

Clerk-Maxwell's Kinetic Theory of Gases

YOUR correspondent, Mr. Guthrie, has pointed out an, at first sight, very obvious and very serious objection to my kinetic theory of a vertical column of gas. According to that theory, a vertical column of gas acted on by gravity would be in thermal equilibrium if it were at a uniform temperature throughout, that is to say, if the mean energy of the molecules were the same at all heights. But if this were the case the molecules in their free paths would be gaining energy if descending, and losing energy if ascending. Hence, Mr. Guthrie argues, at any horizontal section of the column a descending molecule would carry more energy down with it than an ascending molecule would bring up, and since as many molecules descend as ascend through the section, there would on the whole be a transfer of energy, that is, of heat, downwards; and this would be the case unless the energy were so distributed that a molecule in any part of its course finds itself, on an average, among molecules of the same energy as its own. An argument of the same kind, which occurred to me in 1866, nearly upset my belief in calculation, and it was some time before I discovered the weak point in it.

The argument assumes that, of the molecules which have encounters in a given stratum, those projected upwards have the same mean energy as those projected downwards. This, however, is not the case, for since the density is greater below than above, a greater number of molecules come from below than from above to strike those in the stratum, and therefore a greater number are projected from the stratum downwards than upwards. Hence since the total momentum of the molecules temporarily occupying the stratum remains zero (because, as a whole, it is at rest), the smaller number of molecules projected upwards must have a greater initial velocity than the larger number projected downwards. This much we may gather from general reasoning. It is not quite so easy, without calculation, to show that this difference between the molecules projected upwards and downwards from the same stratum exactly counteracts the tendency to a downward transmission of energy pointed out by Mr. Guthrie. The difficulty lies chiefly in forming exact expressions for the state of the molecules which instantaneously occupy a given stratum in terms of their state when projected from the various strata in which they had their last encounters. In my paper in the *Philosophical Transactions*, for 1867, on the "Dynamical Theory of Gases," I have entirely avoided these difficulties by expressing everything in terms of what passes through the boundary of an element, and what exists or takes place inside it. By this method, which I have lately carefully verified and considerably simplified, Mr. Guthrie's argument is passed by without ever becoming visible. It is well, however, that he has directed attention to it, and challenged the defenders of the kinetic theory to clear up their ideas of the result of those encounters which take place in a given stratum. J. CLERK MAXWELL

Additional Remarks on Abiogenesis

SINCE my communication in *NATURE*, March 20, a further investigation of the subject has shown me that the experiments there recorded do not yet fully prove the reality of abiogenesis. My argumentation based on those experiments is liable to the following objection:—

The principal experiment (water, potassium-nitrate, magnesium-sulphate, calcium-phosphate, glucose, and peptone) is conducted in a neutral solution. In the control-experiments neutral ammonium-tartrate is used as nutritious substance for the supposed germs. But this salt disassociates by boiling, loses ammonia, and the reaction becomes acid. When, therefore, Bacteria appear in the principal experiment and not in the control-experiments, this result can be explained by admitting that the germs resist a temperature of 100° in a neutral liquid, but are killed by the same temperature in an acid solution. This explanation agrees very satisfactorily with the fact proved by Pasteur, that an acid reaction is much more deleterious to living germs than a neutral reaction at the same temperature.

This objection is very rational, but it does not throw over my conclusion respecting the reality of abiogenesis, for the following reasons:—

It is now obvious that in the control-experiments ammonium-tartrate cannot be used, a nitrogenous body must be sought, not too complex, that remains neutral by 100°. For this end I have found urea to answer well. Pure urea is perfectly fit to furnish nitrogen to the Bacteria, but not to furnish them their carbon. Bacteria sown in a solution of urea and mineral salts do

not develop themselves, but when sugar is added their growth goes forth rapidly. The following solution—100 c.c. water, 0.2 grms. potassium-nitrate, 0.2 grms. magnesium-sulphate, 0.04 grms. calcium-phosphate, 1 gm. glucose, 0.5 gm. urea, is eminently fit for the development of Bacteria. Also a solution that contains, instead of the sugar and the urea, 0.5 gm. peptone.

