transparent, as though the hand were seen through it. This experiment is not new, but I have never seen it described. The explanation of it is quite evident.

2. Drop a blot of ink upon the palm of the hand, at the point where the hole appears to be, and again observe as before. Unless the attention be strongly concentrated upon objects seen through the tube, the ink-spot will be visible within the tube (apparently), but that part of the hand upon which it rests will be invisible, unless special attention be directed to the hand. Ordinarily the spot will appear opaque. By directing the tube upon brilliantly illuminated objects, it will, however, appear transparent, and may be made to disappear by proper effort. By concentrating the attention upon the hand, it may also be seen within the tube (especially if strongly illuminated), that part immediately surrounding the ink-spot appearing first.

3. Substitute for the hand a sheet of unruled paper, and for the ink-spot a small hole cut through the paper. The small hole will appear within the tube, distinguishing itself by its higher illumination, the paper immediately surrounding it being invisible. Many other curious experiments will suggest themselves. For example: if an ink-spot somewhat larger than the tube be observed, the lower end of the tube will appear to be blackened on the inside.

4. While making these experiments, an improvement upon the experiment described in NATURE, vol. xii., p. 502, was suggested, as follows:—Look through a paper tube with one eye at green paper, and through another tube with the other eye, at red paper. The paper should be illuminated by the direct solar ray. The two colours, at first vivid, are rapidly enfeebled. After half a minute, transfer both eyes to either one of the papers, say red. To the eye fatigued by green, the red colour is very brilliant, and the effect is the more striking on account of the simultaneous impressions now received by the two eyes.

Washington University, St. Louis F. E. NIPHER

Antedated Books

THE evil practice of issuing antedated periodicals has long been a matter of complaint amongst naturalists. The editor of the *Journal für Ornithologie* is a well-known sinner in this respect—the quarterly number of that journal, although invariably dated on the first day of each quarter, being always several months in arrear. But a still more flagrant instance of this practice is now before me in the third number of the new edition of Layard's "Birds of South Africa," which, although only issued to the subscribers within these last few days, is dated on the cover "May, 1875!" As two new genera (*Aëthocichla* and *Neocichla*) are instituted herein, the result is to give these names an unjust priority of fifteen months over what they are legally entitled to. This seems to be a still easier method of gaining precedence than the American practice of publishing telegraphic bulletins of new discoveries, and will not, I trust, be persevered in, if attention is called to it. F.Z.S.

August 7

Protective Mimicry

I HAVE been reading over in the file of NATURE the controversy that arose out of Mr. Alfred Bennett's paper at the British Association in 1870, on "Natural Selection from a Mathematical Point of View," in which he attacked Darwin's theory on what seems to be one of its strongest points, namely, protective mimicry. I do not feel certain whether he is right or not in denying that natural selection is adequate to produce mimicry. The argument really depends on a question of fact, namely, whether the first variation could be great enough to be useful to its possessor; and from the great comparative variability of colour, I see no decided impossibility in this.

But the writers in that controversy neglected other facts of colour which it seems impossible for natural selection to produce, from the infinite improbability of a first variation ever occurring. One of these is the change of colour with the seasons in such animals as the ermine, which is brown in summer and white in winter. Had the ermine been either permanently brown or permanently white, there would have been nothing wonderful in it, but it seems impossible that the character of becoming white in the winter and brown in the summer could ever have originated in ordinary spontaneous variation, without a guiding intelligence.

