DISCUSSION AND CORRESPONDENCE

NEWTON'S LAW OF GRAVITATION—A DANGER SIGNAL

THE writer is fully aware of the fact that it is very much easier to criticize adversely a book on physics than it is to write a reliable text in this field. On the other hand, he has also observed that it is almost impossible to eradicate an error when it has once obtained a start through the prestige of the name of a noted scientist. A brief history of a case of this kind is recorded on page 127 of the second edition of "The Principles and Methods of Geometrical Optics" by James P. C. Southall. But, in my opinion, errors are not as serious in advanced works as in text-books written for college students, since investigators think independently, whereas the undergraduate usually considers anything printed in a book adopted by his instructor to be absolutely unimpeachable, quite regardless of whether the author is a novice in the art of exact expression or an old, seasoned writer. Among the relatively large number of text-books on physics submitted to me recently for examination two contain an altogether gratuitous inexactness of statement of Newton's law of gravitation which should not be allowed to pass unnoticed. It is especially desirable to call attention to this matter because it is highly probable that these two volumes will influence a large number of students.

"Any two bodies attract each other with a force proportional to the product of their masses (*i.e.*, to the amount of matter they contain) and inversely proportional to the square of the distance between them." ". . . when the distance between them is *d*." Again: "Any two bodies in the universe attract each other with a force which is directly proportional to the product of their masses and inversely proportional to the square of the distance between them." ". . . *d* the distance between their centers, . . ."

The book from which the first quotations were made is extremely misleading with respect to the meaning of the word "distance" and the second text adds confusion by referring to an undefined "center." For illustration, what is *the* center of a plane triangular lamina of homogeneous material? Is it the center of mass, or the center of the inscribed circle, or of the circumscribed circle, or of an escribed circle, or the intersection of the altitude lines, etc.? Taking the law as stated and implied, what would be the value found by a student for the force between a homogeneous sphere and an exactly concentric enclosing homogeneous spherical shell of finite thickness of wall? If the distance (zero) between the centers is taken the result will be infinite; if the radial distance between the outer surface of the solid sphere and the inner surface of the enveloping shell is taken the result will be variable. In marked contrast with the above citations is the presentation in article 6, page 139, of another very recent book, the one by A. A. Knowlton.

It would be helpful to many students if they were afforded the opportunity of mastering the following extremely simple case. Let a mass M (or the center of a homogeneous sphere) coincide with the geometrical center of an arc of a circle (or wire of negligible cross-section) along which a mass m is uniformly distributed. Let the radius of the arc and the angle subtended at M by the extremities of the arc be respectively a and 2ϑ. The force along the bisector of the angle 2ϑ is given correctly by

$$F = (GmM \sin\vartheta)/(a^2\vartheta).$$

If the mass m were concentrated at a point on this bisector at a distance x_1 from M, the same force would be exerted when

$$x_1 = a\vartheta^{\frac{1}{2}}(\sin\vartheta)^{\frac{1}{2}}.$$

The center of mass of the arc is situated at

$$\bar{x} = (a \sin\vartheta)/\vartheta.$$

If "center" in the second quotation means the middle point of the arc the force will be

$$F' = (GMm)/a^2 = (\vartheta F)/\sin\vartheta.$$

If "center" signifies the center of mass the force will be

$$F'' = (GMm\vartheta^2)/(a \sin\vartheta)^2 = (\vartheta^2 F)/(\sin\vartheta)^2.$$

The errors in per cent. may be read from the following table.

ϑ	$100\,F'/F$	$100F''/F$
0	100.	100.
$\pi/6$	$100\pi/3 = 104.72$	$100\pi^2/27 = 114.84$
$\pi/4$	$100\pi/2^{3/2} = 111.07$	$100\pi^2/2^{3/2} = 137.03$
$\pi/3$	$200\pi/3^{3/2} = 120.92$	$800\pi^2/3^{3/2} = 176.80$
$\pi/2$	$100\pi/2 = 157.08$	$100\pi^2/8 = 387.58$

Also, when

$$\vartheta = \pi/4,\ x_1 = 1.0539a \text{ and } \bar{x} = 0.9003a.$$

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THE PRAIRIES AGAIN

THE explanation of the cause of the treelessness of the prairies, offered by Professor Jones in SCIENCE for October 7, 1927, represents one of those cases, especially numerous in this field, in which broad generalizations are based on rather limited observations. It is therein assumed that rapid drainage of postglacial waters was responsible for the establishment of the prairie flora.

