discussion of all the available observations of this star up to the year 1894 relatively to the form and changes of the light curve.

COMET PERRINE.—We have received a telegram from Prof. Kreutz, Kiel, dated October 18, in which we are informed that Comet Perrine on October 16, at 9h. 38·8m., Lick mean time, appeared of the eighth magnitude, and was situated in R.A. 3h. 36m. 8s. and N.P.D. 23° 13′ 16″. It was observed to have a small tail.

A later telegram, dated October 19, gives the following position and magnitude: October 18, 11h. 31·1m., Pola mean time, R.A. 3h. 25m. 31s., N.P.D. 20° 34′ 44″, magnitude 10·0.

HEREDITARY COLOUR IN HORSES.

MY attention has been drawn to a collection of data on the hereditary transmission of colour in horses, which appeared in the last Christmas Number of the *Horseman*, a newspaper published in Chicago, U.S. It is signed with the pseudonym of "Tron Kirk." I corresponded with the author, who is noted for his knowledge of horse-breeding, and he assures me of their substantial correctness. His statistics are chiefly obtained from breeders' catalogues, and, however valuable in other ways, fail seriously through the great disproportion which must exist between the number of the different sires and that of the dams, a single sire in the polygamatous arrangements of a stud begetting a numerous offspring from nearly as many dams. It is stated that no less than 3100 foals were begotten by only 46 different bay sires, or more than an average of 67 foals by each sire. Now the number of offspring of the 16 different forms of colour union registered in Table I. is, with one exception, by no means large: in 9 cases it is less than 100, and in one of these it is only 6. Consequently the prepotencies, or the reverse, of individual sire will fail to balance each other, and are sure to produce anomalous results.

The data I propose to use are those contained in Table I.; they have been extracted from the memoir in the *Horseman*, but are newly arranged both in line and column. I have omitted grey altogether, no grey stallions being recorded, and all the grey foals coming from grey dams.

TABLE I.

No. of observations	Colour of		Per cents. of colours in offspring				Totals
	Dam	Sire	Chestnut	Bay	Brown	Black	
68	Ches	Ches	100	—	—	—	100
1900·	Bay	Bay	10	81	6	3	100
19	Brn	Brn	—	42	52	5	99
25	Blk	Blk	—	4	28	68	100
407	Ches	Bay	33	61	4	2	100
366	Bay	Ches	30	63	3	4	100
52	Ches	Brn	—	86	11	2	99
69	Brn	Ches	16	65	10	9	100
72	Ches	Blk	6	76	15	3	100
57	Blk	Ches	30	40	—	30	100
221	Bay	Brn	1	79	14	6	100
450	Brn	Bay	6	66	18	10	100
156	Bay	Blk	3	60	30	7	100
268	Blk	Bay	7	53	16	24	100
55	Brn	Blk	—	22	38	40	100
6	Blk	Brn	—	16	50	33	99

My first inquiry was to determine whether the sire or the dam exercises the larger influence in transmitting his or her own colour to the offspring, this being a point on which different breeders express contradictory opinions. The truth in the present instance is easily arrived at by means of Table II., where the percentages of the offspring who resemble their Dam are compared with those that resemble their Sire. This is done in each several pair of "reciprocal" unions, such as that which consists of [*Dam*, bay—*Sire*, brown]; and of [*Dam*, brown—*Sire*, bay]. The table shows a total of 394 cases of resemblance

TABLE II.

Colour of the Dam	Per cent. of offspring who in colour resemble their Dam						
	Sire	p.c.	Sire	p.c.	Sire	p.c.	Totals
Chestnut ...	Bay	33	Brn	0	Blk	6	39
Bay	Ches	63	Brn	79	Blk	60	202
Brown ...	Ches	10	Bay	18	Blk	38	66
Black ...	Ches	30	Bay	24	Brn	33	87
							394

Colour of the Sire	Per cent. of offspring who in colour resemble their Sire						
	Dam	p.c.	Dam	p c.	Dam	p.c.	Totals
Chestnut ...	Bay	30	Brn	16	Blk	30	76
Bay	Ches	61	Brn	66	Blk	53	180
Brown ...	Ches	11	Bay	14	Blk	50	75
Black ...	Ches	3	Bay	7	Brn	40	50
							381

in the one set, to 381 in the other; in short, it proves that the potency of the dam in transmitting colour is substantially the same as that of the sire.

