your and *yore* are classed as identical in sound. Your, shore, and drawer are thus "phoneticised" into yawer, shawer, and drawer. These words are, indeed, often so pronounced in dialectic speech; but the science of phonetics must be retrograding instead of advancing when, in an "Introductory Science Text-Book," such differences can be ignored, and such mere negligences cited as examples of correct usage.

All short vowels stop sharply on consonant-R, as on other consonants, as in *parrot, very, spirit, sorry, hurry;* but long vowels take on the connective glide even before consonant-R, as in *weary, fairy, wiry, gory, fury.* Thus wea(ə)ry, fai(ə)ry, wi(ə)ry, etc.

The vowel quality inherent in the mouth-cavity of R is that of er-ir in *her, sir.* Consequently, in such words as *firm, yearn,* the r has the effect of lengthening the syllable by making it contain two sounds of the same vowel. Let us put the words under the microscope: —

fi - - - - (ə)rm; yea - - - - (ə)rn.

Test this further by analyzing the syllable "word." If the r were "silent," the vowel would be sharply stopped by the consonant d. Thus, wo - - - - d; but the true pronunciation of this syllable interposes a glide between the vowel and the d. Thus, wo - - - - (ə)rd.

In forming this smooth transitional r the tongue is slightly lifted from the bed of the jaw; therefore when a vowel follows the r, the consonant sound of the letter is also developed; as in fe ring = fear ring There is a tendency among many speakers to finish all open vowels with this lift of the tongue, so that the consonant r is inadvertently interpolated between two words, as in "Pennsylvania-r-Avenue, "I saw-r-it all."

Nice distinctions — like those which are the subject of this paper — are of no importance where mere intelligibility is concerned; for example, in the speech of the deaf. In such cases, the widest differences may be disregarded, so long as the words are understood. But in the study of phonetics, the most minute varieties require to be distinguished, because what in one case may be a distinction with but little difference, may in another become a very shibboleth.

I make no apology for introducing so small a topic to your attention. In a practical subject nothing is too small to be carefully investigated. The whole organism of speech is but small, and the differences of organic action from which the greatest elementary distinctions result are, in actual measurement, exceedingly small.

The sounds of R, with all their differences — rough, smooth guttural, lingual, labial, definite, indefinite — are only one in kind; and we must recognize them in their faintest as well as in their most obtrusive forms.

ON THE SECULAR MOTION OF A FREE MAGNETIC NEEDLE.[1]

BY L. A. BAUER.

A MAGNETIC needle suspended so as to move freely in all directions will set itself tangent to the lines of terrestrial magnetic force. At any particular epoch it will have a definite direction. It wil make a definite angle with the meridian, which, measured in the horizontal plane, is known as its declination, also a definite angle with this plane, which, measured in the vertical plane, is termed its inclination or dip. About this mean position of equilibrium a variety of small periodic variations take place, accompanied at times by fitful or irregular ones, which occasionally become quite respectable. Concerning this we shall have nothing to say. But the needle undergoes another, and by far the largest excursion, requiring centuries for its fulfilment. Since its discovery in 1634 by Gellibrand, as exhibited in the secular variation of one of its co-ordinate angles, the declination, it has been the cause of no end of fruitless speculation. It has engaged some of the best minds and given rise to most ingenious theories, but the riddle is still unsolved.

As the needle assumes different positions for different epochs, it gradually sweeps out in space a cone, whose vertex is the centre of gravity of the needle. Or, if you describe a sphere having as a centre the centre of gravity of the needle, and prolong the axis of the needle until it intersects the sphere, the successive intersections will form some tortuous curve. The geometric nature of this cone, or of this tortuous curve, remains to be investigated. A preliminary analytical attempt was made by Quetelet in 1877. He used fifty years of continuous observations of declinations and dip made at Brussels, and found that a cone of revolution would best fit his observations, the period of a complete revolution being 512 years.[2] Mr. Schott made a graphical attempt for an average New England station, using about fifty years of observation. The scantiness of his material prevented him from making any safe deduction as to the course of the needle.[3]

To our knowledge, however, no attempt has as yet been made for the long series of observation which we possess at quite a number of stations The usual custom is to discuss *separately* the secular variation of the different magnetic elements, as though they were *different* effects of forces acting, instead of *component* effects. We believe that this, in some measure, is the reason that with 100–300 years of observation no greater headway has been made in the conception of the requirements of the secular-variation problem.