In Illinois, Iowa, northern Missouri, Nebraska and portions of other states, there are extensive deposits of loess, the rougher portions of which are still covered with a prairie flora, though a large part of the former prairie is now under cultivation.

This loess, where it appears at the surface (it is sometimes interglacial), is a distinctly post-glacial deposit gradually built up by winds from the dust of sandbars, dunes, etc. In western Iowa it sometimes exceeds one hundred feet in thickness, and in other parts of the region frequently runs from fifteen to thirty-five feet.

This deposit was formed *after* the post-glacial drainage—most of it long after—for the deposition was evidently comparatively slow. The terrestrial molluscan fauna of the loess clearly points to a well-drained surface (in many cases distinctly dry), formed in a mild climate long after glacial conditions had ceased. There could have been no rapid drainage of glacial waters on these areas, and the explanation fails for all the loess-covered prairie areas.

It fails for the sharp loess ridges in western Iowa and Missouri which have forest on the sheltered side and prairie on the exposed side, though the slope and soil are essentially the same. During the more than forty years that the writer has studied these ridges there has been no noticeable change in the line between forest and prairie.

It fails for the driftless, or nearly driftless, area of northeastern Iowa and southwestern Wisconsin, on which prairie areas are still found.

It fails utterly for the "prairie openings" found on every type of soil in the rougher wooded sections of the prairie region.

It fails also for such regions as the Kansan drift area, covering a large part of Iowa south and west, a large part of which was treeless. The Kansan surface was deeply eroded, undoubtedly long after the recession of the glaciers, and over much of it deposits of loess were formed even after the erosion.

It clearly fails also for the alluvial prairies. Many of our stream valleys show alternating forest and prairie areas, according to exposure to drying atmospheric factors. In most of these cases the drainage has been essentially the same and the differences in flora can not be explained by merely assuming a subsequent advance of the forest.

Post-glacial drainage fails to explain another type —the sandy prairie on fixed dunes such as occur in Muscatine and Harrison counties, Iowa, on the treeless parts of the dunes at Gary and Miller, Indiana, and elsewhere. The flora of these open fixed dunes consists in large part, or wholly, of prairie plants. There was no rapid drainage here, yet the prairie flora is established.

Finally, it fails for the large prairie areas in southern Kansas, Arkansas, Oklahoma and Texas, far below the limit of glacial ice, and where there is absolutely no evidence that the surfaces were swept by glacial waters.

Professor Jones does not regard the grasses of the prairies as a climax stage and states that the forests are gradually encroaching upon them.

It should be noted that over large prairie areas grasses did not (and do not) constitute the dominant vegetation, but numerous other plants, especially Compositae and Leguminosae, formed the conspicuous part of the flora.

That the prairie flora represents a climax stage is shown by the following facts:

(1) The flora has persisted, in many cases in strips of ten to twenty feet in width, through many years without material change.

(2) Where the prairie turf has been broken or disturbed the disturbed areas are first invaded by native "prairie weeds" (often with introduced weeds), but in the course of a few seasons the prairie flora comes back.[1]

(3) Thousands of acres of the prairie groves have perished, and most of them in the prairie region proper would do so in time if man did not change conditions by planting and cultivation and by creating protected zones by erecting buildings and planting shelterbelts. Their areas were again occupied by the prairie flora where cultivation did not interfere.

It should also be noted that there has been no such general natural invasion of the prairie by forest as might be inferred from statements of some recent writers. Where changes have occurred along the borders they represent comparatively slight fluctuations, caused by alternating wet and dry cycles of seasons. They are quite as likely to be retreats as advances. In favored places the forest may extend itself over ridges by the protection afforded by its own border trees, but this is not general. Where contour lines break abruptly, the line between prairie and forest is quite sure to be sharp and constant.

In SCIENCE of December 30, 1927, Professor Arthur M. Miller controverts Professor Jones' arguments concerning the origin of the prairies, and presents his explanation of the treelessness of the Kentucky "Barrens," stating that "aided possibly by forest fires, vast herds of buffalo and deer and elk were able to reclaim it from the forest."