The intention of the second inquiry was to test an important part of my recent theory on "The average contribution of each several ancestor to the total heritage of the offspring" (*Proc. R. Soc.*, June 3, 1897, and NATURE, July 8, 1897). According to this theory each of the two parents contributes *on the average* one quarter of the total heritage, each of the four grandparents one sixteenth, and so on. If this be strictly true, and if the potency of the two sexes be the same, one half of the varied offspring from the [bay—bay] pairs added to one half of those of an equal number from the [brown—brown] pairs, would be identical in character with the same number of the offspring of [bay—brown], also with those of [brown—bay]. The same holds true for every other form of union between sires and dams of different colours. However, the statistics in Table I. run so roughly that this particular comparison would fail to lead to trustworthy results. It is true that reciprocal unions are seen to give rise to similar results in [chestnut and bay], to fairly similar ones in both [bay and brown], and in [brown and black], and to not very dissimilar ones in [bay and black], but each of the two remaining sets is incongruous. Moreover, the figures contained in them run wildly; thus in the line [black—chestnut] the sequence of the numbers, 30, 40, 0, 30, is a statistical impossibility, and in the line [chestnut—brown] the sequence of 0, 86, 11, 2, is very suspicious. It is obvious that a more trustworthy interpretation of the true state of the case might be deduced from these rude data, if the four entries in each line could be appropriately consolidated so as to be expressed by a single number. It occurred to me that a good way of doing so would be to determine the amount of red pigment corresponding to each entry in the same line, and to sum those amounts. Guided at first by the judgment of the eye, and afterwards by observing how nearly each successive assumption satisfied the observed facts, I fixed on the following allowances, supposing full red pigmentation to count as 1. For chestnut, 0·8; for bay, 0·7; for brown, 0·4; and, recollecting the considerable amount of red pigment in the blackest human hair, I fixed the allowance for black at 0·1.

There are twelve equations in which these four values appear; so if all are fairly well satisfied by the above assumptions, we may rest content. I did not take pains to have the red pigment

extracted and weighed from equal quantities of hair of the four several colours, because there is room for doubt as to the medium tints of those colours, and because those mediums may not be precisely the same in America as here. It seemed better to work the problem backwards, in the way to be easily understood from the following example. The [bay—bay] unions, according to Table I., produce 10 per cent. of chestnut offspring, 81 per cent. of bay, 6 per cent. of brown, and 3 per cent. of black. Therefore the quantity of red contained in each hundred offspring of [bay—bay] parents should be reckoned at

$$10 \times 0{\cdot}8 + 81 \times 0{\cdot}7 + 6 \times 0{\cdot}4 + 3 \times 0{\cdot}1 = 67{\cdot}4 \text{ units.}$$

Since this is the amount of red contributed by the *two* bay parents, the contribution from either bay parent singly will be only half as much, or 33·7.

Similarly the contribution of red from a single chestnut parent will be found to be 40·0; of a brown, 25·3; and of a black, 10·4. Consequently the quantity of red in each hundred offspring of [bay and brown] unions will, according to the theory, be reckoned at

$$33{\cdot}7 + 25{\cdot}3 = 59 \text{ units.}$$

This number has been entered in its proper place in Table III. as the "calculated" value, and may there be compared with the "observed" value obtained from the reciprocal unions of [*Dam*, bay—*Sire*, brown], and of [*Dam*, brown—*Sire*, bay]. Now, the former of these is seen in Table I. to have produced 1 per cent. of chestnut, 79 per cent. of bay, 14 per cent. of brown, and 6 per cent. of black, yielding by the method just described, 62·3 units of red; by a similar treatment the latter of these unions, namely [*Dam*, brown—*Sire*, bay] will be found to yield 59·2 units. The mean of 62·3 and 59·2 being 60·75, that is 61 when reckoned to the nearest integer, is also entered in a separate column in Table III.

TABLE III.—*Amount of Red in Offspring, observed and calculated.*

No. of cases	Offspring of		Red observed	No. of cases	Offspring of		Red observed	Mean observed	Calculated	Differences
	Dam	Sire			Dam	Sire				
407	Ches	Bay	71	366	Bay	Ches	70	70	74	+ 4
52	Ches	Brn	65	69	Brn	Ches	63	64	65	+ 1
72	Ches	Blk	64	57	Blk	Ches	55	60	50	− 10
221	Bay	Brn	62	450	Brn	Bay	59	61	59	− 2
156	Bay	Blk	57	268	Blk	Bay	52	54	41	− 13
55	Brn	Blk	35	6	Blk	Brn	35	35	36	+ 1

The general result of the comparisons is that calculation agrees with observation as closely as the rudeness of the statistics could lead one to expect. The average error between each of the six calculations and the corresponding means of each of the six pairs of reciprocal observations is about 5 per cent., while the greatest error barely exceeds 10 per cent. I therefore consider these results to corroborate that part of my theory of inheritance which they were intended to test.