These solutions were now used in the control-experiments.

For instance:

a. Principal experiment. 100 c.c. salt-solution,* 2 grms. glucose, 0.3 grms. peptone boiled and treated in the ordinary manner (See *NATURE*, vol. vii. p. 380). On the third day the liquid contains countless swarms of Bacteria.

b. Control experiment. 100 c.c. salt solution, 1 gm. glucose, 0.5 gm. urea, boiled exact. No Bacteria appear; on the eighth day the liquid is perfectly clear.

c. Control experiment. 100 c.c. salt solution, 0.5 gm. peptone, boiled, &c. On the eighth day complete absence of Bacteria.

In each of these experiments the reaction is neutral. They are therefore fully comparable. The experiments b and c prove, moreover, that the closing tiles exclude completely the atmospheric germs, a fact that was also proved by direct experiments, wherein the solutions b and c were used and dust strewn on the closing tile in the manner formerly described.

But is it not possible to generate Bacteria in a liquid which has been boiled when acid?

To elucidate this point, the above-named solution a was rendered acid (2–4 c.c. of a 1 per cent. solution to 100 c.c.) and treated as usual. No Bacteria appeared, whether the liquid was, after boiling, neutralised with soda or not.

But this negative result is easily conceivable; for the acid alters essentially the calcium-phosphate, changes CaHPO_4 into $\text{Ca}_3\text{H}_2\text{P}_2\text{O}_8$. And that this alteration is not without influence, is rendered probable by the fact, which I have recorded in the *Maandblad voor Natuurwetenschappen*, No. 7 (April 23, 1873), namely, when in the principal experiment instead of CaHPO_4 is used a mixture of $\text{Ca}_3\text{P}_2\text{O}_8$ and $\text{Ca}_2\text{H}_4\text{P}_2\text{O}_8$ the result (the genesis of Bacteria) is much less constant. The neutral calcium-phosphate by boiling with water breaks up in the basic and the acid salt, but this division must take place in the presence of sugar and peptone.

On the other hand, the acid modifies the peptone. This is easily demonstrated by comparing, in the polariscope, the rotating power of a neutral peptone-solution with the power of the same solution. After boiling with acid a notable difference is observed.

The acid can, nevertheless, be employed with the following modification:—In 100 c.c. water are dissolved 0.2 gm. potassium-nitrate, 0.2 gm. magnesium-sulphate, and 2 grms. glucose; 2 c.c. of a 1 per cent. solution of tartaric acid are added, so that the liquid has a strong acid reaction. It is then boiled for ten minutes. Then with a red hot platinum spatule a little soda is taken from a hot crucible and thrown in the flask. The quantity of soda required is approximately ascertained by a preliminary trial. Care should be taken not to render the liquid alkaline. Then 0.05 gm. calcium phosphate and 0.3 gm. peptone are added together, and the boiling continued for ten minutes. The flask is closed as usual, and deposited in the hatching-bath. Three days after, it swarms with Bacteria.

When, instead of calcium-phosphate and peptone, are added 0.05 gm. calcium-phosphate and 0.5 gm. urea, nothing appears; and the result is equally negative when the following solution is taken:—100 c.c. water, 0.2 gm. potassium-nitrate, 0.2 gm. magnesium-sulphate, 0.05 gm. calcium-phosphate, 1 gm. potassium-natrium-tartrate, 0.3 gm. peptone. In this latter case no acid is used. The addition of the tartrate is made to have a sufficient quantity of carbon in the liquid. These control experiments prove that none of the employed materials, neither the glucose, nor the calcium-phosphate, nor the peptone did introduce germs.

By these experiments the above-stated objection is, in my opinion, satisfactorily refuted.

In concluding these remarks, I must mention an important fact. For the above-described experiments, I employed mostly the ordinary glucose, an amorphous, yellowish white mass, not chemically pure. By crystallisation from strong alcohol, I purified this sugar. In three different preparations I obtained thus three samples of perfectly white more or less pure glucose. One

* Composed of 1 gm. potassium-nitrate, 1 gm. magnesium sulphate, 0.2 gm. neutral calcium-phosphate in 503 c.c. water.