widely accepted, as if acids could have the power of liberating trypsin from trypsinogen is not correct; on the contrary, they prevent this liberation.

2) That HEIDENHAIN came to this conclusion must be ascribed to the accidental occurrence, that instead of using the pressed out juice or watery extracts of the pancreas, he had taken glycerin-extracts from the gland. The favorable action caused by the presence of acetic acid in his experiments and which I have been able to confirm, is to be ascribed to the fact that acetic acid decreases the injurious action of the glycerin on the liberation.

3) As it has now been proved that the gastric juice does in no wise further the liberation of trypsin, but rather opposes it, we may therefore draw the conclusion, *that in this process of liberation all the work falls to the intestinal juice; a fact still increasing in importance where the investigations of* POPIELSKI *have proved, that no free trypsin whatever appears in the pancreassecreta, but that it is only there in the shape of trypsinogen.*

Having arrived at the end of my communication, I beg Prof. HAMBURGER to accept my warm thanks for the opportunity afforded to me to make these researches and also for the useful hints kindly given to me.

Physiological laboratory of the State University at Groningen. May 1903.

Physics. — *"Some remarks on the reversibility of molecular motions."* By Dr. A. PANNEKOEK. (Communicated by Prof. H. A. LORENTZ).

1. The following considerations deal chiefly with the question whether a mechanical explanation of nature is possible. Mechanics treat the motion of discrete particles or of continuous masses; now the question may be raised, whether all natural phenomena can be explained by means of such a motion. In other words, it is the question, whether or no we know particular properties of these phenomena, which exclude the possibility of a mechanical explanation of general application. A particular property which seems to do so, is the irreversibility of the natural phenomena, the change in a definite direction. When investigating whether this is really the case, we need only consider the simplest form in which the irreversibility of natural phenomena occurs: the second law of the mechanical theory of heat.

Poincaré says about this in his "Thermodynamique", that it entirely excludes the possibility of a mechanical explanation of the universe.

The motions of which mechanics treat, are all reversible; there occur only forces which depend on place, so relations between the 0^{th} and the 2^{nd} derivative according to time; if the sign of t is reversed, these equations retain their validity. It is true that in mechanics also cases are treated in which the first derivative according to t occurs in the equations (friction); we are, however, justified in calling these cases not purely mechanic, because in them heat is produced, so that in a complete explanation we must introduce considerations (thermodynamic ones), which we are just trying to solve in purely mechanic ones. It is therefore desirable to call only those cases *purely* mechanic which are *reversible*; these only are conservative. In the above-mentioned not purely mechanic cases there is dissipation of energy, so that, the law for the conservation of energy being a general law of nature, a mechanical description of them is not complete. The kinetic theory of gases shows us that this description only mentions the visible motions in the system, but not the molecular motion, which is required to make the description complete. The word mechanic, occurring in the question raised in the beginning must therefore be interpreted in such a way that we consider only cases of conservative systems as purely mechanic.

The question whether the irreversibility of the natural phenomena *decisively excludes* a mechanical explanation, must be answered in the negative, when we succeed in giving a mechanical description of one typical and simple irreversible process, or in other words, if we can point out in a certain case that a process consisting of purely mechanic, so reversible motions, is irreversible. We must then at the same time get an insight into the question, how it is in general possible, that a process in its general character can be so different from that of the partial processes of which it consists.

2. Boltzmann has shown that we meet with such a case, though an abstract one, when we have a perfect gas, consisting of perfectly elastic spheres, between which no other forces act than those eventuating in collisions between two particles. He proved that the function $H = \int f \log f \, d\omega$, in which $f \, d\omega$ is the number of the molecules whose points of velocity lie in the volume element $d\omega$ of the velocity diagram [1]), can only be made smaller, never greater by the collisions.

[1]) The "velocity diagram" is obtained by representing the velocity of every molecule by a vector drawn from a fixed point. This vector ends in the "point of velocity" of this molecule.

As this function taken with the reversed sign, expresses at the same time the logarithm of the "probability" of a certain distribution of the velocities, BOLTZMANN expresses his result also under the following form: the effect of the collisions is that a gas always gets from a more improbable to a more probable condition.

Here we have therefore a process, consisting of purely mechanic partial processes, which shows change in one direction only. That however BOLTZMANN's considerations have not yet led to a perfectly satisfactory insight, and that this contrast is felt as a contradiction, is proved by the objections and doubts, which have been adduced against these considerations without refuting them. Let us assume a fictitious system in which at the moment t_0 all the places are the same, but all the velocities exactly the opposite of those of the real system. The two systems can represent a gas in exactly the same way, there being no possibility of seeing which is the real and which the fictitious one. Yet the fictitious one will successively pass through all the conditions through which the natural one has passed before the time t_0, in reverse order; all the collisions take place in opposite direction, and the system gets from a "more probable" to a "more improbable" condition.

