



# XVII. Application of the kinetic theory of gases to gravitation

S. Tolver Preston

To cite this article: S. Tolver Preston (1878) XVII. Application of the kinetic theory of gases to gravitation , Philosophical Magazine Series 5, 5:29, 117-127, DOI: [10.1080/14786447808639397](https://doi.org/10.1080/14786447808639397)

To link to this article: <http://dx.doi.org/10.1080/14786447808639397>



Published online: 13 May 2009.



Submit your article to this journal [↗](#)



Article views: 4



View related articles [↗](#)

so (6) becomes

$$\frac{\sinh qz}{h'} = \frac{q}{h} \cdot \frac{\{(T_0 + T_3) \cosh pl_1 - (T_1 + T_2) \cosh pl_0\} \{-(T_1 - T_2) \cosh pl_0\} \{(T_0 - T_3) \cosh pl_1\}}{(T_0 T_2 - T_1 T_3) \sinh pl}, \quad (16)$$

which is a form convenient for calculation.

9. So also for the second method (§ 5), we can write down the value of the quantities occurring in the right hand of (10),

$$\frac{T' - T}{\frac{dT}{dx}} = \frac{(T_1 - T_2) \sinh pl_0 - (T_0 - T_3) \sinh pl_1}{p(T_0 \cosh pl_1 - T_1 \cosh pl_0)}; \quad (17)$$

and similarly for the small  $t$ 's, of which a set  $t_0, t_1, t_2, t_3$  have been observed. In this case there is no loss of heat in passing through the crystal; so we ought to have

$$\frac{dT}{dx} = \frac{dT'}{dx},$$

which gives the condition

$$\frac{T_1 + T_2}{T_0 + T_3} = \frac{\cosh pl_1}{\cosh pl_0}; \quad \dots \dots \dots (18)$$

and unless this condition is satisfied there is some error in the experiment, and it is useless to proceed.

I have to express my thanks to my brother, Mr. Alfred Lodge, of St. John's College, Oxford, for several suggestions in the writing out of the above and for some improvements in the notation.

In the second part of this communication some practical details will be given, together with the results of some trials of the method now going to be made.

University College, London.

## XVII. Application of the Kinetic Theory of Gases to Gravitation. By S. TOLVER PRESTON\*.

### No. III.†

1. **I**N the last Number of the Philosophical Magazine is a short paper by Mr. James Croll on Le Sage's Theory of Gravitation, in which he alludes to a difficulty that has

\* Communicated by the Author.

† For two preceding parts, see Philosophical Magazine, September and November 1877 (under title "On some Dynamical Conditions applicable to Le Sage's Theory of Gravitation").

presented itself to him after reading my two former papers on this subject. As any theory that makes a pretention to truth ought to court every criticism, I am glad to notice here the difficulty alluded to, at the same time availing myself of the opportunity to touch upon certain other points that would appear to want a little further elucidation.

2. The point in question is, that, since gravity is proportional to mass, it is admittedly necessary to assume that the total volume of free space in a substance must be great compared with the total volume of matter contained in the molecules of the substance (in order that the medium producing gravity may be able to penetrate the substance and act upon the molecules in the interior). Mr. Croll finds a difficulty in reconciling this assumption with some deductions regarding molecules by Sir William Thomson, in a paper published in 'Nature,' vol. i. (p. 551). Now I think it may be shown clearly here (and that this will also be apparent to Mr. Croll on referring more minutely to the wording of the above paper) that this paper was not intended strictly to give molecular *dimensions*, but rather molecular *distances* (from centre to centre), or number of molecules in unit of volume. It is true that an estimate of molecular dimensions is given on the special assumption that the radius of a gaseous molecule is equal to "half the average shortest distance reached in a vast number of collisions." Whether this is the actual radius, therefore, depends evidently on whether the two molecules come into *contact* at collision or not. This might not be; and if not, the radius might be smaller. Thus it is at least conceivable that a layer of a medium may exist between two vibrating approximated molecules, much as a drop of water floats on a film of air. I do not wish to insist upon this comparison; but no one will, I think, consider that it is *necessary* that molecules should come into contact; and if not, it is impossible to measure their dimensions, but only their sphere of activity. This, therefore, would remove the difficulty; but I do not wish to hold necessarily to this explanation, as it appears to me that there are some grounds for supposing that molecules do come into contact.

