

prised a second specimen of pitchblende from the same locality as the first; carnotite from Montrose County, Colorado; gummite from North Carolina; and pitchblende from Bohemia. All the minerals, with the exception of the gummite, contained both copper and lithium in easily recognisable amounts. The qualitative analysis of 1 gram of the gummite showed no copper, but did show the presence of lithium in small amount.

The discovery of lithium and copper in uranium-radium minerals does not necessarily indicate the change of copper into lithium, since the presence of lithium may have been fortuitous; but assuming the accuracy of Prof. Ramsay's observation, the presence of lithium in uranium-radium-copper minerals is precisely what one should expect. The presence of lithium and absence of copper in the sample of gummite may be explained by the assumption that the change of copper into lithium has been completed. It may be added that even if further investigation should reveal the absence of lithium in any uranium-radium-copper mineral, the result would not constitute a valid argument against Prof. Ramsay's hypothesis, since the latter referred to copper in solution and not in the solid state.

HERBERT N. MCCOY.

University of Chicago, November 6.

A Convenient Formula in Thermodynamics.

It is possible that many teachers of thermodynamics may not have noticed that the characteristic equation for 1 kilogram of air takes the easily rememberable form $p v = T/10$, when p is measured in standard atmospheres, v in cubic feet, and T in thermodynamic centigrade degrees, the accuracy of the even integer being fully as great as that of the gas law itself. These units are, of course, a curious mixture of the English and Continental systems, but this seldom makes much difference in actual problems, and the convenience of the formula for rough mental computations is sometimes very great.

The data upon which this computation of the gas constant is based are the statements in the third (1905) edition of Landolt and Boernstein, that 1 litre of air under standard conditions weighs 1.2928 grams, and that an English yard is 0.91438 metre, and the value $T_0 = 273^{\circ}.13$ given by Buckingham in the Bulletin of the Bureau of Standards for May. The value $R = 0.1$ is consistent with these assumptions within less than one-fiftieth of 1 per cent.

The corresponding values of C_p and C_v , reduced from the mean of the results of Regnault (1862), Wiedemann (1876), and Witkowski (1896), are $C_p = 0.3467$ and $C_v = 0.2467$ cubic-foot-atmospheres.

Cambridge, Mass.

HARVEY N. DAVIS.

A Miocene Wasp.

IN NATURE of June 13, 1901 (vol. lxxiv., p. 158), I described a curious variation in a bee (*Epeolus*), the second transverso-cubital nervure of the wings having its lower half absent. This aberration was evidently an example of "discontinuous variation," and from its occurrence in several specimens captured at the same place, it seemed that it must be inherited. There is a genus of Scoliid wasps, *Paratiphia*, in which the absence of the lower part of the first transverso-cubital nervure is normal. The species, found principally in the southern and western parts of North America, are quite numerous; and the broken nervure, looking exactly like the aberration described in the bee, is a good generic character. Nothing has hitherto been recorded concerning the past history of this genus, but I have before me a well-preserved *Paratiphia* from the Miocene shales of Florissant, Colorado, collected by Mr. S. A. Rohwer at station 14 in 1907. This insect, which I shall call *Paratiphia praefracta*, is black, with the thorax large and robust (about 4 mm. long and $3\frac{1}{4}$ mm. broad); the head rather small (slightly more than 2 mm. diameter); the antennæ thickened; the abdomen constricted between the first and second segments, and parallel sided beyond; the hind

tibiae dentate on the outer side; the wings clear hyaline, anterior wing about 7 mm. long, with the large stigma very dark, the nervures light ferruginous. The specimen is a male. The venation is perfectly normal for *Paratiphia* in every respect, including the broken transverso-cubital vein.

It is certainly an interesting fact that a character like that of an imperfect vein, which can arise suddenly as a mere aberration, should persist from Miocene times (at least) to the present, and characterise a whole genus. From observations on bees and other Hymenoptera, it is evident that this modification has occurred many thousands of times without becoming permanent; that it has become so in the case of *Paratiphia* is therefore all the more remarkable.

T. D. A. COCKERELL.

University of Colorado, Boulder, Colorado,
November 7.

The Eggs of the Platypus.

SINCE writing the notice of Mr. le Souef's book on Australian wild life in NATURE for October 24 (vol. lxxvi., p. 635), I have been making inquiries as to the existence in collections of any examples of platypus egg definitely known to have been taken from the nest after extrusion. It has been suggested to me that Mr. Caldwell and Dr. Semon might possess such specimens. The former gentleman told me, however, some years ago that he never found an extruded specimen, and I learn from Dr. Semon that he was equally unsuccessful in this respect. In his letter he writes that "I have never found extruded eggs of *Ornithorhynchus*, but only intra-uterine specimens. To obtain the former, it would be necessary to open a very large number of burrows."