[1] For discussion of specific cases see writer's "Papers on the Prairie" in University of Iowa Studies in Natural History, Vol. XI, No. 5, 1925.

While Professor Miller offers this as a local and not a general explanation, the bison and fires have been regarded as general causes.[2]

Even if we concede the accuracy of the maps, records and traditions cited, the explanation is not convincing, even for the particular area. As a general cause, however, applicable to the entire prairie region, it is wholly untenable. The objections to it may be briefly stated as follows:

(1) Larger herds of bison, etc., disappeared from Iowa long before the white man settled the state, yet the prairie has held its own even to the present.

(2) The sharp loess ridges in western Iowa and Missouri, already noted, with their sharply defined prairie and forested slopes, also militate against this theory.

(3) Many of the prairie slopes in western and northeastern Iowa are so steep that heavy animals like the bison would find a footing with difficulty and would not be tempted to use such places when more accessible prairie pastures were abundant nearby.

(4) The number of animals necessary to keep the prairie treeless would be vastly greater than anything the oldest records suggest. There are not even traditions of such large herds as would be postulated by this theory. In Iowa alone there were more than forty thousand square miles of prairie.

(5) The numerous prairie openings (true prairie, as shown by the flora) which are scattered through the groves and forests of the general prairie region, could not have been formed in this way. During the more than fifty years that the writer has observed these openings they have maintained their prairie characteristics, yet the bison is a mere faint tradition in this region.

(6) The association of the bison and fires as a cause is contradictory. If the bison kept the prairie closely cropped where did the fires find fuel?

The flora of the prairies is distinctly xerophytic, and its presence upon the areas which we call "prairies" is determined by those ecological factors which make for xerophytism.

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ON CHANGING FAMILY NAMES

WITH Professor Bradley's proposition (SCIENCE, LXVIII, 1928, pp. 102–104) to allow the same rules to operate in selecting the type genus of a family as in selecting the type species of a genus the writer is in entire accord. He does not, however, accept Professor Bradley's proposed regulation to cover changes of family names, and, in place of his proposed Article 5, would substitute:

Art. 5. When the name of the type genus of a family or subfamily is found to be a homonym or synonym, the family or subfamily takes the next oldest valid name.

This is in accord with the principles involved in changing a generic name. When such a name must be changed, it is not insisted that the new name be based on the same type species as the invalid name—as Professor Bradley insists that the new family name must be based on the same type genus as the old—but the next oldest name is used, a name that may frequently be based on a type subgenerically distinct from the first type species.

The writer feels that his proposal is superior to Professor Bradley's for the following reasons: (1) It carries out consistently as applied to the selection of the type genera of families and subfamilies the same principles now utilized in the selection of the type species of genera and subgenera. (2) In cases where different authors have used different names for the same family or subfamily and the oldest of these becomes invalid, it allows for the introduction of the next oldest already more or less familiar name rather than the creation of an entirely new and strange name.

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OBSERVATIONS IN PERU

IN "Science News" (SCIENCE for August 3) there is mention of the earthquakes in and along the northern coast of Peru. Here at Negritos, which is located just about one mile northeast of Pt. Parinas (Lat. 4° 40′ 15″ South; Lon. 81° 20′ 5.9″ West), we felt the quakes, which were strong enough to cause the hanging electric lights to swing and to be felt by persons seated and standing. However, in my laboratory a wash bottle flask of one liter capacity which was standing on its head was not upset. This would seem to show that here the shocks were not very strong.

It may also be of interest to some of the readers of SCIENCE to know that on the 17th of August we had a very red and long-lasting sunset. It was most unusual for this season of the year. The sky was very bright well up to about two thirds of the way to the zenith, the light lasted about one hour later than the normal sunset glow (which here is usually short). The two nights following were also fairly red but much shorter glow and less color. The why of this I do not know unless it was the volcanic eruption in or near Batavia, in Java, as reported in the paper of August 10 (*El Tempo,* Piura, Peru).

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NEGRITOS, PERU

[2] Harshberger, John W., "Phytogeographic Survey of North America," 1911, p. 517; Campbell, D. H., "An Outline of Plant Geography," 1926, pp. 106, 107.