Now let $P' \dots q'$ be made to pass through all values from which, θ' and ϕ' being suitably chosen, they can assume after encounter the given values $P \dots P + dP \dots q \dots q + dq$. The final values of θ and ϕ will vary, but all possible values of θ and ϕ must appear for some or other of the values through which $P' \dots q'$ pass, and therefore we shall by this process obtain the whole number of pairs which are in the state $P \dots q$ after encounter, without restriction of the state which they had before encounter. It will be, namely:

$$dPdQdpdq \cdot \frac{\iint F'f'dP'dQ'dp'dq'}{\iint dP'dQ'dp'dq'}$$

But the number which are in the state $P \dots q$ now is

$$dPdQdpdqFf.$$

Therefore, as the result of encounters, it is increased by an amount proportional to

$$dPdQdpdq \int \int (F'f' - Ff) dP'dQ'dp'dq'.$$

From this point, thanks to the labours of Boltzmann and Watson, the proof is easy, and I need not repeat it, that $\frac{dH}{dt}$ is negative or zero.

I have assumed condition A. I do not say that that is the only assumption that will answer the purpose. But it is sufficient. And it is, I think, the most useful assumption, because the distribution of coordinates assumed to exist is that which would tend to be produced by any disturbances acting on the system from without.

The proof in this form is not open to the objection that by reversing the velocities we can prove two mutually contradictory propositions.

Oh, that now my friend would write a book, and point out with regard to these assumptions what more is necessary, or what less sufficient. S. H. BURBURY.

Lincoln's Inn, December 5.

P.S.—Dr. Larmor describes the reverse motions as the “exceptions which do not disprove the rule.” I would apply the maxim *Exceptio probat regulam* in a slightly different sense. They are the exceptions which put the rule to the proof. They compel you to define accurately the limits within which the rule holds. When that has been done for Boltzmann's law (if it has not been done already), it will be time to consider how far the cases which fall within the law are more important than those which fall without it. S. H. B.

December 15.

THE presence of any assumption in Dr. Watson's able proof of Boltzmann's Minimum Theorem might easily be overlooked; but if Mr. Culverwell will apply his test of reversing the motions in each separate stage of the proof, he will unearth the assumption at once. On the top of p. 43 Dr. Watson says:

“And therefore the expression

$$FfdP_2 \dots dq_{n-1} q_n$$

is the number of pairs of molecules, one from each of these sets, passing from the state $P, P+dP \dots q, q+dq$ to the state $P', P'+dP' \dots q', q'+dq'$ per unit of time, where q_n is put equal to 0 in f .”

Now let the motion of every molecule be reversed as Mr. Culverwell suggests. It will be convenient to speak of the two states as the *unaccented* and *accented* states, and we shall thus have the assumption that the expression

$$FfdP_1 \dots dq_{n-1} q_n$$

(which is also equal to

$$FfdP_1' \dots dq_{n-1}' q_n' \text{ but not to } F'f'dP_1' \dots dq_{n-1}' q_n')$$

shall represent the number of pairs of molecules passing back from the *accented* to the *unaccented* state, and this number will depend on F and f , the frequencies of distribution which the molecules are about to have after the collisions have taken place.

If this assumption be made we doubtless shall have a case in which H tends to a maximum instead of a minimum, and if Mr. Culverwell endows his molecules with the power of forethought and the prediction regarding their future state necessary to enable them to regulate their movements according to this suppositious law, then Dr. Watson's proof, and indeed

any proof, will necessarily fall to the ground. If however the motions of the molecules are allowed to take their own natural course, and nothing special is known about them, the only reasonable assumption to make is that the number of pairs passing from the accented to the unaccented state per unit time is

$$F'f'dP_1' \dots dq_{n-1}' q_n'$$

and this assumption is actually made by Dr. Watson in the next few lines of his proof that H tends to a minimum.

What Mr. Culverwell's objection shows, then, is that it is possible to conceive the molecules of a gas so projected that they would not tend to assume the Boltzmann-Maxwell distribution.

But practically it would be impossible to project the molecules in their reversed motions with sufficient accuracy to enable them to retrace their steps for more than a very few collisions, just as, if we try placing a number of pool balls in a straight line on a billiard table at distances of a foot or two apart, we find it impossible to project the first ball with sufficient accuracy for each ball to strike the next in front all down the line if there are many balls.

The question of the choice of coordinates has been so fully dealt with by Dr. Watson that I need say nothing more. However, if Mr. Culverwell prefers, he may transform from Dr. Watson's $Q_1 \dots q_n$ to any other variables defining the position of the pair of molecules, provided that he works with the corresponding generalised momenta instead of $P_1 \dots p_n$, and he will have no difficulty in choosing one of his new variables to be such that it vanishes at an encounter.

I think Lorenz's paper (“Sitzungsberichte der Wiener Akademie,” 1887, p. 115) affords the fullest account of the assumptions underlying the proof of the Minimum Theorem.

Cambridge, December 5.

G. H. BRYAN.

Science and History.

I SEE by your review of the *National* in the last number of NATURE, p. 162, that Prof. G. W. Prothero, in his “Address on History,” takes occasion to notice Buckle's “History of Civilisation.” “Buckle,” he says, “in illustrating his theory that national character depends largely upon food, attributes the weakness of the Hindoos to an almost exclusive diet of rice. A striking but misleading generalisation, for, as Sir H. Maine has pointed out, the great majority of Hindoos never eat rice at all.” Buckle, however, never said anything of the kind; and since no author wrote more clearly than he did, it is evident that the Professor, like many before him, has not taken this extract at first hand.