Another case of at least equal difficulty is the case of change of colour for the purpose of protection, from moment to moment. The chameleon is the best known instance of this, but I believe there are many such cases among fishes. It seems utterly impossible for such a character to originate in spontaneous unguided variation. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, July 20

A REMARKABLE instance of this phenomenon is shown in a small crustacean, of the genus "Rypton" (Mr. Spence Bate has not yet determined whether it be a new species or no). This very delicate little animal is found only in holes in the coral inhabited by the common "Echinus" of Mauritius; its colour is a deep purple, with four longitudinal stripes of a much lighter tint; and this is precisely the pattern of the spines of the said Echinus. WILMOT H. T. POWER

λ Ophiuchi

I AM going to undertake the calculation of elements of λ Ophiuchi, which you proposed to calculators in NATURE, vol. xiv. p. 29. I shall also within a short time give orbits of γ Coronæ, which has not been separated as far as I know since spring, 1867, when it was observed in Harvard College, and of ξ Libræ (Scorpii). About the latter binary star we know but very little. Mädler has given a circular orbit with a period of over 100 years, while Thiele gives a highly eccentric orbit with a period of about fifty years. It may very likely be found that the older determination is the most trustworthy, but the case deserves a thorough examination, which I am going to make. I have been engaged in a re-determination of elements of 6 Coronæ, by which the long period has been re-ascertained.

There are different other double stars which with advantage might be inquired into, and thus prevent different investigators from confining themselves to the same objects, while others remain uncared for. I hope that you will be kind enough to publish the above remarks in your widely circulated paper.

Markree Observatory, Collooney, Ireland, July 17 WILLIAM DOBERCK

The Cuckoo

THE cuckoo is still singing in this part of the country. I may mention, as a point of some interest, that the note of this bird in South Germany is precisely the same in pitch as it is here, the observations in both cases having been made with a tuning-fork in the month of May.

Can any of your readers inform me whether the cuckoo *in all parts of the country* is in the habit of occasionally singing the *cuc* without the *koo?* GEORGE J. ROMANES

Ross-shire, July 24

THE FERMENTATION OF URINE AND THE GERM THEORY

CAN Bacteria or their germs live in liquor potassæ (Pharm. Brit.) when it is raised to the boiling-point (212° F.)? Such is now the simple issue to which certain great controversies have been reduced. If Bacteria germs cannot resist such an exposure, then, by M. Pasteur's own implicit admission, his exclusive germ-theory of fermentation must be considered to be overthrown by the broader physico-chemical theory. The truth or not of M. Pasteur's germ-theory is the central question in dispute, but standing on either side, or in close juxtaposition, are two dependent subjects of controversy whose importance for biological science and for medicine is even greater.

The question whether living matter can or cannot originate *de novo*, for example, depends upon the answer which is to be given to the question whether Bacteria and their germs are or are not killed in boiling liquor potassæ. This, also, is practically admitted by M. Pasteur in his comments (*Comptes Rendus*, July 17) upon my recent experimental evidence.

The other subordinate problem, the solution of which

depends upon the same issue, is the truth or falsity of an exclusive germ-theory in explanation of the origin and spread of the communicable diseases. If the germ-theory of fertilisation can be proved to be untrue, and if living ferments can be proved to originate spontaneously, we should soon cease to hear much about an exclusive germ-theory of disease. This derivative doctrine would not long survive the death of its parents.

Thus M. Pasteur's theory of fermentation, the popular doctrine *omne vivum ex vivo*, and the germ-theory of disease, must all be simultaneously overthrown if it cannot be proved by M. Pasteur, or some of his followers, that Bacteria germs are not killed when they are immersed in strong liquor potassæ raised to 212° F. (100° C.). How matters have been brought to this desperate predicament may be explained in a very few words.

Since the year 1862, M. Pasteur has defended four main positions, on the strength of which he has based his germ-theory of fermentation, his repudiation of "spontaneous generation," and his support to the germ-theory of disease. In the year 1870 and subsequently, I have many times submitted these four positions to an independent criticism by means of experiment, and the result has been a confirmation of two of them, and a rejection of the remaining two—the rejection being necessitated rather on account of facts obtained by new methods than from any implied defect in the particular range of experiments from which so distinguished an investigator as M. Pasteur deduced his opinions. Our respective views on these four points may be thus tabulated :—