BOLTZMANN denies that this involves a contradiction, for the fictitious system is *"molecular-geördnet"*. That this remark does not solve the difficulty (BRILLOUIN, among others, expressed doubts as to this in a note in the French translation of BOLTZMANN's Vorlesungen) must be ascribed to the fact, that the ideas "ordened" and "unordened" for molecular motions are difficult to define sharply. Sometimes ordened is interpreted as if it meant that in the fictitious system to every molecule its future course is accurately prescribed. This however is not satisfactory. If we know at the moment t_0 the places and velocities of the natural system, we are enabled to determine beforehand, so to prescribe, the future course for the natural and for the fictitious system and for both in exactly the same way.

The fact that the motions in the fictitious system are ordened can be better pointed out by means of the following consideration. If we take two groups of molecules with the points of velocity P_1 and P_1', which come into collision, then after the collision the points of velocity Q_1 and Q_1', R_1 and R_1' etc., will all lie on a sphere of which the line P_1P_1' is a diameter. The places of $Q, R, \ldots.$ on the sphere depend on the direction of the planes of collision $A, B, \ldots.;$ to every plane of collision belongs a definite place of the points of velocity and the latter are scattered all over the sphere, because the former have all kinds of directions. If we now take the reversed, fictitious system, all these points of velocity come back in P_1P_1', because *definite* planes of collision A, ... belong to every pair of points of velocity Q_1Q_1' .... The fictitious system, therefore, is subjected to the *condition*, that molecules with definite points of velocity do not collide according to arbitrarily chosen planes or to planes whose direction is determined by chance, but according to planes which are entirely *determined* by the position of these points of velocity. This condition may be called an ordening of the motions.

We must, however, add another remark. In the natural system we had not only points of velocity in P_1P_1', but also at the ends of the other diameters of the sphere P_2P_2', P_3P_3' .... etc. and these *too can reach the same points Q_1Q_1' as P_1P_1', if only the planes of* collision have every time the required direction different from A. Of all the points of velocity and planes of collision we have just now chosen and considered separately all those which in the natural system lie before, in the fictitious system after the collisions in P_1P_1'. We might, however, just as well have chosen and considered separately those which in the natural system lie after, in the fictitious system before the collision in Q_1Q_1'; in this case we might have been inclined, to call the fictitious system unordened, and the natural system ordened. The difference between the two would of course become clear, when we paid attention to the *number* of collisions which cause the points of velocity to pass from P_1P_1' to Q_1Q_1', R_1R_1' etc. or vice versa. In *reality the collisions in the* natural system have a scattering effect, through which the distribution of the points of velocity over the sphere is more regular and arbitrary after impact than before. In this respect there is a real difference between the natural and the fictitious system, that in the former the distribution before the collision is more irregular, less accidental. The difference between being ordened and unordened in the molecular motions in the two systems appears here *as a difference in the degree of the ordening.*

It seems to me that though we cannot bring forward conclusive objections against the denomination used by BOLTZMANN, yet further considerations which throw some light on these phenomena, might be of some value.

3. The ordening of the motions, in which the difference between the natural and the fictitious system consists, can only be clear, when, as in the kinetic theory of gases, we examine larger masses and mean values, in which the coordinates and velocities are considered as fluently varying quantities. When we take the particles separately, in which the coordinates and velocities are perfectly defined, the

difference between a natural and a fictitious system does not appear, and the process can only be taken as perfectly reversible.

The result of each of the steps of which the whole process is built up (free path + collision), is determined 1st by the coordinates and velocities, 2nd by the direction of the normal to the collision plane. In the statistical method of treatment of the kinetic theory of gases the latter is considered as an independent datum, which therefore is thought to be defined by chance; we may then give it different values, which are distributed according to chance, i. e. regularly, and in this way the scattering, regulating effect of the collisions appears, which is the cause of the irreversibility of the process. In the purely mechanic conception, in which we must take the condition of every separate particle as rigorously defined, the direction of the normal is no independent datum; in reality this direction is accurately defined by the coordinates and the velocities of the colliding particles. Here the result is therefore determined by the coordinates and the velocities only and according to this way of considering the question, the process must be considered to be reversible.

The question how it is possible that a process may be considered in two ways, so totally different comes therefore to the same as the question, how quantities which in reality are rigorously determined by other quantities, may yet be considered to be independent and determined by chance.