3. The explanation I have to bring forward is of another character. *The interstices are in the molecules themselves.* This explanation was also, I believe, suggested by Le Sage himself. The old notion of a molecule being a hard spherical mass certainly appears rather crude. In view of the numbers of different capacities for vibration possessed by a molecule, as proved by the spectroscope, it appears a necessary deduction that a molecule must be of a complex structure. Interstices would make it complex. In ordinary architecture we

do not observe a solid block structure, if I may so express it, but a more or less *open* structure as consistent with lightness combined with elasticity. So molecular architecture (as size is only *relative*, and *principles* apply everywhere the same) may be of an *open* structure, as consistent with elasticity. This open structure, involving various separated parts, would give the molecule the faculty of taking up various vibrations, as it is known to be capable of doing.

4. Admitting, therefore, molecules to possess an open structure, the passage of the gravific medium through the molecules of matter might be compared (merely for a simile) to the passage of a stream of air through a scaffolding, the air passing in great part through, but exercising a gentle pressure against the solid parts of the scaffolding. So, in analogy, with the gravific medium; or by this open structure of molecules we have all the conditions for the pressure termed "gravity," together with the permeability essential to make gravity proportional to mass. [We make no postulate as to the *form* of open structure.]

5. One point may be noticed here in connexion with the inference that the molecules of solids are in *contact*. The old postulate of perfectly *rigid* molecules put a difficulty in the way of assuming that the molecules of a solid are in contact, because the "elasticity" (or compressibility within certain limits) of a solid could scarcely be reconciled with this postulate of perfectly *rigid* molecules. The dynamical theory of molecules put forward by Sir William Thomson, which explains the elasticity of a molecule by a simple motion of the matter forming it, enables us to explain the elasticity of a solid (with molecules in actual contact) *by the elasticity of the molecules themselves*. By this theory also the *open* structure we have suggested becomes a natural consequence.

6. That matter does possess an open structure, due to some cause, appears to be sufficiently proved by independent facts. How otherwise could waves of light and the magnetic disturbance pass so freely through matter? It appears natural to assume that the molecules of a solid are in contact, on account of the resistance they oppose to displacement in all directions. If so, it would appear necessary to look for the interstices in the molecules themselves; and we think we have shown that this conclusion is not merely warranted by the case of gravity, but that it is in itself rather probable on independent grounds.

7. It may be observed that, by means of interstices in the molecules themselves, a mass may possess any degree of openness and yet be practically closed—*i. e.* closed to the penetration of all ordinary matter, such as the air, liquids, &c.,—as

evidently one molecule cannot readily penetrate into the interstices of another. On the other hand, the minute particles of the gravific medium pass through them with perfect freedom ; and though these interstices are so small, they are on the other hand so numerous (on account of the number of the molecules) that their total sum may represent a relatively very large vacant space. Under these conditions matter may be practically solid or continuous, because impenetrable by the finest portions (molecules) of other matter, and yet possess any desired degree of openness.

