slits in the large sharks are small, here they are of immense size. Their function is to allow of sufficient water to flow in and over the gills to oxygenate the fish's blood; but in Selache they serve also as supports to the strainers; and as so big a body must require a great lot of food, the in-takings and out-puttings must be many, and might account for the gradual increase in the size of these slits until they reached their present immense proportions, where they have to subserve both the functions of nutrition and circulation. The convexity of the gill-openings is towards the shark's mouth, the concavity of these fringed rays is in the same direction. The edge represented in the drawing as jagged—an appearance assumed in drying—is attached to the inner edge of the flaps covering the gill-openings, being somewhat more firmly attached towards the central portion, which in the drawing is far too cartilaginous-looking. The gills are outside the whalebone fringes. There seems little reason to doubt but that the free points of the fringes of the one row can be so erected from its gill ray edge as to bend forwards and join, and perhaps slightly interlace with those of the opposite row, and thus there would be a series of arches of whalebone protruding into the neck cavity of the fish. When these fringes are applied to the surface of the gill rays, the water could flow without resistance. The gills were quite free from parasites, in this respect differing from the gills of the Rhinodon of the Seychelles. Although this is not the place to enter into minute details, there is little doubt that Dr. Fleming is wrong in stating that the skin seems smooth when the hand is passed from the head to the tail; and yet though the scales are, as described by Dr. J. E. Gray, armed with small curved points bent in all directions, so that the skin feels rough each way, the hand can be rubbed several times more easily from head to tail than from tail to head, indicating that a larger number of the curved points are directed towards the tail.

The oil from the liver of a medium-sized Basking Shark is worth nearly 40*l.* sterling; but the difficulties and danger of capturing these sharks seem altogether to be greater than those attending the whale-fishery. The same was true at the Seychelles. Men engaged at the sperm-whale fishery off St. Denis often told me they dreaded to harpoon by mistake a Rhinodon. A whale must come up to breathe or else choke itself. But there were stories told me of how a harpooned Rhinodon, having by a lightning-like dive exhausted the supply of rope, which had been accidentally fastened to the boat, dived deeper still, and so pulled pirogue and crew to the bottom—there, in spite of the harpoon in its neck and its attendant incumbrances, it was at home for a great length of time.

ED. PERCEVAL WRIGHT

ON THE PHYSICAL EXPLANATION OF THE INEQUALITY OF THE TWO SEMI-DIURNAL OSCILLATIONS OF BAROMETRIC PRESSURE[1]

THERE are, perhaps, few phenomena in the domain of terrestrial physics which have received more attention than the diurnal variation of barometric pressure, and on the causes and explanation of which, nevertheless, there is more diversity of opinion even at the present day. Dove, Sabine, Herschel, Espy, Lamont, Kreil, Broun, and many others have in turn engaged in the discussion of this vexed problem, and at the present time Mr. Alexander Buchan is publishing an elaborate and most valuable *résumé* of the existing data in the *Transactions* of the Royal Society of Edinburgh as a preliminary to a renewed investigation.

The general features of the diurnal variation of pressure are familiar enough to every one who has ever observed the rise and fall of the barometer for a few days in India, and most other tropical countries. From about 3 or 4 in the morning the pressure increases gradually towards sunrise, then more rapidly, and culminates generally between 9 and 10 A.M. A fall then sets in, which becomes rapid during the hottest hours of the day, and the pressure reaches its minimum generally between 4 and 5 P.M. The pressure then increases till about 10 P.M., but in general does not attain the same height as at the corresponding morning hour. Lastly, a second fall brings it to a second minimum between 3 and 4 A.M., which, except on mountain peaks and at such stations as Simla and Darjiling, is, as far as my own experience goes, never so low as the afternoon minimum.[1]

Thus, then, the pressure rises and falls twice in the twenty-four hours, attaining, in general, its absolute maximum about 9 or 9.30 A.M., and its absolute minimum between 4 and 5 P.M.

This may be taken as a general description of the phenomena as exhibited in the tropics; but it presents many striking variations at different places, and at one and the same place at different times of the year. These variations affect—the hour at which the pressure attains its maximum and minimum values, the absolute amplitude of the oscillations, and lastly, their relative amplitude. It is this phenomenon—the variation in the relative amplitude of the day and night oscillations—the probable physical explanation of which I have now to bring to notice.