Permit me to take this opportunity of removing a possible misapprehension respecting the scope of my theory. That theory is intended to apply only to the offspring of parents who, being of the *same variety*, differ in having a greater or less amount of such characteristics as any individual of that variety may normally possess. It does *not* relate to the offspring of parents of different varieties; in short, it has nothing to do with hybridism, for in that case the offspring of two diverse parents do not necessarily assume an intermediate form.

I am further desirous of drawing attention to an absurd error in my recent memoir quoted above, through the accidental transposition by me of the words Dam and Sire in the side columns of the Table II. of that memoir (which Table was constructed out of the Table I. that preceded it). The result was that the potency of the Dam to that of the Sire in transmitting colour was stated to have come out as 6 to 5, whereas the fact is the exact converse, namely, as 5 to 6. I ought to add that this strange blunder, which was detected and obligingly pointed out to me by two separate correspondents, had no effect upon the general conclusions of the memoir, because the ratio of 6 to 5 was treated as an insignificant disproportion, and the two sexes were dealt with on equal terms.

FRANCIS GALTON.

AERONAUTICAL ASCENTS FOR MEASURING THE ELECTRICAL FIELD OF THE AIR.

ON September 11, M. Lecadet, astronomer of the Lyons Observatory, made his fifth aeronautical ascent for testing the electricity of the air at high altitudes. This system of observation was invented by Dr. Exner, a member of the Vienna Academy of Sciences, who sent into the atmosphere a balloon directed by Lechner, on June 6, 1885. The balloon reached only an altitude of 600 metres, and the results of the reading, taken by an inexperienced observer, were of no value.

On September 27, 1892, M. Andrée, director of the Lyons Observatory, determined to ascend himself, with M. Lecadet as his assistant. The ascent ended in a total wreck.

In the following year M. Lecadet made two ascents at Meudon with the Government balloon, after having procured permission from the War Office. In the first trip (August 1893) the balloon was conducted by Captains Paul Renard and Julian, and ascended only to a very moderate altitude. The second experiment took place on August 9, and only one officer, Captain Hugot, was sent up with M. Lecadet. The experiments showed that the electrical field of the air gradually diminished, though the measures were executed with the cumbersome instrument designed by Dr. Exner.

In the following month, in September 1893, two ascents were made from Tempelhof, with the balloons of the Prussian Government, by Dr. Bornstein, a member of the Berlin Society for Aerial Navigation. The results were about the same as those observed by Lecadet.

After carrying out these experiments, M. Lecadet devised a new instrument. The readings are taken with an Exner electrometer, but instead of being placed in equilibrium with the electricity of the air by two jets of water at a vertical distance of five metres from each other, the effect is obtained by two cylinders of paper impregnated with nitrate of lead, which, once being lighted, are burning without flame. They are placed each to the extremity of one single wire, whose length can be varied at will. The apparatus weighs 3 or 4 kilogs., instead of at least 50. Many experiments made at Lyons Observatory proved that there was no danger of ignition of the gas issuing from the balloon, but the Minister of War declined the proposal of authorising ascents from its balloon arsenal.

The first experiment with the new system was tried at Lyons a few months ago with M. Boulade, an able local aeronaut. The experiments were conducted with great care and success up to 1000 metres, and the electric field of the air was again found to gradually decrease.

As Lyons is in close vicinity with the Alpine district, it was considered unsafe to try an ascent at a great altitude under these circumstances. M. Lecadet therefore went to Paris, and secured the assistance of M. Besançon, a member of the international committee for the *Ballons Sondes*. The balloon had a capacity of 1700 cubic metres; it was a new one, in China silk. There was no cloud in the sky, and only some vapour near the earth's surface. The two aeronauts reached an altitude of 4200 metres. The wind was rather strong, as in five hours they ran about 220 miles in a W.S.W. direction. They landed at Aubigne (Marne et Loire) in a regular gale, but escaped unhurt, owing to the use of a special grapnel and tearing-rope invented by M. Besançon.

The readings taken were very numerous—about 300—and the results are a continuous decrease of the electric field from the level of the ground. Through the courtesy of M. Lecadet, we are enabled to give the summary of results, which will be laid before the Academy of Sciences by M. Mascart when all the calculations have been completed, which will require some time.

Altitude	$\frac{\Delta v}{\Delta x}$
Close to the earth	120 volts
1000 metres	39 ,,
4200 ,, ...	11 ,,

The results show that at about 6000 metres the $\frac{\Delta v}{\Delta t}$ will be almost 0. Then the balloon will have reached the surface of electrical equilibrium.

If the electrical tension at this altitude is supposed to be 0, the potential of the earth $\Sigma \frac{\Delta v}{\Delta x} = -160{,}000$ (about). The eminent director of the French Meteorological Service has expressed his satisfaction at the results obtained, and has suggested