With the view presented of the problem, some of the interesting questions we may ask ourselves are: Will the orbit described by the north end, say, of the needle, be a closed curve or approximately so? That is, will the needle at the end of a certain period assume the same direction that it had before, and again sweep over the same curve in the same length of time? Or, will the needle never return to a previous position, and thus never fulfil a secular variation period? If we have such a thing as a true period, is it the same all over the globe? If we have to deal with *different* periods, as the discussions of declination observations at various stations would seem to indicate, are these local or independent, and thus belong to different systems of magnetic forces? Or, do they but indicate different *stages* in the development of the secular variation, whereby either the period itself is a fluctuting one, or the orbit consists of several branches or loops? If the secular waves travel from east to west, traversing the whole globe, then by making an instantaneous circuit of the earth in an easterly direction, shall we find the needle at every station farther along in its secular orbit? Shall we find a continuously progressive and consistent motion throughout our survey, thus correlating the stations and referring the cause to a common origin? If we find this to be but roughly so, then by selecting as a base station, one where we have a long series of observations, we may with the aid of the shorter series at other stations, by adopting a time-coefficient determined from a comparison of the curves, attempt to answer some of the questions propounded without waiting until centuries have given us a complete period? Finally, what is the law of force acting upon the needle to cause it to describe its orbit?

To carry on a study of the secular variation to the best advantage, it would be highly desirable that at all stations where we have a tolerably long series of observations they be put in the best shape possible by one familiar with the subject and the station. It would then be an easy matter to establish secular variation stations all over the globe, where future observations might be made. This would mean simply the inauguration of a grand scheme, the fruits of which might not be seen for centuries. While such a gathering of material has already been made for many stations, there is, however, abundant material left.

The first station selected for discussion is London, where we have the best series of observations of both elements. The declinations date with Boroughs's in 1580. They can be represented (within their probable error) by the following formula, derived

[1] Abstract of a paper read before Section A of the A.A.A.S., Aug. 18, 1892.

[2] See Bulletins de l'académie royale de sciences etc. de Bruxelles, 47me année, 2me serie, T. xlv.

[3] See U. S. Coast and Geodetic Survey Report for 1885, App. No. 6, p. 272.

from a least-square adjustment of all the observations between 1580 and 1890: —

$$D = +6.24^\circ + 17.75^\circ \sin [0.7^\circ (t - 1850) + 112.7^\circ] \text{ - - - - } (1).$$
$$\pm .10 \qquad\qquad\qquad\qquad \pm .2$$

Where D stands for the declination at any time t, positive when west, and $0.7^\circ = \frac{360^\circ}{514}$ = angular motion in one year. The differences between the observed and computed values would seem to indicate a fluctuating smaller period of about 80 years, having a variable parameter of about $\frac{1}{4}^\circ$. Somewhat similar results were found at Paris by Mr. Schott, and at Christiania by Professor Weyer. No attempt was made at present to establish this second term, it lying within the probable error, which for a computed result is $\pm$ 20′.

The inclinations date with Norman's in 1576. The observations between this date and 1891 can be represented by the following formula: —

$$I = 70.40^\circ - 3.98^\circ \sin [0.7^\circ (t - 1850) + 23.0^\circ] \text{ - - - - } (2).$$
$$\pm .065 \quad \pm .09 \qquad\qquad\qquad \pm .1$$

Where I = inclination at any time t and the period involved, 514 years — apparently the same as for (1).

The probable error of a computed value is $\pm$ 10′, which, considering the material, is satisfactory. Both (1) and (2) apply to latitude 51° 30′ and longitude 0° 07′ west of Greenwich. The mean of the Greenwich and Kew observations was taken to apply to this station. Comparing (1) and (2), a remarkable result peculiar to this station will be noticed — that the epochs are practically complementary, hence the following approximate relation between the declination and dip can be found: —

$$\frac{\delta^2}{(17.75)^2} + \frac{\iota^2}{(3.98)^2} = 1 \text{ - - - - } (3).$$

Where $\delta = D - 6.24$ and $\iota = I - 70\ 40$.