In the central hall of the British Museum is shown an egg-shell of a platypus sent from Queensland by Mr. G. P. Hill in 1902, but this, like Mr. le Souef's specimens, was doubtless found in its present broken condition.

Such broken shells might, apparently, be extruded from the uterus with the foetus; and, so far as I can find, there still appears to be no definite evidence that the eggs are really laid entire.

THE REVIEWER.

November 15.

Literature relating to Australian Aborigines.

IN NATURE of May 9 (vol. lxxvi., p. 32) I observed a communication from Mr. R. H. Mathews in which he makes certain statements imputing to me, by insinuation, what amounts to literary dishonesty. Will you kindly permit me to express my views on the subject?

Mr. Mathews says that I have "ignored" certain statements made by him in communications to scientific societies, and which were published before the appearance of my "Native Tribes of South-East Australia" in 1904, in which I record the same facts.

Mr. Mathews speaks of my account of the Dora ceremony, and makes the following insinuation:—"Dr. Howitt 'ignores' that I described that rite in January, 1900. If he did not avail himself of my work, which appeared four years earlier than his, then there is a wondrous agreement in our details."

My account of the Dora ceremony was given to me by Mr. Harry E. Aldridge in 1882. It was from his own experiences at the ceremonies on more than one occasion, and he had a knowledge of the tribal language.

Mr. Mathews also says that I "ignore" a map which he published in 1900, and which is substantially the same as one at p. 44 of my work. He adds the following sentence:—"In comparing the two maps and the explanatory letterpress accompanying mine, we observe a marvellous coincidence. Many other examples could be cited."

The map showing the native tribes of South Australia at p. 44 of my work was compiled from data supplied by the Rev. Otto Siebert, who obtained them by careful and protracted inquiries from persons knowing the several localities, as well as from personal knowledge. Practically

the details as to the tribes of the Lake Eyre district and of the Flinders Range are substantially the same as those given in a sketch-map which accompanied one of my pioneer papers entitled "The Dieri and other Kindred Tribes of Central Australia," which appeared in the *Journal of the Anthropological Institute* of August, 1890. This was the result of investigations made from 1871 to that date. In this instance, if I were to follow Mr. Mathews's example, I might suggest that he has "ignored" this pioneer work, and express something similar to his "wondrous agreement in our details." There is, however, nothing to wonder at in the agreement of two investigations of the same subject. My statements are based upon independent investigations, made in some cases many years ago, when the results were recorded for future publications. If Mr. Mathews finds instances in which his information has a "wondrous" agreement with mine, he may rest assured that his conclusions are correct.

My information as to the Yantruwunta class-names *Tiniwa* and *Kulpuru* was received from my correspondent the Rev. Otto Siebert, and was based on his personal knowledge, in 1897.

I learn from Mr. Mathews's letter that he has sent "more than one hundred contributions to various scientific societies." I have only met with two of them, neither of which recommended itself to me by its accuracy. It is therefore difficult to understand how I can have "ignored" statements of which I am ignorant.

It will be evident that there is no foundation for Mr. Mathews's injurious insinuations, which, I regret to say, bear upon them evidence of a personal animus. The case is one of *Honi soit qui mal y pense*.

A. W. HOWITT.

Metung, Victoria, July 8.

In the foregoing remarks Dr. A. W. Howitt states that at the time his book appeared, in 1904, he was not aware that I had published a description of the *Toara* (*Dora*) ceremony, and also a map of South Australia, four years previously. It seems incredible that he did not make himself acquainted with the current literature bearing upon the Australian aborigines up to the time he published his book.

If Mr. Howitt obtained his account of the *Dora* from Mr. Aldridge in 1882, it is remarkable that he did not publish it until twenty-two years afterwards, especially as he was very prompt in publishing his report of the *Kuringal* ceremony in 1884 and the *Jeraeil* in 1885. He does not, however, appear to have had much confidence in Mr. Aldridge's account of the laws of marriage and descent in vogue among the same tribes who practised the *Dora* ceremony. He says that Mr. Aldridge sent him "a number of tables of marriages and descents, which differed considerably amongst themselves; so much so that the correctness of some of them seemed doubtful." Mr. Howitt did not run the risk of printing more than one table out of them all, but even that one is erroneous, as I have demonstrated elsewhere.

