of entomology, at the Kansas State Agricultural College, have been promoted, respectively, to a professorship and associate professorship in the same institution. Dr. R. H. Painter, who recently received his doctorate from Ohio State University, has been appointed assistant professor. During the summer he was engaged in entomological work in Spanish Honduras.

DISCUSSION

HOOKE'S LAW

It is distressing to observe the unwarranted reverence for Hooke's law so very generally shown by writers of text-books on physics for schools and colleges. This law dates back to 1676 and in the words "ut tensio sic vis" teaches the proportionality between stress and elastic strain. For example, an excellent English text-book, much used in our colleges and universities, states, "So long as the weight used to stretch the wire is not so great as to produce a permanent elongation of the wire, it is found that the elongation is proportional to the stretching force. This is known as Hooke's law." Two American text-books, intended for college use and published about a year ago, teach the same error. Still another American text-book, excellent and very widely used, devotes a third of a page to actual experimental data and states that the table "shows that the elongation is closely proportional to the stretching force." An inspection of these data, however, shows only a rough proportionality. A certain well-known scientific company is doing harm to young experimenters all over the country by publishing an elaborate instruction sheet which conveys the impression that accurate measurements should show strict proportionality between strain and stress. Moreover, I find that some of the writers in the *Philosophical Magazine* have the same mistaken impression.

In view of facts such as the above—and many more might be cited—I feel that a public service will be rendered if I exhume a prehistoric article of mine and make known the truth. This article was published in 1891 in Volume 44 of Wiedemann's Annalen under the title "Ueber das Gesetz der elastischen Dehnung" and a translation appeared in the January, 1892, number of the *American Journal of Science*. The wires investigated were of silver, copper, steel and brass, and they all *without a single exception* showed that the elastic lengthening increases more rapidly than the stretching force. The results with a brass wire are given below, but in order to show more strikingly the inaccuracy of Hooke's law I have made certain additions to the table as given in the published article.

BRASS WIRE

Mean of 20 series of measurements

Added wt. in grams	Lengthening in mm	Successive increments
200	7.111	7.111
400	14.272	7.161
600	21.488	7.216
800	28.770	7.282
1000	36.119	7.349
1200	43.554	7.435
1400	51.076	7.522
1600	58.679	7.603
1800	66.341	7.662

The elastic lengthenings x may be calculated from the added weights p in kg using the equation

$$x = 35.4385p + .5353p^2 + .1487p^3$$

and the discrepancy between observed and calculated values averages .0015 mm.

The probable error of the lengthening 43.554 mm was .0024 mm, and this is indicative of the accuracy of all the measurements. Thus it is clear from the above table that there is *without a single exception* an increase in the successive increments, and moreover the differences of the successive increments, .050 .055 .066 .067 .086 .087 .081 .059, indicate an increase nearly to the end. The diameter of the wire was .282 mm, the length 22700 mm and the initial load 665 grams.

To those curious to know the behavior of a wire unstrained by an initial load the following figures are interesting. Obviously a wire is strained somewhat by its own weight, and a small initial load is absolutely necessary in order to make the wire straight.

COPPER WIRE

Mean of 16 series of measurements

Added wt. in grams	Lengthening in mm	Successive increments
200	5.531	5.531
400	11.084	5.553
600	16.671	5.587
800	22.298	5.627
1000	27.949	5.651
1200	33.646	5.697

In the following measurements made with a copper wire of nearly the same length and diameter, the initial load (weight of pan, damper and half the weight of the wire) was only 192 grams.

Here again is an unbroken increase in the successive increments, and in their differences is an increase followed by a decrease.

