from the above that the maximum possible errors, due to the cause assumed by the critic as the source of the discrepancies which he has found to exist between his work and the self-recorded results given by the autographic machine, are necessarily some fraction of one-eighth of one per cent. Every experienced investigator in this department of scientific research knows, however, that this limit of error falls far within the limit of variation of quality of every material of construction, even when nominally of the same grade. The criticism is therefore seen to have no practical weight.

Now, determining the relative motion of handle as given above, and of the weight, from the strain diagrams published, and taking wrought iron as the best illustrative example, it will be seen that, within the elastic limit, the error claimed to destroy the value of the data secured may possibly amount to 0.001, and that at the limit of elasticity even this error entirely disappears, since the weight there ceases rising. Beyond the limit of elasticity, the error is that due to a rise of the weight equal to an exceedingly minute fraction of the motion of the handle, and is so small that it would be quite impossible to detect it on the diagram by any method of measurement in use. The criticism of the distinguished author of this "*kritik*" is thus seen to be quite insufficient to account for the discrepancies noted by him. He is quite right in looking for the source of error in the machine—provided that the results of the writer are erroneous and those of Prof. Kick are right—for, in the former, the

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sudden change of direction which has been shown to indicate the elastic limit. Notice, also, the inclination from the vertical, and compare it with the inclination of the elasticity-line.

" A perfectly straight line, beneath the elastic limit, perfectly parallel with the elasticity-line, shows the metal to be homogeneous as to strain; *i. e.*, to be free from internal strains, such as are produced by irregular and rapid cooling, or by working too cold. Any variation from this line indicates the existence, and measures the amount of, strain" (*Vol. III, page* 2). The first sentence, in consequence of the inherent error in the apparatus, is, in general, incorrect.

An indisputable and convincing proof that Thurston's machine is inapplicable to scientific investigations is found in the diagrams 101 and, particularly, in 118, Plate C (*Plate III, Vol. III, page* 30). The diagrams show, at *b*, *c* and and at *b' c'*, that a rapid increase of velocity is followed by a rapid sinking of the line. This must necessarily occur ; for the pencil of the registering apparatus, in consequence of the peculiarity of this construction, does not record that moment which is exerted in the acceleration of the weight of those parts which are set in motion by the test-piece.

But Thurston deduces from these diagrams a direct reply to the question: what is the relation of the resistance of the test-piece to rapid or slow distortion ? He deduces, from the fall of the line of the diagram with the motion, that the resistance decreases as the velocity of strain increases. We have seen that this assertion is not confirmed by those diagrams. That Kirkaldy has reached the same conclusion may be due to a similar misinterpretation of the results of experiment. If Thurston's diagrams could give a definite answer to the question, they would rather read—the resistance is independent of the velocity of distortion.

Referring to the conclusions deduced by Thurston's incorrect method, it is remarkable to find it stated that : " To determine the resilience of the material within any assumed limit of story is told by the machine itself and cannot be attributed to errors of personal observation as may those existing in data acquired by the older methods of research.

It may be safely asserted that the errors due to the inertia of the weight and to its acceleration may, by careful handling, be made as minute and as practically immeasurable as those due the same cause in the older forms of testing machines. Considering the facts, that the results obtained by the older methods of testing are liable to errors arising from personal observation, while, in the method adopted by the writer in the autographic recording testing-machine they are automatically registered, it would seem that the advantage, in respect to accuracy, must be on the side of the new method.

The writer believes the facts exhibited above to prove conclusively that the bold assertion of the foreign critic—that with the greatest care these strain-diagrams are liable to be incorrect and untrustworthy—is without real basis and is itself absolutely incorrect.

The second criticism of Prof. Kick, in which he suggests this assumed source of error to be the cause of the differences in the initial portions of diagrams (6, 101 and 85 of Plate II, Vol. II., page 378), which the writer attributed to peculiar conditions of molecular or mechanical structure, is not only invalidated by what has been shown above, but most conclusively by a large number of experiments made before and after the date of the original paper, in which the noted peculiarities were very marked, although the experiments were conducted with uniform precaution. The fallacy of the criticism is still further proven by the characteristic differences

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extension, measure the area of the curve up to the assumed limit. To determine the total resilience, or the shock-resisting power, measure the total area of the diagram;" (*Vol. III page* 3).

While, on the other hand, he concludes: "The rapidity of action, in the cases of shock, and where materials sustain live loads, is a very important element in the determination of their resisting power ; not only for the reason given already, but because the more rapidly the metal is ruptured, the less is the resistance to rupture;" (*Vol. III, page* 15). Since the last sentences which contradicts the former is incorrect, the former *may* be correct ; it remains, however still unproven.

Similarly unproven is the assertion : "The effect of repeated bending, or other form of strain, can be thus inferred from an examination of the strain diagram of the material, obtaining from a single experiment a determination hitherto only obtained by a long and tedious process of repeated distortion;" (*Vol. III, page* 8). It is here quietly presupposed that the diagram of resilience up to the point of fracture, for the test-piece strained by torsion, record, also the amount of work done in case of fracture for all other kinds of strain. The proof of this is wanting; it would be difficult to produce it.

