mainder are now assigned to eight widely separated genera. Of these the two belonging to *Tropidonotus* are the only ones which fail to correspond to the generic character quoted above. No matter, says the extremist in priority, under the rules they must furnish the type!

It has by now become quite clear that uniformity is not to be reached through any of the codes in use, if indeed it ever can be retroactively established by any other not yet constructed, for there will always be some who will not purchase it at too high a price, and the prevailing demand of the moment forgets that there is value also in diversity. Then again, the uncertainty attending the practical application of some of the rules now most advocated precludes denial.

A high authority in matters of nomenclature, whom we all respect and esteem, has lately said in SCIENCE that even elimination can lead to only one result when properly applied—but the trouble is that each eliminator thinks that his way of applying it is the proper one. It is easy to get men to agree to abide by law, but another thing to get agreement as to how the law works.

The devious paths to diverse goals followed by those who have attempted the elimination of *Coluber* Linn. is illuminating as to the certainty of the method—but who shall say, as yet, which one is right?

Cope in 1886 was led by the "rules" to *Natrix* as the proper name for *Coluber*. In 1888 the "rules" led him to substitute it for *Tropidonotus*.

The fact is that meaningless conglomerates such as *Natrix* and many other genera of the early days of zoological classification can not be used now under the rules for determining types without doing occasional violence to intelligence. They never did represent definite conceptions and they ought not to be considered in nomenclature. By consent we allow them to Linnæus, but there is no reason why the privilege should be extended to his successors.

ARTHUR ERWIN BROWN
THE ZOOLOGICAL GARDENS,
PHILADELPHIA,
July 9

THE DISTANCES OF THE FIXED STARS

IN various astronomical and other scientific publications misleading statements are frequently made concerning our knowledge of the distances of the fixed stars. In parallax work practically all reliable observations are of a differential nature, and the interpretations of the resulting measures for distance are largely dependent upon preconceived views as to the arrangement of the stars in space.

For some years past I have been engaged in observational and theoretical work on that intricate problem—where is the origin and what is the physical structure of our sidereal system? The results so far obtained are novel, since they indicate that the structure is radial, in other words the stars and nebulas of our system are moving either directly towards or directly away from our sun; the observed derivations from radial motion being attributed to the unsymmetrical distribution of the attracting masses, and also to the presence of bodies having a secondary origin.

The indications also point to the conclusion that, as seen from our sun, a vast majority of the stars and nebulas are confined to a region whose radial depth is much less than the distance of this region from our sun. Since bodies so situated may be comparatively near to us and still have various radial velocities without causing sensible changes in the configuration of the heavens, the seemingly unchanging aspect of the Milky Way[1] and other celestial regions is explained without the necessity of assigning such great distances (and consequently such great masses) to the bodies of our system.

Considering the still undetermined constants entering into the problem, and the lack of a rigorous method for making direct measures, it surely is no exaggeration to say that a trustworthy value of a star's parallax has not yet been obtained.

The award of the Boyden Premium by the

[1] Whether the theory is in agreement with the actual facts or not, I demonstrate that the inclination of the plane (?) of a Milky Way to the plane of the sun's equator is a necessary consequence of such a structure.

Franklin Institute to Dr. Heyl is doubtless a well-deserved honor, but when one reads in SCIENCE for June 28, 1907, on page 1013, that a definitive result is based upon the wholly unproved claim that "the distance of Algol is no less than forty light years" it seems desirable to emphasize the fact that in the present state of our knowledge the approximate distance of any particular fixed star must still be regarded as an unknown quantity.

J. M. SCHAEBERLE

ANN ARBOR,
June 30, 1907

SPECIAL ARTICLES

HENS THAT HAVE LAID TWO EGGS IN A DAY

IT is so generally believed that it is not possible for a hen to lay more than one egg in a day that a few observations that show this is not always true may be of interest.

The number of eggs laid by a hen in a year has been greatly increased, the maximum number reported by Professor Gowell, of the Maine Experiment Station, who has for a number of years been breeding to increase the yearly output, being 255. It would seem that there is no known biological reason why the maximum daily rate should be one each day, any more than that the number of eggs per year should be limited to a few broods. In either case the ultimate limit of *possibility* would seem to depend upon the ability of the individual to assimilate and transform the materials taken as food into the materials of the eggs. There may be difficulties that are not understood that would make it impossible to develop a race of hens that would habitually lay more than one egg in a day, as there have been difficulties encountered in getting birds that will lay every day in the year, but *a priori* there seems to be no known biological reason why a hen should not lay more than one egg in a day.

While experimenting on the fertility of eggs it became necessary to keep a daily record of the hens that laid. This was done by means of trap nests that were arranged so whenever a hen entered a nest a door was dropped behind her that not only kept her prisoner until she was liberated by the attendant, but excluded all others. That is, the door was locked so it would not swing in either direction. The ordinary numbered leg bands were used to distinguish individuals. The birds under observation were White Wyandottes.

The latter part of February or early in March, 1906, a pullet that had recently begun laying apparently laid two eggs in a day. Although it seemed a clear case it was not recorded as it was thought possible that a mistake had been made in reading the number on the band. When the same hen again laid two eggs on March 21, record was made and to guard against possible errors in reading the number on the band she was banded on both legs, thus distinctly marking her, as no other hen in the house had two bands.

During March and April there are records of five days on each of which this hen laid two eggs. Although her record was carefully kept for more than a year and a half, there are no other records of her having laid more than one egg in a day. It should be added that the records of days on which she was known to lay two eggs come during the months of her greatest egg-producing activity. In fact it will be seen that in the thirty-three days listed in the following table the hen actually laid thirty-four eggs.

HEN NO. 1. MARCH, 1906

Date	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31
Eggs.....	1	1	1	1	1	0	1	1	2	1	0	1	1	1	1	1	1	1	2

APRIL

Date	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Eggs...	0	1	2	1	2	1	2	1	1	1	1	1	1	0

During the year and a half over which my observations extend there have been a number of instances of hens laying two eggs in a day, but the records show that in most cases on either the day before or the day after that on which two eggs were deposited, no egg was laid. Such cases may reasonably be accounted for by supposing premature or delayed delivery, but this can not be true of the hen