8. We would add a few remarks here in regard to the logical necessity of seeking a cause for gravitation. To do so is, as it seems to us, simply to look for an explanation of a natural phenomenon consistent with reason. One sometimes comes across the remark that the effect is an *ultimate* one, incapable of explanation. But then the physical investigator does not readily surrender the right of using his reason ; or we really have no power to assume that physical effects are brought about in a way incapable of appreciation by the reason. The most eminent minds have admittedly been in favour of an explanation. This was so (as is known) with Newton and Faraday. Count Rumford says, "Nobody surely in his sober senses has ever pretended to understand the mechanism of gravitation." Physical effects are generally admitted to be fundamentally effects of *motion* (however diverse they may be). The *one* fundamental cause, therefore, to get an insight into in physical science, is the cause of the development of *motion*. If we made an exception to this in any case (or assumed the motion developed was an *ultimate* fact incapable of explanation), then this would be pursuing a course which, if carried out in its entirety would leave *nothing* to be explained at all ; for it should be observed that the development of *motion* is in principle the *one* physical effect that requires explanation (from the fact that *all* physical effects are effects of *motion*). This inference surely deserves a mature realization. In the case of gravity we observe a *motion* of approach developed in two masses. Here, therefore, we have an instance of the *one* fundamental fact for which in principle an explanation is required. We require an insight into the cause of the development of this *motion* in the two masses. We want something more than merely to observe the *fact* of the motion ; we want (among other things) to understand why the energy of the motion developed has the *particular* intensity observed—also to account for the remarkable fact that the intensity diminishes in the complex ratio of the *square* of the distance, and not in some other ratio. Surely if any thing requires an explanation,

we have something to explain here. What, for example, would be thought of any one saying that the intensity of light varied as the *square* of the distance because it was its "*property*" to do so. The worst of this want of appreciation of the logical necessity for an explanation is, that the attention is called away and the inquiring faculties deadened, and thus these grand problems secure a share of attention which is utterly insignificant compared with that devoted to those of minor importance.

9. To prevent any misconception, we would remark here that the theory we have to suggest as an explanation of gravity is different in several essential points from that of Le Sage. The theory of Le Sage was dynamically defective in several essential points (probably owing to the comparatively small advance made in dynamics at his time). His assumption of continuous streams of particles coming from a number of different directions equiangularly scattered in space, the particles being supposed to come from *indefinite* distances ("ultramundane" particles), must appear evidently somewhat fantastic; for it appears inconceivable how the motion of such a system of streams of particles coming from ultramundane space should be kept up without confusion ensuing, owing to the mutual collisions of the particles of the streams which cross each other in all directions, if (as he assumed) each separate stream were to *move continuously in one direction*. For, however much the collisions might be reduced by reducing the size of the particles, they must occur in a long course of time, especially considering the high velocity at which it is necessary to assume the streams to move. Moreover the great objection to this view is that it involves, for the maintenance of gravity in the visible universe, a continual *supply* of matter from ultramundane space. This objection Le Sage distinctly recognized and could not surmount. The real merit of his theory was his fundamental idea that "gravity," or the tendency to approach of two masses, was due to the one mass sheltering or screening the other from the action of the streams of particles in which the two masses of matter were immersed—so that the remote sides of the two masses (where there is no shelter) are struck by a greater number of particles than the near sides (where there is shelter), and thus the two masses are urged together. The rest of his assumptions are in the nature of postulates, some of them unrealizable. He had little knowledge to draw upon at his time.

10. The points we have to bring forward are briefly as follows. We do not assume, as Le Sage did, the existence of streams of particles flowing as *continuous* currents in assigned

directions and coming from indefinite distances (or "ultramundane" particles, as he termed them). We do not assume that the particles producing gravity in the visible universe converge towards it in streams from ultramundane space. On the contrary, we assume that the matter producing gravity within the confines of the visible universe is *as a whole* at rest; or we regard the medium producing gravity simply as a gas. This gas differs from an ordinary gas only in the multiplicity of its particles, their excessive minuteness, and (consequently) extremely *long* free path. It is a direct consequence of the kinetic theory of gases that, *within the range of free path* of the particles of this gas, the particles move in precisely the right way to produce gravity; *i. e.* all the assumptions that Le Sage made *arbitrarily* as regards the motion of his streams, take place as inevitable necessities *within the range of free path* of the particles of a gas. The motion of the particles (in such a way as to produce gravity) is *automatically* kept up by a process of self-adjustment; *i. e.* *gravity is the inevitable result of the existence of a medium in space constituted according to the kinetic theory of gases.* It has been mathematically proved that the particles of a gas, within the range of free path, move uniformly or *equally towards all directions*. This special character of motion is automatically kept up under the influence of the collisions; or, however each particle (by itself) may change its course, this *general character* of motion is rigidly kept up, and is required to satisfy the condition of *equal pressure in all directions*. But this motion of particles uniformly or *equally in all directions* is precisely what is required for gravity.