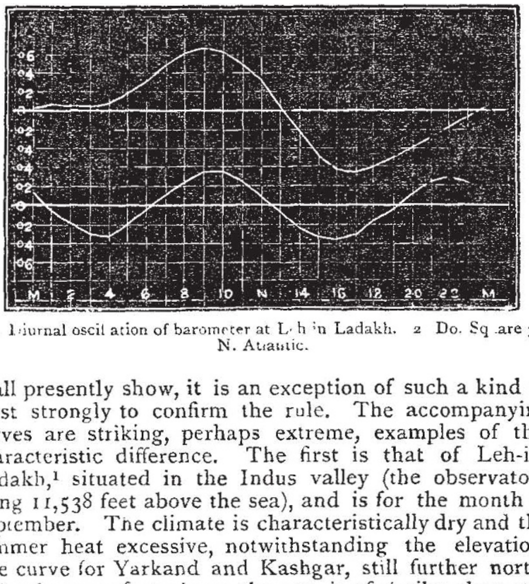
It was observed by Arago, apparently some years prior to 1841, that in Europe "the proximity of the sea has the effect of diminishing the amplitude of the interval during which the diurnal fall lasts, viz., that which occurs between 9 A.M. and 3 P.M.;" and considering the whole phenomenon as made up of a single and double oscillation, it may easily be shown that this interval is determined mainly by the relative amplitude of these two elements. The latest notice on the subject is given in the following extract from Mr. Buchan's Memoir, a copy of the first part of which (for which I am indebted to the author) has reached me only within the last week. In summing up the characteristics of the midday fall of pressure, he says:—"Whatever be the cause or causes on which the diurnal oscillations of the barometer depend, the influence of the relative distribution of land and water in determining the absolute amount of the oscillation in particular localities, as well as over extended regions, is very great. From the facts detailed above, it will be seen that this influence gives a strong local colouring to the results, particularly along the coasts, and that the same influence is extensively felt over the Channel, the Mediterranean, the Atlantic, and other sheets of water on the one hand, and on the other over the inland portions of Great Britain, Europe, and the other continents;" and farther on he adds: "While, as has been pointed out, numerous illustrations can be adduced showing a larger oscillation over the same region with a high temperature and a dry atmosphere than with a low temperature and a moist atmosphere, the small summer oscillation on the coasts of the Mediterranean and those of the Atlantic adjoining is in direct opposition to the idea that any such conclusion is general. For over those parts of the Mediterranean and Atlantic the temperature is hottest in summer and the air is driest—so dry, indeed, that no rain, or next to none, falls; and yet there the amplitude of the oscillation now contracts to its annual minimum. On the western coasts of the Atlantic, from the Bahamas northwards to Newfoundland, the temperature is at the annual maximum, but the air is not dry, being liberally supplied with moisture, and the rainfall is generous. But with these very different meteorological conditions there occurs, equally as in Southern Europe, a diminished oscillation during the summer months in the islands and near the

[1] Possibly some coasts may furnish an exception.

coasts of North America; and in the south of Europe the oscillation reaches its annual maximum just at the season when the annual minimum occurs near the sea-coasts, even although the general characteristics of the atmosphere be substantially the same in both cases."

I am not at present aware whether Mr. Buchan has been led by these observations to any definite conclusions as to the physical cause of the variation he so clearly summarises in the passages above quoted. In the part of his memoir which has reached me all theoretical discussion is deferred. But these passages afford such remarkable confirmation of an explanation at which I arrived some weeks since, on approaching the subject from an entirely different quarter, that I do not think it necessary to withhold longer the publication of my view. If Mr. Buchan's conclusions are the same as mine, the facts that I have to bring forward will seem to afford independent confirmation of that view.