What Buckle did say was: that rice, millet, or whatever the Hindoos fed on, was grown with little trouble and in abundance; that the climate made clothes superfluous; that living was consequently cheap, and that hence the population increased beyond the demand for labour; labour was ill-rewarded, and the population became practically enslaved. I put the argument very shortly and inadequately, for any one may see it fully set forth in the “History of Civilisation,” 1858, vol. i. pp. 63-74.

Sir H. Maine utterly failed to perceive that whatever might have been the food that the Hindoos lived upon, it made no difference to the argument provided that that food was cheap. He was further wrong in his statement that the Hindoos did not feed on rice, as it used to be a far more usual article of diet than in later times; but his worst mistake was to limit the argument to the people of India, who were only one people, out of many, used to illustrate the point.

ALFRED H. HUTH.

London, December 18.

Geometry in Schools.

AS a mathematical teacher of long experience, I wish to state that I thoroughly agree with Prof. Henrici that experimental geometry should be taught *antecedently* to and *concurrently* with a rigorous deductive course.

Teachers who have to introduce young students to the study of deductive geometry (*to begin Euclid*, as it is called) are confronted with two difficulties. Their pupils in many cases (1) have never been seriously taught to reason about anything; (2) have no stock of geometrical ideas to reason about. The attempts made in kindergartens to give sound notions of form

by means of models, patterns, &c., are excellent as far as they go, but there seems to be great need of a systematic course specially designed to lead gradually up to the study of deductive geometry. The following books will be found useful by teachers who care to give the experimental method a trial: "Paper Folding," by T. Sundara Row (Addison and Co., Madras); "Inventional Geometry," by W. G. Spencer (Williams and Norgate); "Experimental Geometry," by Paul Bert (Cassell and Co.); "Natural Geometry," by A. Mault (Macmillan and Co.); "Geometrical Drawing," by A. J. Pressland (Rivington, Percival, and Co.). To these may be added some older ones, which may occasionally be picked up second-hand: Scott Russell's "Geometry in Modern Life," Dupin's "Mathematics," and "Conversations on Geometry" (Anon.).

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LILIENTHAL'S EXPERIMENTS IN FLYING.

IN a previous article in NATURE (vol. xlix. p. 157) we had occasion to refer to the very interesting experiments which were being carried out by Herr Otto Lilienthal with regard to the possibility of human beings being able to acquire the art of flying through the air, more or less, in the fashion of birds.

These investigations in aerial navigation are conspicuous from all other attempts of the present day, by their great difference in the method of procedure adopted. The principle of Maxim's machine, for instance, is to construct an apparatus to navigate the air by itself, carrying one or more passengers. Every movement of the machine, however, is left to the apparatus itself, and to battle with the difficulty of sustaining its own equilibrium the mechanism must necessarily be most complicated.

Lilienthal depends for the success of his apparatus on himself, trusting to his powers of *instinct* to keep his equilibrium by corresponding movements of his centre of gravity. Man in this case is the main flyer, the apparatus being only an adjunct, and it is from the ability of the former that he expects to obtain positive results. His apparatus is simple, cheap, and easily constructed; these are great points, as experiments can be carried on, even at the expense of the loss of a few machines.

The whole success of aerial flying can be summed up in the word *equilibrium*, and it is here that the difficulty lies. Given a perfectly quiet or very nearly still air, there is no doubt that machines can be constructed so as to soar and travel through the air. This state of atmosphere is very rare; but, on the other hand, there are all sorts of disturbances, currents, and wave-motions which render aerial navigation a far greater difficulty than is usually imagined.

One often envies a bird which, with perfect ease, soars above us; but it must be recollected that it is endowed with a delicate system of nerves which are always on the alert, and answer to any call made on them to sustain equilibrium. These movements are made quite unconsciously, and with the loss of the minimum amount of energy. To construct an apparatus that would accomplish this in an efficient manner would be simply impossible; but there seems no reason why man should not approximate to it to a certain extent by the help of an appropriate framework. With perseverance and many trials he should be able to master at least some of the rudiments, and eventually make short flights.

For this reason Herr Lilienthal's experiments must be looked upon as yet only as first attempts, and consequently as experiments pure and simple, and experience only will show how far they can be successfully brought. Falls must be expected in the preliminary trials until the operator becomes accustomed to the many new conditions which make themselves apparent at every step, before they can be mastered instinctively. Similar difficulties have to be contended with when learn-

ing to ride a bicycle. The beginner is at first unable to keep his equilibrium, and so wobbles here and there, with the loss of much power, until he eventually finds himself hugging the earth. This is simply because he is doing something unusual, and is not accustomed to the new conditions. An adept rider, on the other hand, never thinks of the possibility of falling, and quite unconsciously keeps his equilibrium without any exertion or

FIG. 1.

loss of power on his part. So it is with this new sailing machine, and it is only by practice that success can be attained.

To commence operations the simplest apparatus must be used, and the easiest steps attempted. This is the way Herr Lilienthal began. In his first experiments, with the help of his wing-shaped framework, he made flights from elevated points in calm weather, the lengths

FIG. 2.

of these flights increasing as he gained in experience. Sometimes as many as 500 metres were covered in one bound under satisfactory conditions.

In his more recent experiments he has been making considerable progress in developing this mode of sailing. Two objects have been kept well in mind: the first, to accomplish that method of sailing which is adopted by birds which spend hours in the air at a time without ever