PASTEUR	BASTIAN
1. That *all* boiled organic infusions having an acid reaction will, when protected from contamination, invariably remain pure.	1. That *some* boiled organic infusions having an acid reaction will, when protected from contamination, ferment and swarm with Bacteria.
2. That all Bacteria and their germs are killed in such boiled acid fluids.	2. Do.
3. That *some* boiled organic infusions having a neutral, or slightly alkaline reaction, will not remain pure even when protected from contamination. They will, on the contrary, ferment and swarm with Bacteria.	3. Do.
4. That all Bacteria and their germs are not killed in such neutral or slightly alkaline fluids raised to 212° F. (100° C.).	4. That all Bacteria and their germs are killed in such neutral or slightly alkaline fluids raised to 212° F. (100° C.).

Omitting, for the present, all intermediate stages of the controversy which has now been carried on for several years between one or other of M. Pasteur's followers and myself, I will proceed to show how the questions between us have been affected by my latest researches.

The results obtained in these researches have been embodied in a memoir communicated to the Royal Society on June 15, of which an abstract was published in NATURE, vol. xiii., p. 220. A very short "Note" on the subject of these researches was also submitted to the *Académie des Sciences* on July 10, and subsequently published in the number of the *Comptes Rendus* bearing that date. M. Pasteur replied to this note at the next meeting of the Academy (*Comptes Rendus*, July 17), at a time when he would appear not to have seen the fuller abstract of my researches published in NATURE. This will account for an error into which he seems to have fallen in regard to one of the most important conditions prescribed for some of my experiments, to which I shall have occasion presently to refer. In the first place, however, I must call attention to a different part of the subject.

One of the most notable results of my recent work is this :—I have ascertained that a moderately acid urine will, after it has been boiled, remain pure when kept free from contamination at a temperature of 77°–86° F. (25°–30° C.), though the same specimen of "sterilised" urine will ferment and swarm with Bacteria in less than three days, if it is maintained at the higher temperature of 122° F. (50° C.). Many acid vegetal infusions will behave in precisely the same manner.

Here, then, is a ready means by which any careful experimenter may ascertain whether M. Pasteur is not wrong in maintaining his proposition No. 1. And if this is the case, then there is nothing for M. Pasteur to do but to renounce his exclusive germ-theory of fermentation, and to adopt the doctrine of "spontaneous generation," since he still declares that Bacteria and their germs are killed in acid fluids raised to 212° F. (100° C.). His words are (*Comptes Rendus*, July 17, p. 179) :—"J'ai prouvé directement qu'ils perissent dans un milieu acide à 100 degrés."

But there is another means of establishing the truth of my conclusions derived from these recent researches to which I will now allude. This is the point principally referred to in my "Note" to the Academy, and upon which M. Pasteur dwells in the above-mentioned communication.

As regards the frequent fertility of boiled organic fluids having a neutral or faintly alkaline reaction (No. 3) it will be seen that M. Pasteur and myself are thoroughly agreed, notwithstanding Prof. Tyndall's representations to the contrary, made in the columns of this journal in the early part of this year. M. Pasteur now says (*Comptes Rendus*, July 17, p. 178) :—"Je m'empresse de déclarer que les expériences de M. le Dr. Bastian sont, en effet, très exactes ; elles donnent *le plus souvent* les résultats qu'il indique . . . Il n'y a donc entre M. Bastian et moi qu'une différence dans l'interprétation d'expériences qui nous sont maintenant communes." The difference of interpretation to which M. Pasteur alludes depends upon our difference of view in regard to position No. 4. It was specially with the hope of dissipating any doubt remaining upon this part of the question that one section of my new experiments was undertaken. I determined to submit M. Pasteur's interpretation to the test of direct experiments, conducted in a way likely to yield decisive results.

If the fertility of the boiled neutralised fluids or infusions were really due to the survival of germs, as M. Pasteur supposes, then the boiling of the fluid in its acid state (when its germs would by admission be destroyed), and the subsequent addition to it of a sufficient amount of boiled liquor potassæ, without extraneous contamination, should be attended by negative results—that is, the fluid should remain pure, according to M. Pasteur, if it were really germless.