If any one should repeat my work or extend it to other metals which I did not test I suggest that the following precautions be observed: Use wire coiled in large coils instead of wound on spools. If spool wire is used it should first be made smaller by a draw-plate and so straightened. Use fine wire, say .25 to .30 mm in diameter, so that after the cooling which necessarily ensues when a wire is stretched it may regain the air temperature before the lengthening is measured and before the elastic after-effect has a chance to appear. It was found that if the lengthenings were measured about twelve seconds after the application of the loads the measurements were not perceptibly vitiated either by the adiabatic cooling or by the elastic after-effect. Of course the wire should be long and one must always guard against error arising from changes in tower-temperature.

Since it is clear from the above that the ratio of stress to strain in a wire is a variable, it follows that the modulus of elasticity is also a variable, and so if we wish to speak of *the* modulus we should mean the modulus of the unstrained body. How to calculate this is shown in the original articles from which the above figures are taken.

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THE QUANTITATIVE THEORY OF SEX

THE quantitative theory of sex was first derived by the present writer in 1912 (preliminary note 1911), in essentially the same form as it stands today, from his experiments on intersexuality in the gipsy moth. (The term intersexuality was only used since 1915.) The theory claims that in both sexes determiners for femaleness and maleness are present, the relative quantities of which are balanced in such a way that one or the other has the upper hand in the respective sexes. Which of them is to be present in the higher quantity is decided by the mechanism of the sex-chromosomes, the meaning of which is thus explained, namely: one of the determiners (for maleness in the Abraxas type, for femaleness in the Drosophila type) is situated within the X-chromosome, the others (those for femaleness in the Abraxas type, for maleness in the Drosophila type) outside the X-chromosomes (in 1912 we assumed in the autosomes). Therefore, the always identical determiners for one sex are confronted either with one or with two doses of the determiners of the other sex. If the relative quantities of both are balanced in such a way that FF>M<MM (Abraxas type) and MM>F<FF (Drosophila type), normal sex-determination is explained on a quantitative basis: as a quantitative relation between male and female determiners, as I prefer to say, or a balance, as English writers prefer to call it. Intersexuality then appears, if the normal quantitative relation is changed in favor of the other sex in a definite way. When this theory was first derived in 1912 more than one sex-factor for each sex were assumed, namely, at least two for sex and secondary sex characters, and it was pointed out that each might really mean a group of linked factors. This notion has been abandoned since 1914, because the experiments show only the presence of one gene and therefore no necessity arose to split this into a group of linked genes. In 1912, further, the notion of the relative quantities was clearly indicated by the equations FF>M<MM. However, to fit these notions into the general genetical views the quantity of the genes was expressed in terms of their effect and called potencies or valencies. It was proved that M is inherited in the X-chromosome and it was assumed that F was inherited in an autosome. Later work, however, proved that in the gypsy moth F is inherited maternally (which is an experimental fact, not a theory) and it was made highly probable that this means inheritance in the Y-chromosome. Later, it was shown (and corroborated by Schweitzer and Lent) that in addition to the main factor for femaleness in the Y-chromosome there are at least two more factors for femaleness in the autosomes, the relation being a parallel to the main factor for spotting in rodents and its different modifiers. The last step was taken when (since 1917) the quantitative relation (balance) of the sex-differentiators could be linked with the embryological facts regarding intersexuality and thus the quantitative explanation of the mechanism of sex-determination could be enlarged into a physiological theory of sex-differentiation. The results of this analysis were published in a considerable number of papers since 1911, including a book (1920, also translated into English by Dakin) in which the quantitative theory was applied to the whole sex-problem and another book ("Die Quantitative Grundlage von Vererbung und Artbildung") in which the theory was developed into a general theory of heredity. I am glad to say that many zoologists and botanists have since accepted my theory as a solution of the sex problem and have assisted in their further development, thus creating quite an extensive literature about the subject. One of the most noteworthy among these contributions, which, however, had escaped the writer's notice, when publishing his book, was Standfuss's discovery of what nowadays is called triploid intersexuality (1908, 1914). Standfuss had shown a long time ago that sexual intergrades, at that time called gynandromorphs, were obtained in back-crosses from species-crosses in moths. After Federley's work on the chromosomes in such species-