Mr. Howitt refers to a "sketch-map" published by him in 1890.² In that map he showed the *Urapuna* (my *Arrabunna*) tribe as being located away to the north of the *Awmani* (my *Ahminnie*). Not content with this inaccuracy, he increased its magnitude by placing another tribe, which he called the *Wongkurapuna*, on the east of the *Urapuna* and *Awmani*. He was, apparently, not at that time aware that the two names, *Urapuna* and *Wongkurapuna*, represent the same people, the prefix *wonk* merely meaning "speech."

Again, the *Kuyani* (my *Kooyeeunna*), which I place on the southern end of Lake Eyre, is shown on Mr. Howitt's "sketch-map" as occupying the country I have correctly allocated to the *Kutchnamootha* tribe. His map is altogether a blank as to the numerous tribes occupying the country from south Lake Eyre to Port Lincoln and Fowler's Bay. Even Mr. Howitt himself did not reproduce *his own map* in his book of 1904, but preferred to

utilise one which is identical with mine. Spencer and Gillen were apparently misled by the worthless map of 1890. In referring to the *Urabunna* tribe, they speak of "the *Dieri*, whose territory adjoins the *Urabunna* on the south."¹ My map shows the *Urabunna* on the western or opposite side of Lake Eyre to that on which the *Dieri* is situated.

Mr. Howitt states that he got the phratry names *Tiniwa* and *Kulpuru* independently. Even if so, he should have made himself acquainted with and referred to my prior reports of 1899 and 1900. He shirks my statement that he "had never heard of the Blood divisions," and he also passes over my claim to priority in reporting certain important facts in the sociology of the *Wiradjuri* tribes.

It is asserted by Mr. Howitt that he has only seen *two* of my articles on the Australian blacks. I contributed five articles to the *Royal Society of Victoria*, of which Mr. Howitt was a member, and I was told that he took part in the discussions upon some of them. They were all printed in the *Proceedings* of that society, vols. vii., ix., and x., and these volumes were issued to Mr. Howitt in virtue of his membership.

Numerous articles of mine have been published by the anthropological societies of the following places:—London, Berlin, Washington, Paris, and Vienna, as well as by other societies. I presented forty separate copies (reprints) of various papers written by me on the aborigines to the public library in Melbourne in 1899. A bibliography of nearly all the articles published by me is printed in the *Journal of the Royal Society of New South Wales*, vol. xxxviii., pp. 376–381. The whole of my works are therefore within reach of any man who wishes to consult them.

As I am about to make a strong assertion, I will confine myself to five articles published in the *Proceedings* of the *Royal Society of Victoria* and twelve in the *Journal of the Anthropological Institute*. If Mr. Howitt still maintains that he has not seen these seventeen articles, I cannot believe him.

In regard to Mr. Howitt's remark that where I find "instances" in which my information agrees with his I may rest assured that my "conclusions are correct," I may briefly mention that on one occasion, in 1894, I did rely on Mr. Howitt's assertion that descent in the *Kaiabara* tribe is in the male line. From personal inquiries among the blacks in 1898 I was able to correct my former statement, and to show that descent is in the female line.² Mr. Howitt, however, repeats his former error in his book at p. 229, where he says that "descent [in the *Kaiabara*] is in the male line."

Mr. Howitt accuses me of "personal animus." There cannot be any question about the "personal animus" which prompted him to ignore my prior work, which had the effect of temporarily misleading an English reviewer. Nor can there be any mistake about the animus evinced in the statement that he has "only met with two of my papers, neither of which recommended itself to him by its accuracy." His object in both cases is manifestly to make little of my work. Even now, while he tells us that he ignored my prior work in "ignorance," he does not express any regret, but attempts to justify the course he adopted.

In the present case both authors reside in the same country and are known to each other as workers among the same tribes. I cannot help repeating that it is both "marvellous" and "wondrous" if they did not make themselves acquainted with each other's publications, especially as there were intervals of four, five, and eight years in which to do so. Mr. Howitt's experiences should be a warning to others to avoid the pitfall of claiming originality for work which has already been published some years in scientific journals of undoubted repute.

R. H. MATHEWS.

Parramatta, New South Wales.

[No further correspondence can be published on this subject.—ED. NATURE.]

¹ "Native Tribes of S.-E. Australia," p. 231.

² *Journ. Anthropol. Inst.*, xx., p. 30.

¹ "Native Tribes of Central Australia," p. 59.

² *Proc. Amer. Philos. Soc.*, Phila., xxxvii., p. 33c, with map.