Any person glancing over a series of curves illustrating the diurnal rise and fall of the barometer cannot fail to be struck with the characteristic difference of those of places with a continental and those with an insular climate. The case of the Mediterranean described by Mr. Buchan seems, perhaps, to be an exception; but, as I



1. Diurnal oscillation of barometer at Leh in Ladakh. 2 Do. Square 3 N. Atlantic.

shall presently show, it is an exception of such a kind as most strongly to confirm the rule. The accompanying curves are striking, perhaps extreme, examples of this characteristic difference. The first is that of Leh-in-Ladakh,[1] situated in the Indus valley (the observatory being 11,538 feet above the sea), and is for the month of September. The climate is characteristically dry and the summer heat excessive, notwithstanding the elevation. The curve for Yarkand and Kashgar, still further north, and only 4,000 feet above the sea, is of similar character but smaller amplitude. The second curve figured is that for the northern half of square 3, of North Atlantic, published by the London Meteorological Office. In the former the double oscillation has almost disappeared, the nocturnal fall of pressure being represented by little more than a halt for some hours between two periods of rising pressure; and nearly the whole fall of the day takes place between 9 A.M. and 5 P.M. In the case of the Atlantic curve the day and night oscillations are almost exactly alike, the night oscillation being only slightly less than that of the day. These characteristic differences are perhaps best expressed by the ratio of the constant coefficients U' and U'' in Bessell's interpolation formula—

$$x = M + U' \sin (n\theta + u') + U'' \sin (n 2\theta + u'') +, \text{\&c.},$$

since the magnitude of U' determines the inequality, and that of U'', though variable under different conditions of climate, is so to a much less extent than the former term, and chiefly depends on the latitude. The following are the values of U' and U'' in English inches, and their ratios for the mean diurnal curves of a few stations (chiefly Asiatic). The arcs u' u'' corresponding thereto are also given :—

c.	U'	u'	U''	u''	$U' : U''$
		° ′		° ′	
Yarkand (9 months) ...	·0348	4 33	·0215	161 59	1·6 : 1
Leh (September) ...	·0517	343 9	·0254	143 19	2 : 1
Lucknow (year)	·0265	341 30	·0355	168 53	0·75 : 1
Hazambagh, do.... ...	·0193	349 46	·0343	145 45	0·56 : 1
Calcutta, do.	·0265	341 24	·0391	151 7	0·68 : 1
Bombay, do.	·0179	337 17	·0385	157 13	0·46 : 1
Batavia, do....	·0240	24 7	·0369	159 34	0·65 : 1
Square 3, Atlantic, do.	·0055	354 51	·0319	159 26	0·17 : 1

As a general rule the more humid the station and the smaller the range of temperature, the smaller is the value of U', and hence it has sometimes been spoken of as the temperature element of the oscillations; the double oscillation which is superimposed on it being referred by Dove, Sabine, and Herschel to the varying tension of water vapour, by Lamont and Broun to some solar influence other than heat; and by Espy and Kreil to the oscillation of pressure produced by heat in an elastic fluid expanding and contracting under the influence of gravity. To me it seems that there can hardly be a doubt that the last explanation is the true one, and that this has not been generally recognised I attribute to the fact that the consequences of the theory as a purely physical problem have never yet been traced out and verified by such a mass of facts as Mr. Buchan is now bringing together. So long as the *whole* phenomenon is not satisfactorily accounted for, some doubt may reasonably attach to the explanation offered of one only of its elements.

My own attention was first drawn to the subject of the explanation which I am about to give by a paper of Mr. F. Chambers in the *Phil. Trans.* for 1873, in which that gentleman showed, as the result of an analysis of the diurnal variation of the winds at Bombay, that one element of this variation is a double rotation of the wind direction of such a character that the southerly components attain their maximum value at the epoch of the most rapid semi-diurnal rise of pressure, the easterly components at the epoch of maximum, the northerly with the most rapid fall, and the westerly with the epoch of minimum. On these facts Mr. Chambers based a suggested explanation of the barometric tides; regarding them as a phenomenon of static pressure; and assumed (as now appears, on insufficient grounds) that the phenomenon in the northern hemisphere is generally of the same type as at Bombay. There was indeed one feature in his explanation, which it seems difficult to reconcile with mechanical laws, since he supposed air to flow from both east and west towards a region where the pressure is rising above the mean, and by its accumulation to produce a maximum of static pressure. But apart from this, the discovery was an important one, and since it clearly showed that a regular horizontal transfer of air corresponded to the oscillations of pressure, it held out a promise that further steps in the same path might clear up what appeared to be anomalous, and possibly lead to a complete explanation of the diurnal oscillation.