From (1) and (2) the following results are obtained: —

	Declination.		Inclination.	
	Amount.	Time.	Amount.	Time.
Maximum	24.0° W.	1817.5	74.4°	1688.5
Minimum	11.5° E.	1560.4	66.4°	1945.7
Range	35.5°	— —	8.0°	— —
Mean	6.2° W.	1689, 1946	70.4°	1560, 1817
Zero	0.0°	1660, 1976		

From which it appears that for London the mean declination takes place about the epoch of maximum and minimum inclination, and *vice versa*.

With the aid of formula (1) and (2) the curve described in space by the north end of a free magnetic needle was now accurately constructed and graphically exhibited. It was shown that the first approximation of the curve could be taken as a spherical ellipse, the period being about 500 years. For Paris, a similar result was obtained and exhibited, using provisional formulæ. For both stations the curve lay to the greater part west of the true meridian, and the direction of the motion (standing at the centre of the needle and looking towards the north end) was that of the hands of a watch, or opposite to that of the earth. A rough survey was then made of the globe in an easterly direction approximately in the latitude of London, and elucidated by a diagram. It was found that the needle was farther along in its secular orbit at every station. The curves for some stations in the southern hemisphere were also exhibited.

The following conclusions were reached: —

1. The direction of the secular motion of the north end of a free magnetic needle in both hemispheres is that of the hands of a watch.

2. That if the secular orbit is a *single closed curve*, then are the periods *different* for different stations.

3. That if the period is a *common* one, then must the orbit be a closed curve of two or more loops lying within each other. We are getting then, at present, a small loop in America and a larger one in Europe.

4. That our present feeling is rather that, strictly speaking, we have no such thing as a *period* of secular variation, but that the needle partakes of a sort of spiral motion, returning approximately to a former position, but never exactly so.

Future study may possibly modify some of these conclusions. The possibilities opened up by such a study as outlined were next briefly alluded to, and reference made to a possible extension of the well-known Gaussian analysis by the introduction of the variable t — time.

In conclusion, can we not say with Sabine: "Viewed in itself and its various relations the magnetism of the earth cannot be counted less than one of the most important branches of the physical history of the planet we inhabit."

LETTERS TO THE EDITOR.

**** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

On Biological Nomenclature.

I HAVE read with interested attention the discussions by botanists in *Science* on this subject. It would appear that they are fully alive to the need of some canons of nomenclature in their branch of biology — a need which has been felt, and supplied of late years, by the zoölogists.

The nomenclature of botany has always seemed to me to be more stable and uniform than that of zoölogy, for the reason, as I supposed, that the naming of new genera and species has, for the most part, been reserved to a comparatively few leaders in the science; and the same cause has contributed to the fixity of botanical classification, in comparison with the incessant taxonomic fluctuations which zoölogy has suffered.

With the late great increase in the number of working botanists, the distinction of a small select "caste" of authoritative namers and describers in botany seems to be breaking down, with the various good and evil results attending this transfer of power from a privileged oligarchy to more democratic rule.

I think that not improbably the botanists who are now exercised over names may examine with much confidence the canons of nomenclature lately formulated and rigidly enforced by the American Ornithologists' Union. These rules have been found to work admirably in practice. They may not be the best possible, but on the whole they are the best extant. A number of leaders in other departments of zoölogy, besides ornithology, as in mammalogy, herpetology, ichthyology, malacology, entomology, etc., have found them entirely available. If some two or three of the rules are not so acceptable as the rest, yet it seems to be generally conceded that it is better to abide by them all, than to dissent from the code as a whole on account of a few comparatively unimportant points that may not be liked so well as the rest are.

Referring to the excellent article of C. H. Tyler Townsend in *Science* of Sept. 16, it seems to me that the moot points he raises have all been considered carefully by the ornithologists, who have settled each of these questions to very general satisfaction; and that the considerations upon which their conclusions have been reached are entirely applicable to the botanical questions involved.

I wish to say a word respecting the somewhat epigrammatic rule, "once a synonym, always a synonym," for the form of which I am measurably responsible, if I remember rightly. Like any other curt sententious saying, the rule is, as I perceive by Mr. Townsend's remarks, liable to be misunderstood. There is no question that, as he correctly says, "If a form which had been described and then thought to be the same as some other species, is later proven to be a valid species, the name originally proposed should stand." Certainly it should. That is not the application at all of the phrase "once a synonym, always a synonym;" and I never heard before of its application to the case Mr. Townsend adduces. What the aphorism really means is best illustrated by a concrete example:

Let there be a genus *Smithia* in botany. Let a genus *Jonesia*