11. The only further condition necessary is, that the range of free path of the particles should be great enough, so that (approximately) uninterrupted streams of particles move through the full range through which gravity has been observed to act. This length of free path (by any given number of particles in unit of volume) may be increased to any extent, simply by reducing the size of the particles. Taking, therefore, the visible universe as a whole, we have no streams of particles, but simply a gas at rest. The streams only exist within the range of free path of the particles, or within the range of gravity. We may compare the medium filling the visible universe to the air of a room, in which there are no streams, but the air is *as a whole* at rest. Contract the room (in imagination) up to the range of free path of the molecules of air, and we have streams of molecules sweeping in all directions through the room. The space in which we observe gravity may be compared to this contracted room, within

which streams of particles are sweeping through uniformly in all directions, the uniform motion of the particles *equally in all directions* (necessary to produce gravity) being *automatically* kept up under the influence of the mutual collisions, in a way demonstrated to take place in the case of a gas. It should be observed that this self-adjustment of their motion by the particles is not a mere result of chance, but a rigid adjustment of such a character that, if the uniformity of the motion were artificially disturbed, the particles when left to themselves would immediately correct the irregularity. The above length of free path, though great in one sense, becomes small and suitable for a gas pervading the vast range of the visible universe. Unlike Le Sage, we do not object to the collisions of the particles among themselves; for these collisions (in the case of a medium constituted as a gas) *maintain* the uniformity of motion. We require no *supply* of matter to produce gravity, and no supply of energy. The energy is self-contained. It is simply the case of the normal motion of the particles of a gas. Motion is as natural as rest. Nothing surely could be more simple than these conditions.

12. It might be said that this theory implies a *limited* range to gravity. It may be extended to any desired range simply by making the particles small, and consequently the free path great. We venture to think that rather than that a theory should be required to explain that the stars gravitate, a theory should be required to explain that they do *not*\* gravitate. For surely the idea of an indefinitely extended universe all of whose parts gravitated towards each other, would represent dynamical conditions of *instability* on the most gigantic scale. Imagine the incongruity of the idea of the whole universe tending to agglomerate in one (perhaps infinite) mass. To our mind no theory of gravity would be satisfactory that did not explain away this. The kinetic theory gets over this difficulty in a most complete manner, by allowing gravity to take place within a conformable range, without extending it to indefinite distances and thereby involving conditions of instability.

13. As we have said, we do not shirk in the slightest degree any criticism as regards this theory, but shall be glad to meet it, knowing that, if true, it will stand a full examination; and if false, the sooner it is proved so the better. There is one other point on which perhaps an objection might be raised. It might be said, If a gas exists in space, how is it that we do not detect its presence in experiments on the specific heat of other gases, this gas being at the *same time* present? or

\* Of course we do not refer to *double stars*, in close range.



why does not some of the heat pass from the gas experimented on to this gas? In answer to this, it must be kept in view that the gravific medium, though in principle constituted as an ordinary gas, differs from an ordinary gas profoundly in several respects. First, it is necessary to assume that its particles are (as essential to the long free path) incomparably more minute than those of an ordinary gas, and the number of particles in unit of volume much greater. A molecule of an ordinary gas surrounded by the particles of the gravific medium, might be compared (as regards relative dimensions) to a visible mass surrounded by the molecules of air. Next, it is necessary to assume that the velocity of the minute particles of the gravific medium is incomparably greater than that of the relatively massive molecules of ordinary gases. Now, it is a known fact that the *resistance* to the passage of bodies through a medium constituted according to the kinetic theory *diminishes* as the normal velocity of the particles of the medium increases. By making, therefore, the normal velocity of the particles of the medium sufficiently great, all perceptible resistance to the passage of bodies through it will disappear. It is as if the medium did not exist; it becomes quite impalpable, or its presence impossible to detect. This is consistent with observation. The amount of energy, or motion, abstracted from a body passing through the medium, and given up to the medium, is exactly measured by the *resistance* encountered by the body. It is this transference of energy to the medium that constitutes the "resistance." If, therefore, there is no measurable *resistance* to the passage of the body through the medium, there is no measurable energy abstracted from the body. This gets over our difficulty; for since the molecules of ordinary gases (at their relatively slow velocity) move through the gravific medium without appreciable *resistance*, there is no perceptible transference of energy (i. e. "*heat*") from them to the gravific medium. In other words, the presence of the gravific medium cannot interfere with the experiments on the specific heat of ordinary gases. In short, the high normal velocity of the particles of the medium necessarily renders it in all respects completely *impalpable*, or its presence impossible to detect by the senses. The high velocity of the particles is only naturally adapted to the *minute* size of the particles.