But numerous experiments performed in this manner have shown me that sterilised urine, to which boiled liquor potassæ, in proper quantity, is added, will ferment and swarm with Bacteria in a few days—and all the more quickly if the experimental vessels and their fluids are maintained at a temperature of 122° F. (50° C.).

M. Pasteur, whilst admitting the facts, says that this addition of boiled liquor potassæ to sterilised urine causes the mixture to ferment because such added liquor potassæ contains germs which were not killed when this fluid was raised to 212° F. (100° C.).

This, truly, is an astounding hypothesis. My reply, however, is simple. It was an objection already anticipated and met by me, as any one may see by referring to the concluding portion of my abstract, as published in NATURE.

The answer is this :—If boiled liquor potassæ were a germ-containing medium, then one or two drops of it (as of other germ-containing media) would always be capable of contaminating many ounces, or even a gallon or more of sterilised acid urine. This, however, is never the case.

The boiled liquor potassæ is only capable of imitating fermentative changes, and of leading to the appearance of Bacteria when it is added in quantities strictly regulated by the quantity and degree of acidity of the specimens of urine with which experiment is being made.

Another fact, just as strikingly opposed to M. Pasteur's view that Bacteria germs can survive in boiled liquor potassæ has been revealed by my researches on the fermentation of urine. It is this :—A very slight excess of liquor potassæ over and above the quantity needed for exact neutralisation almost always yields negative results. This, of course, would be quite inexplicable if the liquor potassæ really acted as a mere germ-containing medium.

An error of procedure of this kind, unwittingly made by M. Pasteur, because he was not forewarned, was in all probability the reason of his obtaining negative results when he operated with solid potash raised to 110° C. or higher. M. Pasteur says (*loc. cit.*, p. 179) : The potash was dropped into the urine in quantity sufficient to render it "alkaline." The negative results obtained in these trials he attributes to the fact that the potash had been heated to 230° F. (110° C.), whilst I feel certain that they were rather due to the addition of an excess of potash, seeing that the addition, as he himself says, rendered the fluid "alkaline."

Briefly, then, M. Pasteur admits me to be correct in stating that boiled liquor potassæ, in proper quantity, will fertilise sterilised urine, and I prove that his interpretation of this fact is wrong by referring him to the totally different effects which would result from the addition of one or two drops, or of a slight excess of boiled liquor potassæ. These effects are wholly irreconcilable with the notion that living germs are capable of surviving after they have been boiled in strong liquor potassæ.

H. CHARLTON BASTIAN

OUR ASTRONOMICAL COLUMN

REISSIG'S COMET (?) OF 1803.—The following particulars of a stellar-looking object, with considerable retrograde motion, were communicated to Bode—at the time the centre of general astronomical correspondence—by Reissig, of Cassel, son of a well-known optician at that place. He stated to Bode that on the morning of Feb. 2, 1803, he perceived with a 30-inch comet-seeker, near the double-star 148 Ophiuchi B. (36 Ophiuchi Fl.), a star of from 5th to 6th magnitude, which he had not remarked on Jan. 28, with a 7-feet reflector. "The star or comet," under a power of 400, appeared without sensible nebulosity, and somewhat magnified. On the early morning of Feb. 4, the stranger appeared to have moved to the westward. The weather was not clear again till the morning of the 7th, when the object was faint from presence of the full moon, and it was difficult to fix its position. On the 9th it was found near 139 Scorpii B. (25 Scorpii Fl.); at 3.2 A.M. it occulted this star, and at 4.9 there was first perceived a space between them. Unfavourable weather following, further observation was prevented. Reissig sent Bode a small chart of the path of the object "between π Ophiuchi and Antares," and the four following places, from observations with a 3-feet Gregorian reflector and an annular micrometer.

