FATIGUE IN THE ELASTICITY OF STRETCHING.

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ALTHOUGH twenty-eight years have passed since the publication of Lord Kelvin's article, in which he describes the phenomenon of fatigue in the elasticity of torsion, no one to my knowledge has yet demonstrated the existence of a similar fatigue in the elasticity of stretching. In some of Lord Kelvin's experiments, it will be remembered, this phenomenon was very marked. In one experiment[1] he used two exactly similar wires stretched by vibrators of equal weight and equal moment of inertia. No. 1 had been kept at rest for nine days, while No. 2 had been kept oscillating more or less every day during that time. In an experiment then made, No. 1 subsided from an amplitude of 20 scale-divisions to 10, after 100 vibrations in 242 seconds, making a period of 2.42 seconds. No. 2 subsided through an equal angle, after 44 or 45 vibrations, and the period was 2.495 seconds. The increase in the period is thus a shade over 3 per cent, and this means a diminution of over 6 per cent in the modulus of rigidity. While the phenomenon of fatigue in the elasticity of stretching is much less marked, it gives us, nevertheless, a new fact concerning the properties of matter, and deserves careful attention.

The observations noted below were made in the spring of 1891 in the Physical Institute of the University of Strassburg. The object then immediately in view was to demonstrate the invalidity of Hooke's law, and to point out a method of determining the Young's modulus of an unstrained wire. Consequently the phenomena described below were unlooked for, and at that time received merely a passing glance. Then, on account of lack of time, no attempt was made to subject the phenomena to an

[1] Mathematical and Physical Papers, Vol. III., p. 26.

extended investigation; and since that time there have not been at the disposal of the writer place and apparatus suitable for experiments demanding such a high degree of accuracy.

The apparatus used was able to give exceptionally accurate results. Since all its details, together with the method of experiment, have been fully described in an earlier article,[1] extended description of the same is omitted here. Suffice it to say that the wires used were about 23 m. in length and 0.25 mm. in diameter; that the temperature was remarkably constant, and could be measured to $\frac{1}{80}°$; and that the constant use of two cathetometers enabled me to keep the true elastic lengthening separate from the elastic after-effect (*elastische Nachwirkung*) and all other disturbing influences.

A careful examination of my results showed that the elastic lengthenings in the first of any series of measurements were uniformly less than those caused by the same stretching weights later in the day, the temperature remaining either exactly or approximately the same. This is shown clearly in the table below, where the first column shows the weights added to the pan; the second, the lengthenings produced in an unfatigued steel wire, as given by the averages of three sets of observations; the third, the lengthenings produced in the fatigued wire, as given by the averages of six sets of observations: —

Kilograms.	Millimeters.	Millimeters.	Difference.
0.2	6.993	7.009	0.016
0.4	14.065	14.085	0.020
0.6	21.188	21.210	0.022
0.8	28.353	28.366	0.013
1.0	35.548	35.569	0.021
1.2	42.792	42.807	0.015

It cannot be said that these differences in the last column are due to a diminution of the Young's modulus on account of a rise in temperature; for, after each one of the three sets of measure-

[1] Wiedemann's Annalen, Vol. 44, No. 11, 1891, and Am. Jour. Sci., Vol. XLIII., January, 1892.

ments, the averages of which are given in the second column, I immediately took two other sets, and thus I get the six sets, the averages of which appear in the third column. In every such series of three sets of measurements, the temperature was entirely free from fluctuation. The rise during the first series was $\frac{1}{4}°$. It was the same in the second, and was $\frac{1}{20}°$ in the third; and in this third series the fatigue was as marked as in either of the others.