14. It would seem difficult to avoid the application of the above principles to the case of molecules in close proximity—"cohesion" or "chemical union." For, first, it would appear obvious that molecules in contact would be urged together with *exceptional* force, owing to the parts in contact cutting

off the *entire* stream of particles\*. Secondly, the *shapes* of diverse molecules (which would have no particular influence while the molecules were at a distance) would, when the molecules are in contact, have a great influence, according to whether the solid parts (or interstices) fitted over each other, so as to afford more or less shelter from the streams of particles. Possibly this might account for (or at least throw some light upon) the extraordinary *varied* behaviour of chemical "*affinity*." If this were justified, it would certainly be a remarkably *simple* cause. It is just possible that a thing may be missed sometimes by looking too deep. The processes of nature are as a rule recognized to be *simple*, this being the necessary condition for order. "Simplicity is the soul of mechanics." This view, if well founded, would have the advantage of correlating *all* molecular actions (including "gravity") under *one* cause. We have thought it just as well to mention these views in passing (without attaching the same definiteness to them as we attach to gravitation).

15. We would in conclusion make a few remarks upon a matter of *principle* connected with this subject. It must be evident that under a dynamical theory of gravitation, when a mass is lifted, the energy expended in lifting cannot be converted into "*potential*" energy, but must be converted into *kinetic* energy, in imparting motion to the particles impinging upon the upper side of the mass, and which tend to urge it downwards. Conversely, when the mass falls, *kinetic* energy is transferred from the particles of the medium to the mass. As a general principle, therefore, by the abandonment of the theory of "*action at a distance*," there can be no such entity as "*potential*" energy at all. We cannot avoid thinking that the very necessity to put forward a theory, that energy can possess, as it were, a *double* nature (kinetic, and *not* kinetic), in order to harmonize with the theory of "*action at a distance*," is by itself a sufficient logical condemnation of this latter theory. The idea of "*potential*" energy (*i. e.* an energy which is *not* kinetic) involves the inconceivable idea of an energy *without motion*, *i. e.* a kind of *spiritual* energy, whose existence or non-existence leaves matter in the *same* physical state. Already serious doubts have been cast upon its validity as a logical principle by some of the most eminent minds. From the prevalent use of the term "*potential*" energy, and at the same time the common repudiation of the theory of "*action at a distance*," one would be inclined to draw the inference that there was an idea to a certain extent prevalent

\* We believe Le Sage called attention to this in its application to "*cohesion*."