[The critic forgets, throughout his paper, that the *quality of the material* is the property which it is attempted to determine, *not* the relations of the several kinds of strain.—R. H. T.]

Thurston mentions the fact that : "The phenomenon here discovered is an elevation of the limit of elasticity by a continued strain;" (*Vol. III, page* 12). This "discovery" has also, noted in the initial portion of the diagram where different metals are compared, as shown in the published diagrams of iron, steel, copper, tin, etc. Such differences could not possibly arise from the assumed cause.

Professor Kick adduces as what he asserts to be "absolute proof" of the existence of the source of error above alluded to, the peculiar strain-diagrams, 101 and 118, Plate III. These show the rapid motion of the handle (not of the weight) to be followed by a fall of the weight and a drop of the pencil. This was attributed by the writer to a weakening of the metal by rapid distortion ; a conclusion which has been confirmed by a study of Kirkaldy's experiments with his tension apparatus, by many experiments since made by the writer with the autographic machine, by numerous experiments made by Com. Beardslee at the Washington Navy Yard with a tension machine having peculiar facilities for exhibiting this phenomenon, and especially by the experiments made on a very large scale on iron beams for targets, as described by Gen. Barnard in a paper read before the Society (Transactions, Vol. I, page 173), and referred to by the writer, in the discussion at the Seventh Annual Convention (Vol. III., page 128).

The error into which the critic has fallen will be seen at once when it is noted that during this rapid motion of the handle and the distortion of the test-piece, produced by a heavy blow on the handle, the weight had no time to move and the drop of the weight succeeded the distortion, as is explicitly stated in the original paper to be an evidence of the weakening which is a consequence of rapid distortion. This evidence would seem to be quite sufficient. But the experiment described by Gen.

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been made and published by General Uchatius (*Die Stahlbronge, Vortrag. Gehatten am* 10 *April*, 1874, *Wien.*) in 1873, in the following words : "In all metals which possess a considerable degree of ductility, it is interesting to extend the investigation to the elastic limit. We thus learn that these metals attain their highest elasticity only when they are strained beyond the elastic limit to a certain point and are allowed to remain so for a time." "This advantage has never, until now, been noted."

Thurston's machine tests specimens for torsion and the extension of the external fibres, calculated from the angle of torsion, cannot be considered as the measure of the extension of the fibres by tensile tests, for two reasons : *First*, because a diminution of length of the torsion test-piece must occur, which is not measurable from the angle of torsion graphically recorded by the machine. *Second*, for the reason that the external fibres are reinforced and strengthened by the internal fibres, in consequence of their lateral cohesion, they being less affected. That elongations of 69 and even 120 per cent. are found in wrought iron can only find its explanation on pages 100 and 101—elongations which no wrought iron will give.

If Thurston finds nothing remarkable in this, but states that this elongation of fibre is proportional to the reduction of section noted with the standard testing-machines, it should be said. on the contrary, that it is entirely inadmissible, for the percentage of elongation to be given any relation to the reduction of cross section. Robert Lane Haswell has noted this already. If we pull apart a test-piece like that shown in the accompanying illustration [The sketch represents a pair of test-pieces, of which one, *a b*, is intact ; the other, *a′ b′*, on the

Barnard, to say nothing of those of Com. Beardslee, are certainly conclusively corroborative.

The exception taken by the critic to the principles (6) and (7) are fully met by the above and no more need be written on this point.

In the paper here referred to, Prof. Kick goes on to state that the phenomenon of "elevation of the elastic limit by strain," claimed to have been discovered by the writer, was discovered by Gen. Uchatius of Vienna, and published in April, 1874. (*Die Stahlbronge Vortrag. Gehalten am* 10 *April*, 1874, *Wien.*)

The writer is greatly pleased to find his work confirmed by so distinguished an authority, but his own discovery of this remarkable and important phenomenon was made months earlier, and was announced at the Annual Meeting of this Society, November, 1873, and formally placed on record in a "Note on the Resistance of Materials," read November 19th, 1873. (Transactions, Vol. II, page 239.) The phenomonon was also discovered by Com. Beardslee, U. S. Navy, soon after, and by an entirely independent method of investigation, and was made known by him before the end of that year. It has since been observed by many experimenters, but the writer has as yet met with no claim of priority of discovery.

Prof. Kick asserts that the extensions estimated by the writer cannot be correct, because of a diminution of length in the specimen, and because of the influence of the cohesion existing between the inner and outer fibres of the mass. The writer can only say that experiment does not seem to confirm these assumptions and assertions.

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point of rupture at a point of largely reduced section.—*Translator.*] the greatest reduction of area will occur at the point of rupture, and at that point, also, is the greatest elongation.

If we determine the percentage, or the extension by taking the greatest length, $a\ b$, immediately before rupture, and designate the proportion of elongation by the quotient, $\frac{a'b'-ab}{a\ b}$, then, for the same material, very different values will be obtained, according to the length $a\ b$, whereas, the amount of extension at the point of rupture, which is nearly the same in either long or short specimens, greatly changes the percentage of extension.