Some time before this paper reached me, the Rev. M. Lafont had placed in my hands four years traces of a Secchi anemograph, erected on St. Xavier's College, Calcutta, and these having been measured off, tabulated, and reduced, I was interested to find that the diurnal wind variation at Calcutta showed the double diurnal oscillation quite as distinctly, and relatively even more prominently than that of Bombay. But one important difference presented itself. The north and south elements of the oscillation, while agreeing in epoch with those of

[1] This is computed from the hourly observations, recorded during six days, by Capt. E. Trotter, R.E., and of one day by Dr. J. Scully, together with six days' observations by the latter at the hours 4 and 10 A.M. and P.M.

Bombay, were reversed in direction and taken together with the latter, showed a tendency to a cyclonic circulation of the atmosphere around the Peninsula during falling pressure, and an anticyclonic circulation with rising pressure. Moreover, the east and west components agreed almost exactly in epoch with the north and south components, the result being a movement of air from the north-west, with falling pressure, and from the south-east with rising pressure. These facts, taken in conjunction with the positions of Bombay and Calcutta, on opposite sides of the Peninsula, seemed to point to the differential conditions of land and water being probably concerned in the phenomenon. Another and not less important fact connecting the winds with the diurnal oscillation of the barometer appeared at the same time. When the wind variation was analysed by Bessel's method, there appeared an east and west oscillation of considerable magnitude, corresponding in epoch with the barometric inequality expressed by the first periodical term of the barometric formula. This was easily distinguished from the oscillation of the sea and land winds, since the latter is nearly north and south at Calcutta. At Bombay where the sun and land-breezes are nearly east and west, such an oscillation would be undistinguishable, even if it really exists.

The east and west oscillation of diurnal period indicates an outflow of air to the eastward during the daytime, an inflow from the east during the night, and the former phase of it evidently corresponds to the hot winds of the Gangetic plain and northern India, and indeed to the day-winds of the dry months of the greater part of India. They blow towards the sea from the eastward, only in the western portion of the Dakhan, Mysore, &c. This system of day-winds consists of an outflow of air from the Peninsula towards the sea on both coasts, the westerly direction greatly predominating.

The next step in the inquiry was to ascertain what general cause would operate to produce this efflux and influx of air; and the obvious suggestion was that it must consist in the differential action of the sun's heat on dry air and water.

Let V be any volume of dry air at pressure P, and absolute temperature T, and let τ units of heat be communicated to it, raising its temperature from T to $T + t$, while the volume remains constant. The pressure will be thereby increased from P to $P + p$, wherein

$$p = P\left(\frac{T + t}{T} - 1\right) = P\frac{t}{T} \qquad (1)$$

Also

$$\tau = V\varsigma\frac{P}{P}\frac{T_0}{T}tc, \qquad (2)$$

wherein ς is the density of air at the standard pressure P and temperature T_0, and c its specific heat at constant volume, compared with water as unity.

If now the same quantity of heat τ be employed in evaporating water at temperature T (the whole being consumed as latent heat), and filling the volume of air V with vapour at pressure p', the total pressure will become $P + p'$, and

$$\tau = V\varsigma\frac{p'}{P}\frac{T_0}{T}\lambda$$

where ς is the hypothetical density of water vapour at P and T_0, and λ its latent heat at temperature T. Substituting for ς its approximate equivalent $\frac{5}{8}\varsigma$

$$\tau = V\tfrac{5}{8}\varsigma\frac{p'}{P}\frac{T^{\circ}}{T}\lambda \qquad (3)$$

and equating (2) and (3) and eliminating common factors,

$$Ptc = p'\tfrac{5}{8}\lambda$$

$$p' = P\frac{tc}{\frac{5}{8}\lambda} \qquad (4)$$

From (1) and (4)

$$p : p' = P\frac{t}{T} : P\frac{tc}{\frac{5}{8}\lambda}$$

$$p = p'\frac{\frac{5}{8}\lambda}{Tc} \qquad (5)$$

which gives the ratio of the increase of pressure produced by the same quantity of heat, employed in the one case simply in heating dry air, and in the other in charging it with vapour. At a temperature of 80° Fahr. = $T = 541$,

$$p = 7{\cdot}36\,p';$$

that is to say, when a given quantity of heat is employed in heating dry air at the temperature of 80° it raises its pressure more than seven times as much as when it simply charges it with vapour without altering the temperature. With lower values of T the difference will be still greater.