We shall find the answer to this question in the fact, that very small variations in the coordinates and velocities bring about considerable variations in the direction of the normal. If we determine the directions by means of the points in which they cut a spherical surface described with a radius equal to the mean free path, the velocities being measured by the path covered in the mean time interval between two collisions, and if we call the ratio between the radius of a molecule and the mean free path a small quantity of the first order, then we may formulate this proposition more precisely as follows: variations of a given order of smallness in the coordinates and the velocities bring about variations in the direction of the normal which are of one order lower; variations in the direction of the normal give rise to variations of the same order in the coordinates and the velocities after impact.

If we ascribe to the coordinates and the velocities of two colliding molecules values $x_1\ y_1\ z_1\ x_2\ y_2\ z_2\ u_1\ v_1\ w_1\ u_2\ v_2\ w_2$ which are rigorously determined, then the direction of the normal $\lambda\ \mu\ \nu$ is also rigorously determined. If however we mean by these 12 data that these quantities lie between x_1 and $x_1 + dx_1$ etc. . . . w_2 and $w_2 + dw_2$, i. e. that the condition is included in a twelve-dimensional volume element *of the first order*, then λ, μ and ν are left undefined. This way of proceeding is that of the kinetic theory of gases in which we are therefore justified in considering the normal to the tangent plane of two colliding molecules to be determined by chance.

If we wish to know this direction accurate to the first order, then the 12 coordinates and velocities must be known to the second order. If within this volume element we determine the place by means of new coordinates $x_1' y_1' z_1' \ldots v_2' w_2'$, (we might call them coordinates of the 2nd class) which vary within that element over a finite region, e. g. from 0 to 1, then the direction $\lambda \mu \nu$ is a function of these coordinates of the second class, and they determine the 12 coordinates and velocities after impact also to the first order.

Every collision brings about a lowering of the order of determination of the coordinates and the velocities; every collision causes a scattering by which the condition of the system becomes one order less determined. In order to know the condition (the coordinates and the velocities) after n collisions (at least accurate to quantities of the first order) we must know the initial values of the coordinates and the velocities accurate to the $(n + 1)^{th}$ order. The longer the period is for which we want to predict the motion, the higher is the order which is required for our knowledge at this instant. The limit is here the pure mechanic conception, according to which the state is determined for ever, because the data are determined with absolute accuracy.

BOLTZMANN's observation, that a system, whose motion is reversed really proceeds from a more probable condition to a less probable one, namely to that from which the natural system started, and that afterwards conditions are reached, which show again an increasing probability, includes the assumption, that in the initial state the coordinates and the velocities were determined to the $(2n + 1)^{th}$ order, so that the reverse motion brings the system after n collisions back to the initial volume element of the first order; afterwards the direction of the normal is no longer determined, and the further process must be investigated according to the rules of the calculus of probabilities. The condition whose validity is required for the proof of the H-theorem, is not satisfied during the whole backward course of the process; it is here therefore impossible to decide anything as to the decrease or increase of H. As soon as the initial state is again reached the direction of the normal ceases to be determined, and the required condition is satisfied. From the further course we may therefore predict with certainty, that H must decrease.

The observation may here be inserted, that we speak of chance in nature, when small variations in the initial data occasion considerable variations in the final elements, because we cannot observe those small variations. Cyclic motions for instance will also always give rise to such cases.

For the special case considered here the result we have found may be formulated as follows: when in a purely mechanic, reversible process which occurs a great many times in the same way, events occur in which small variations in the initial data occasion considerable variations in the final state, then the total process gets the properties of an irreversible process.

Botany. — *"On a Sclerotinia hitherto unknown and injurious to the cultivation of tobacco."* (*Sclerotinia Nicotianae* OUD. et KONING).

(By Prof. C. A. J. A. OUDEMANS and Mr. C. J. KONING).

The following communication contains five paragraphs.

Par. I gives an account of a visit to the tobaccofields in the Veluwe and Betuwe, in the autumn of 1902, about the time that the tobaccoleaves begin to be gathered.

Par. II contains an investigation of the disease which had attacked the plants, evidently a fungus, which had long been known as "Rot", but the nature of which had not yet been cleared up.

Par. III gives a summary of the experiments made with the Sclerotia of the fungus.

Par. IV deals with the anatomy of the Sclerotia and the *Sclerotinia* produced from them.

Par. V contains the result of some biochemical investigations.

Par. VI gives a few hints, the application of which may prevent or reduce the damage caused by *Sclerotinia Nicotianae.*

1. A VISIT TO THE TOBACCOFIELDS.

In order to study more closely the origin of the well-known patches and specks on dried tobaccoleaves, one of us repeatedly visited the tobaccofields in the Veluwe and Betuwe in September 1902. These visits repaid the trouble very well indeed, as they gave an opportunity of becoming acquainted with an evil which caused much damage, had not yet been clearly defined and so deserved a closer study.

In these visits one was first of all struck by the fact that the very extensive fields under cultivation were divided into smaller square