h. m.					
Feb. 2 at 4 51 A.M.	...	R.A.	253° 48′	...	Decl. 26° 19′ S.
,, 4 ,, 3 49 ,,	...	,,	252 4	...	,, 25 49
,, 8 ,, 4 4 ,,	...	,,	249 30	...	,, 25 12
,, 9 ,, 4 45 ,,	...	,,	248 51	...	,, 25 11

With regard to these places, Bode remarked that they do not lie in a regular curve, which may well be attributed to the observations (apparently rough). He observed, further, that the elongation of the object from the sun on Feb. 2 was 56° 34′ W., that as seen from the sun its motion must have been retrograde, and hence it was "a very singular comet."

On attempting to found a parabolic orbit upon the positions given by Reissig, taking, however, the place of 25 Scorpii for the place of the object at 3.2 A.M. on the 9th, it is soon apparent that the distance, instead of being very great, as Bode surmised, must have been very small, so small, indeed, that the earth's perturbations during the week's observations, might, and probably would, greatly distort the apparent track as deduced from the orbit. In fact, after a number of trials, in which, as was to be expected, the elements resulting therefrom differed but slightly and yet gave large differences in the geocentric places, we find that, assuming the elements to be—

Perihelion Passage, 1803, February 10·164 G.M.T.

Longitude of Perihelion	146° 15′
,, Ascending Node	307 45
Inclination...	0 55
Log. Perihelion Distance	9·98234

Heliocentric Motion—Direct,

the following apparent track of the comet results—

0h G.M.T.	Longitude.	Latitude.	Distance from the Earth.
	° ′	° ′	
1803, Jan. 25 ...	62 35 ...	+ 2 17 ...	0·0336
26 ...	60 52 ...	+ 2 6 ...	0·0258
27 ...	57 44 ...	+ 1 47 ...	0·0182
28 ...	49 56 ...	+ 0 57 ...	0·0106
29 ...	10 29 ...	− 3 1 ...	0·0040
30 ...	278 57 ...	− 5 18 ...	0·0065
31 ...	262 2 ...	− 4 6 ...	0·0137
Feb. 1 ...	256 2 ...	− 3 40 ...	0·0214

No further weight is to be attached to these inferences from calculation than as tending to render possible such positions of an object moving under the laws of gravitation, but duly regarding the rough character of Reissig's observations, his last place differing some ten minutes of arc from what we might judge it to have been, if 25 Scorpii were occulted an hour previous. If a comet were really moving in an orbit with elements resembling the above, it might have passed in twenty-four hours (January 29-30) from Pisces to Sagittarius, and the circumstance of the object not being found by Reissig near the place it occupied on February 2, with a much larger telescope on January 28, would be accounted for. We are met nevertheless by the difficulty, that for a body at so small a distance from the earth, to appear like a star of the fifth or sixth magnitude, devoid of nebulosity, it is necessary to assign it very small dimensions, while the appearance described is quite irreconcilable with the aspect presented by the few comets which have been seen in close proximity to the earth, particularly that of 1770, which at its perigee was upwards of two degrees in diameter according to Messier.

Reissig claimed to have discovered the comet of 1801 some twelve days before it was detected by Pons, but the account he sent Bode of his observations is a singularly lame one. (B. J., 1805, p. 129.)

It must be admitted that the examination of such observations as those of Reissig and of Huth, as treated in this column last week, is mainly a matter of curiosity, still if it be possible to show that the observations are not necessarily to be regarded as impositions upon the astronomical world, it will be granted that something is gained thereby.

SATELLITES OF SATURN.—Mr. Marth's elaborately constructed ephemerides of the satellites of Saturn appear in the *Astronomische Nachrichten*, Nos. 2,098–2,100, with some remarks on the advantage of careful estimations of conjunctions with the ends of the ring and the limbs of the ball over micrometrical measures during the next two or three years. The preparation of these ephemerides must involve an amount of labour and care of which few but those who have attempted such calculations can form any adequate idea, and their value is proportionally great.