This great difference is no doubt much reduced in nature by the effects of radiation; and while some evaporation is effected on the land surface, there is some increase of temperature over the sea, but it may be expected that some part of this difference will manifest itself in the greater intensity of the forenoon pressure in the lower strata of the atmosphere on the land as compared with the sea, and in fine clear weather as compared with cloudy weather, when banks of clouds present an evaporating surface. With regard to this latter point, it has been shown by Lamont and Kreil's investigations, that between clear and cloudy days, there is a difference of this kind, and that it is manifested not only in the greater magnitude of the diurnal co-efficient U', but also, although to a much less degree, in that of the semidiurnal co-efficient U'' of the barometric formula. Further evidence of the same kind is afforded by the values of these co-efficients for the several months at Calcutta.

	U'	u'	U''	u''
		° ′		° ′
January	·0287	330 18	·0415	151 34
February	·0319	327 12	·0423	146 48
March	·0343	329 27	·0437	146 44
April	·0361	336 53	·0425	146 38
May	·0325	344 43	·0385	148 13
June	·0218	357 28	·0336	146 23
July	·0192	2 6	·0396	150 30
August	·0218	0 5	·0372	144 29
September	·0232	354 41	0400	151 25
October...	·0234	343 12	·0393	160 59
November	·0250	337 38	·0399	164 22
December	·0270	335 18	·0411	158 55

The driest months in Northern India being March and April, while July is the wettest and most cloudy.

On Espy and Kreil's hypothesis of the cause of the double oscillation, there is no apparent reason why the evening maximum, arising from contraction and dynamic pressure, should be equal to the morning maximum, which seems unquestionably due to the increased tension of the lower atmosphere in consequence of heating and the introduction of vapour; and any inequality will, of course, appear in the value of U', or of the co-efficients of other terms of odd periodicity. But the fact established by the anemometer that an outflow of air from a heated land area takes place during the day-time, at once assigns a cause for the greater part of the equality, viz., an alteration of the static pressure. This is not an overflow in the upper regions of the atmosphere, but an outflow of the lower strata, or a tendency in that direction. It does not, of course, follow that to produce a reduction in the mass of air over a continent, there should be an actual motion of the air outwards in all directions. The very small forces in action will be manifested even more in retarding in-flowing currents than in accelerating efflux; and it is only in very dry and highly-heated regions, such as India, that they produce well-marked diurnal surface winds, blowing outwards towards the sea;

winds of elastic expansion, such as are the hot winds of India and Australia; winds which are distinct from convection currents, though, it may be, coexisting with and accelerating them. The relations of these winds to the barometric tides are very marked, but it does not seem that the differences of tidal pressure would suffice to generate them, were there not a movement of the air in the same direction arising from more persistent differences of pressure. They probably also depend much on local and irregular differences of pressure.

The air thus removed in the day-time from continental areas must, of course, collect over the nearest areas of evaporation, with the effect of diminishing the mid-day fall of pressure over those tracts; and thus seems to be explained those apparent anomalies in the magnitude of the mid-day semi-oscillation of the barometer to which, in the passages quoted from Mr. Buchan's memoir, he has drawn attention, viz., in the case of the Mediterranean area and the Atlantic coast of North America.

The direction in which this movement of the air takes place will, of course, vary with the locality, but there will always be, on an average, a greater diurnal movement towards east coasts than towards those facing to the west. This may be illustrated by the case of Calcutta and Bombay, and it is more extensively illustrated by the predominant westerly direction of the land-winds of India, and the cold westerly diurnal winds[1] that blow across the high plains (17,000 to 19,000 feet) of the Changchenmo and Rupshu in Western Tibet. The reason is sufficiently obvious. As the great waves of pressure advance from east to west, the local barometric gradient of any place (in so far as it is determined by the diurnal oscillation) will be expressed by a tangent to the existing phase of the wave. During the hottest part of the day, viz., from 9 or half-past 9 to half-past 4 or 5, this gradient (which is the steepest and most prolonged of the four) inclines to the eastward, and increases the declivity towards east coasts arising from the excess of pressure over the land. In the opposite direction, viz., towards west coasts, it goes to diminish that declivity. At night the case is reversed. The west to east barometric gradient from 10 P.M. to half-past 3 or 4 A.M. is in the same direction as that tending to produce an influx of air from the sea towards the land on west coasts; this, however, is opposed to the land wind of the coast line, which is a true convection current, and arises from quite different causes; and, although traceable in the wind variation at Bombay, it there manifests itself only by decreasing the velocity of the former. There are, moreover, independent grounds for the inference that this compensating in-flow chiefly affects the higher strata of the atmosphere, while the day wind is chiefly produced in the lower and more heated strata. At Calcutta the easterly (or negative westerly) tendency of the wind at night is very prominently exhibited in the curve of diurnal variation, but although of longer duration it is at no time so intense as the westerly tendency in the early afternoon hours.

