

Neuerwall 69.
Hamburg. Jan 29. 1893.

Dear Sir

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Many thanks for your letter, which contained much that interested me. Of course it was natural that I should like to know that the most advanced views of the present day are not unfavorable to my particular theory of gravitation (as opposed thereto).

I may just mention that Dr. Isenkrähe of the Gymnasium Crefeld sent me ~~shortly~~ (after publication of my first paper in the Phil Mag 1877) a book of his on gravity, published in 1879. This is a kinetic theory (subsequent to mine) with in-elastic atoms, and no mention whatever is made of the mean length of path of the atoms, which in my view is the whole key to such a theory, without which it is worthless. This seems to me true, even if the atoms (in Dr Isenkrähe's theory) were perfectly elastic, and suffered only a diminution of translatory motion. For according to my exposition, gravity is a "dimensional property" of a gas, or gravity cannot hold between masses unless their distance be well within the range of mean path. Otherwise the gas can easily re-adjust its pressure (I mean when the distance of the opposed masses is a considerable multiple of the mean path): or gas can easily rush in laterally to fill up or neutralize the reduced pressure, and so nullify gravity. Curiously enough Dr Isenkrähe (although he treats his theory mathematically) does not seem to see this point. And I confess I did not see it myself at the time of my correspondence with him so clearly, as it presents itself to me now (as an essential condition to any kinetic-gas theory). If you found interest in seeing Dr Isenkrähe's book or had time, I could of course easily forward it.

The recent experimental researches of Prof Dewar (not to forget some of Cailletet's smaller scale results, perhaps less reliable however?) are doubtless very valuable and fundamentally important, especially his view of the discovery of the powerlessness of action at a distance to resist cold - ^{or that it is a mythical force} to speak, which is illustrated by the observation that potassium and liquid oxygen will not combine. Pictet or Cailletet had however, I think previously shown that even sulphuric acid and soda would not combine at the temperature of liquid oxygen.

In my work "Physics of the Ether" 1875, curiously enough is the following statement viz -
"I know of no physical cause ceases to exist, the effect also ceases; it follows that at the absolute zero of temperature (absence of vibrating energy) the general phenomena of 'cohesion' including the aggregation of molecules in 'chemical union' would cease to exist." (page 64).

Some special experiments on attraction by vibration I made about nine years ago both very

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large tuning forks some pounds in weight, considerably impressed me, I may add also. The removal of practically the last foothold to action at a distance by Prof. Dewar's experiments, will doubtless serve to attract general attention to the gravitation problem. It strikes me it would be of special interest to perform experiments on the tensile resistances of metals and other bodies at the low temperature of liquid oxygen.

The mention of Dr. Rankine's theory above may serve to illustrate more clearly the point that I claim as novel for my own, or regard as essential to any such kinetic theory of gravity: and the point may have some physical and mathematical interest independently also.

In regard to a former letter of yours (29.10.91) I assume that the prohibition of the second law of thermodynamics against utilizing the motion stored ^{energy} in a material medium, ^{nevertheless} could no longer apply when the mean length of path is far outside molecular limits or dimensions? Thus Lord Kelvin (Phil. Mag. May 1873) even talks of the possibility of utilizing the stores of energy in Le Sage's indefinitely great supplies of energy to the known universe in the form of "ultramundane corpuscles" — by means of bi-refracting crystals, assuming their permeabilities or weights thereof to be slightly different dependent on which axis of the crystal is vertical. This appears to me interesting theoretically as concluding that the

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utilization of the energy stored in the finest media is not opposed to any thermodynamic principle under certain conditions. I have been led to believe that the condition is that the length of path should lie outside that are generally regarded as molecular limits, or space dimensions comparable to the size of gross molecules, or their range of path in the atmosphere near the earth's surface. But, assuming this view to be correct, any attempt at precise definition ^{of conditions} here seems to be rather difficult (even)? Although I commenced this letter on the day of receipt of yours, it has been kept back a day or two with the view to revision and ~~its~~ completeness.

Yours very truly
S John Preston

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