It is easily seen that a large percentage of extension will be obtained with very short test-pieces than with long ones of the same material. The reduction of area, therefore, affords a means of measuring the ductility of the material, affording, however, no precise determination of the percentage of elongation, which can only have a definite value when taken within the elastic limit.

The theory of strength of materials is a department of mechanics in which the greatest care should be exercised in drawing conclusions; it would also seem to be better to admit this, where satisfactory results are not obtained, than to enter with indefinite phrases into the realms of conjecture.

In that part in which Thurston treats of the effect of temperature upon the resistance of materials, conclusions 1 to 9 (*Vol. III, page* 21) have no significance, and simply say, "We do not know what is determined." The following sentences are not more valuable: (10.) That

In regard to the elongations given by the writer, amounting, with some ductile materials, to 120 per cent., it need only be repeated that it was explicitly stated that those figures are given as the best indication of the ductile quality of the material, that they are proportional to the maximum elongation of the most extended portions of the metal tested by tension, for the very reason stated in opposition by Prof. Kick, that the tension specimen invariably "necks down," and does not stretch as a whole, or uniformly; and it was stated that these factors of extension are related to the reduction of cross-section observed in tension, and are such as do occur within the elastic limit in homogeneous materials and such as would be observed were the material under tension, to draw down uniformly from end to end until fracture occurs, leaving the whole piece, in that case, of the diameter of the fractured section actually observed in the tension experiments.

The writer has stated his idea that the reduction of section by tension and not the extension of the whole specimen, is the most accurate measure of the ductility of the material. After passing the elastic limit, and after "necking down" begins, the elongation of a test-piece under tension is a function of its diameter and not of its length; and the whole extension may be expressed by the formula, $E = A\,l + B\int d$, an expression which the writer has not yet met with in any work on this subject.

The writer has noted these errors of the critic with as much surprise as regret, and especially as he finds them associated with the very excellent caution against "roaming in the fields of conjecture" in such scientific work.

Finally, comparing conclusions (10) and (11) with (19) and (20 ?) in which the effects of temperature are referred to, the critic notes an apparent

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the general effect of increase or decrease of temperature is, with solid bodies, to decrease or increase their power of resistance to rupture, or to change of form and their capability of sustaining dead loads. (11.) That the general effect of change of temperature is to produce change of ductility, and, consequently, change of resilience or power of resisting shocks and of carrying live loads. This change is usually opposite in direction, and greater in degree than the variation simultaneously occurring in tenacity. On the other hand: (19.) In pure and well-worked metals, decrease of temperature is accompanied by an increase of strength as well as an increase of elasticity and resilience. The last statement is evidently contradictory of conclusion 11. Which is now correct?

With the great care which we know to have been taken in the translation, these inconsistencies must be from the original, which we have not at hand. We are the more certain of this from the fact that other discrepancies exist, which, from appearances, could only have been transferred from the original, although they are of comparative insignificance.

In these opposing statements, the value of the Thurston machine is not contested for *practical* purposes. In many cases the diagrams recorded by this undeniably simple apparatus have contributed to the confirmation of tests of resistance of materials, and the merit of Thurston is decided and indisputed.

discrepancy which a more careful reading would have explained and the necessity of reference to them, perhaps, not have arisen. It is not, however, impossible that the writer was not sufficiently explicit. Referring to the original paper, it will be seen that the author quotes from an earlier monograph on the effects of temperature in which all of the earlier researches of both physicists and engineers, so far as they were accessible to him, were collated, and the conclusions, derived by comparison, were that a rise of temperature decreases the resisting power of materials while increasing their ductility and sometimes their resilience; a decrease of temperature seemed to produce the opposite effect. The generally conflicting testimony of those who, on the one hand, had experimented by steady stress, and those who, on the other hand, had experimented by shock, thus seemed to be reconcilable. The apparent discrepancies between authorities were concluded to be due to differences of method similar to those which are claimed by Prof. Kick to distinguish the researches of the writer from those of the better known authorities, —but with more reason.

Subsequently the invention of the autographic testing machine having, for the first time, furnished a means of making simultaneous determinations of the several mechanical properties of the test-piece, the real facts seemed to be proven to be slightly different, and as stated in (20) that with pure well-worked metals the principle enunciated in (28) is fully illustrated, and a decrease of temperature is accompanied by an increase of strength, ductility and resilience; (21) that materials which are impure and irregular in character may exhibit exceptions to and even reversals of that principle in changes of ductility, and, while increasing in power of resisting simple stress, may, by a diminution of temperature, lose their power of resisting shocks; and that the effect of change of temperature probably varies with the character of the material.

The writer is grateful for the pleasant compliment contained in the closing paragraph of the paper of Prof. Kick, and trusts that the above remarks may indicate that the ordinarily useful work of the confessedly valuable addition to "practical" testing apparatus, which has been found in the autographic recording testing machine may prove to be supplemented by not less valuable scientific work.