The probable error in the measurements was small. For example, when the weight added to the pan was 0.4 kg. the elastic lengthenings produced were as follows: —

	May 12.		May 13.		May 15.		Mean.
Unfatigued wire .	14.065		14.06		14.07		14.065
Fatigued wire .	14.08	14.08	14.08	14.09	14.085	14.095	14.085

The increase in the measurements, made May 15, is due chiefly to the fact that on that day the temperature of the tower in which the observations were made was nearly $\frac{1}{2}°$ higher than on the other two days.[1]

Brass wire gave phenomena exactly similar to those described above. In the following table are given the results of the first four sets of measurements, made April 25, between 8.58 and 10.57 A.M. The rise in temperature in this time was perfectly uniform, and amounted to $\frac{1}{4}°$; and, according to Dr. Miller, this would not lower the modulus by more than $\frac{1}{8000}$.

Kilograms.	Millimeters.	Millimeters.	Millimeters.	Millimeters.
0.2	7.10	.13	.12	.11
0.4	14.25	.265	.26	.28
0.6	21.47	.50	.50	.50
0.8	28.76	.775	.78	.77
1.0	36.11	.125	.14	.145
1.2	43.53	.57	.56	.55
1.4	51.05	.07	.065	.07
1.6	58.66	.71	.685	.71
1.8	66.33	.355	.335	.36

[1] According to the measurements of Dr. Andreas Miller, of Munich, an increase of $\frac{1}{2}°$ in temperature lowers the Young's modulus of steel by $\frac{1}{5000}$.

The lengthenings obtained in the first set are evidently less than the others. Whether the fatigue is progressive, that is, whether it is a function of the time the wire is kept in use, my measurements do not indicate.

A similar fatigue was distinctly noticed in a silver wire.

In copper wire the after-effect was troublesome, and obscured the phenomenon of fatigue, and indeed made it uncertain whether in this particular metal the phenomenon existed. I consider it probable, however, that it does exist, just as in the other metals mentioned.

In answer to the query, Why is the fatigue in the elasticity of stretching so comparatively slight, the following may be said: In Lord Kelvin's torsion experiments the elastic forces had been kept in almost constant action for several days. In my experiments, on the other hand, measurements were made at intervals of two minutes, and the stretching weight was allowed to remain on the pan only twelve or thirteen seconds, so that even when observations were being made the elastic forces were in action only 10 per cent of the time, and this was about 1 per cent of the entire day. Under these circumstances, it is hardly surprising that the fatigue is slight. It will be borne in mind that if I had kept the elastic forces continually in action, I should have had a superposition of after-effects, and this after-effect was the one thing which I was at that time particularly anxious to avoid.

Reference should here be made to an article by Dr. Miller,[1] entitled "Der primäre und sekundäre longitudinale Elastizitätsmodul und die thermische Konstante des Letzteren." Dr. Miller states that he found in his measurements that the lengthening of his wire was greater in his first measurement than in the succeeding ones, a result exactly the reverse of mine. Still he states that the Young's modulus of the wire in its original condition, or its primary modulus (E_p), as he terms it, is greater than its modulus after it has been subjected to repeated stresses, — its secondary modulus (E_s). This statement accords with my result, and is doubtless true, although his measurements give him no

[1] Aus den Abhandlungen der k. bayer. Akademie der Wiss., II., Cl. XV., Bd. III., Abth.

ground for making it. He correctly says that $E_p = \frac{\pi_1}{l_0}$ and $E_s = \frac{\pi_2}{\lambda}$, and then goes to work to prove that $E_p > E_s$. In his demonstration, however, he in one place makes the incorrect assumption that $E_s = \frac{\pi_1}{l_0 + l}$, where l is what he terms the *Nachwirkungsrückstand*, a lengthening due chiefly to the after-effect. So we would here remark, in the first place, that in computing the modulus, no one is justified in combining this l with the true elastic lengthening; and, in the second place, that if we assume $E_p = \frac{\pi_1}{l_0}$ and $E_s = \frac{\pi_1}{l_0 + l}$, we see at a glance that of necessity $E_p > E_s$ (l being in every case a positive quantity), and the author's laborious proof is superfluous.

The elastic lengthenings in my measurements are affected to an inappreciable extent by the after-effect, and not at all by the thermal effects within the wire, as has been demonstrated in my article, already cited.

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