that this term "*potential*" energy could still be used in a certain sense, even *after* the theory of action at a distance had been abandoned. We think it can be clearly shown that this is not legitimate. For, by the rejection of the theory of "action at a distance," external matter or a medium (in a state of motion) must be concerned in developing motion in matter; and therefore it must be a case of *kinetic* energy, not "*potential*" energy. Either (for example) the motion of approach of two masses (or molecules) is developed (as supposed) *without* the concurrence of external matter, or (secondly) this motion is simply transferred to the masses from external matter. In this latter case (which represents the case where the theory of "action at a distance" is rejected) the energy exchanged can only be the energy of motion (*kinetic* energy), *not*, therefore, "*potential*" energy. It might, perhaps, be urged that even when the theory of "action at a distance" is rejected, a raised mass can still be said to have "*potential*" energy (due to its position), because it *can* fall. This, however, may be proved not to be legitimate. For, from the very fact that (by the rejection of "action at a distance") the energy expended in raising the mass was converted into *kinetic* energy, it cannot have been converted into "*potential*" energy (*i. e.* an energy which is *not* kinetic) *as well*. A double equivalent of energy cannot be generated\*. We think we have clearly shown, therefore, that by the rejection of the theory of "action at a distance," the idea of "*potential*" energy must (to be logically consistent) be unreservedly abandoned. The rejection of "*potential*" energy makes all energy of *one* character, *viz.* energy of *motion*; and then the great principle of the *indestructibility of motion* inevitably presents itself for acceptance. With the theory of "action at a distance," the idea of "*force*" (in the old sense of an action across space without the intervention of matter) must be given up. Thus we have in the physical world, only the two great fundamental conceptions of *matter* and *motion* left; or all physical phenomena come thus to be correlated in one grand and fundamental aspect, *viz.* as consisting in the various exchanges and phases of *motion*.

London, Jan. 11, 1878.

*Note.*—We think it right to add that we make no claim to have shown (as this had been already done by others) that the molecules

\* To say that a raised weight tending to approach the earth by the action of the gravific medium, possessed "*potential*" energy because it *can* approach the earth, would be like saying that a ship confined by a cable and tending to approach a rock by the action of the wind, possessed "*potential*" energy, because it *can* approach the rock (by the breaking of the cable). The cases are evidently parallel.

of a gas regulate their motions so as to move in a *particular* manner, though we doubt whether, if we had not arrived at this conclusion *independently* for ourselves, we should have been able to make a practical application of it. The point it has been our object to call attention to (and which apparently has not been noticed by others) is, that the motion of the particles of a gas *within the range of free path* precisely satisfies all the conditions Le Sage *arbitrarily* assumed in order to produce gravity—or that the special character of the motion Le Sage *arbitrarily* assumed his streams of particles to have, *actually exists* within the range of free path of the particles of a gas—in other words, that all the effects of gravity can be produced by *the mere existence of a gas in space*, and indeed *must* be produced if such a gas exists.

---

XVIII. *Electromagnetic and Calometric Absolute Measurements: the Absolute Value of Siemens's Unit of Resistance in Electromagnetic Measure; the Relation between the Current-work and the Heat-evolution in stationary Galvanic Currents; and the Absolute Values of some constant Hydroelectromotive Forces in Electromagnetic Measure. (Condensed Comparison of the Results of a Series of Investigations.)* By H. F. WEBER, *Professor of Mathematical and Technical Physics at the Federal Polytechnic Academy of Zurich.*

[Continued from p. 43.]

### III. *The Heat produced by Stationary Galvanic Currents.*

MR. JOULE, thirty-seven years since, showed by experiment that the quantity of heat which a stationary galvanic current of intensity  $i$  generates in a conductor whose resistance is  $w$ , during the time  $z$ , is proportional to  $i^2 wz$ . Sir W. Thomson then, in 1851 (and Prof. Clausius and others later), proved in the theoretical way that the value of the mechanical work which is expended in the stationary galvanic current of the intensity  $i$ , in a conductor with the resistance  $w$ , along which the electromotive force  $E$  is in action, in the time  $z$  is equal to the product  $iEz$ , or, pursuant to Ohm's law, equal to the expression  $i^2 wz$ , where the quantities  $E$ ,  $i$ ,  $w$  are to be taken as measured according to absolute measure. If we make the assumption that, in a stationary galvanic current in which the evolution of heat is the only action of the current-flow, the amount of heat developed in the unit of time,  $Q$ , is the full equivalent of the work expended in the same time, then we have

$$JG = i^2 w = iE,$$

where  $J$  denotes the mechanical equivalent of the unit of heat.