In like manner may be explained the difference of epoch of the corresponding phases of the semi-diurnal east and west variation at Calcutta and Bombay. The gradient of pressure, in so far as it depends on the semi-diurnal oscillation, will, of course, be to the west with a rising pressure, and to the east with a falling pressure, and this normal tidal gradient is affected by the small difference of amplitude over land and sea, in such manner that its changes will be accelerated as affecting east coasts, and retarded as affecting west coasts. Now if we suppose that the acceleration in the one case and the retardation in the other amount to an hour or an hour and a half, and that the interval between the change in the direction of the gradients, and their effects on the wind, as manifested by the anemometer, is also about an hour and a half, we should roughly reproduce the conditions shown to exist at Calcutta and Bombay respectively.

[1] This I state on the authority of Dr. Cayley, who assures me that on the high plains these afternoon winds are always from the west.

According to this view, the local static pressure of the atmosphere, apart from irregular movements, is shown by the height of the barometer at the hours of minimum pressure, and the difference of these expresses the weight of the atmosphere removed and restored by the oscillatory movements chiefly between land and sea.

I should add, in conclusion, that this communication is merely a *résumé* of some of the more salient topics discussed in two papers, "On the Winds of Calcutta," and "The Theory of Winds of Elastic Expansion," which will shortly be published *in extenso* elsewhere.

H. F. BLANFORD

CARBONIFEROUS LAND SHELLS

IN a recent visit to the South Loggius, in Nova Scotia, in which I was assisted in the examination of the cliff by Mr. Albert J. Hill, Manager of the Cumberland Coal Mine, we found a number of well-preserved shells of *Pupa vetusta*, in the indurated clay, filling an erect sigillaria, in a bed considerably higher than those in which the shell was previously known. It is nearly in the middle of group xxvi. of my section of the South Loggius, 222 feet above the main coal-seam, 842 feet above the bed in which the species was first recognised by Sir C. Lyell and myself, and about 2,000 feet above the lowest bed in which I have yet found it. It thus appears that this little pulmonate continued to flourish in the carboniferous swamps, after its remote ancestors had been covered with 2,000 feet of sediment, including many beds of coal, and nearly the whole thickness of the productive coal-measures. *Conulus priscus*, the only other land-snail found in this section, on the other hand occurs only, so far as known, in the lowest of the beds above-mentioned. Two other carboniferous land-shells, *Pupa vermilionensis*, Bradley, and *Dawsonella Meeki*, Bradley, have been found in the coal-field of Illinois; and it is worthy of remark that, according to Dr. P. P. Carpenter, all the four species belong to distinct generic or sub-generic forms, and that all these forms are still represented on the American Continent.

On the same visit, we were so fortunate as to find another large sigillarium stump, rich in reptilian remains, which it is hoped may, on examination, afford new species and further information on those already known.

J. W. DAWSON

THE BIRDS OF KERGUELEN'S LAND[1]

AS regards the publication of results achieved by the naturalists accompanying the recent Transit expedition, our American friends appear to be getting the start of us. While we are engaged in issuing "preliminary reports," they have already arranged and classified their collections, and are beginning to publish their discoveries. The specimens of birds obtained by Dr. Kidder, surgeon and naturalist attached to the astronomical party at Kerguelen's Land, or Desolation Island, have been placed for determination in the hands of Dr. E. Coues—one of the most competent zoologists in the United States—and the result has been the very interesting memoir now before us. We knew already that Kerguelen's Land was not an inviting place of residence for the more highly organised animals, and that few birds were to be found there. We know now what those few are, and have full particulars about most of them, their lives, and habits. According to Dr. Coues' determination, Dr.

[1] "Bulletin of the United States National Museum," No. 2. Contributions to the Natural History of Kerguelen Island, made in connection with the American Transit of Venus Expedition, 1874-75. By J. H. Kidder, M.D. I. *Ornithology*. Edited by Dr. Elliott Coues, U.S.A., 8vo. 52 pp. (Washington, 1875.)