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Seiler Strasse 29.
Hamburg, Sept. 3. 1894.

Dear Sir

I was much interested in reading your letter of the 31 ult.: and now my second to you may be partly superfluous.

What I cannot help thinking in regard to my modification of Le Sage's theory (fundamentally enough to stand perhaps for a new theory in the eyes of some) - is that a great many otherwise competent do not understand it. For how explain otherwise the way it has escaped criticism? The late James Croll, in a paper in Phil. Mag. Jan 1878, characterized my paper as "imparting" a clear and able exposition of Le Sage's theory. Although he admitted that I had proved some results from established *conclusions of the kinetic theory of gases. Lord Kelvin, remarkably perhaps, did not notice my first paper (Phil. Mag. Sept 1877) or comment it; although he recommends his own paper, which mine enormously

Foot Note * The oversight of supposing gravity to be due to a reduction in number of screened atoms, instead of to a reduction in velocity of the atoms, is not credit into my second

simplifies, and first sets on a natural basis. You may remark it may be strong to say this of one of the acknowledged talents of Lord Kelvin. But I think all my water overshoots now and then, even the most competent.

Now as you will appreciate such an exposition; I will try here briefly to set forth what my idea or the novelty of it, is. And it is so fundamental, the kinetic theory, "the exquisite kinetic theory", as Lord Kelvin calls it in his recent work "The Constitution of Matter" - that my exposition may be worth attention.

In a paper "On some points relating to the Dynamics of Radiant Matter", published in Nature March 17, 1881, I pointed out more at length how the prevalent saying that "the molecules of gases move in a very irregular manner", tends to conceal the fact that as a system this motion is regular or perfectly symmetrical: it is only irregular when each molecule separately is considered. The regularity of this motion consists in its being a "radiant" motion, as I indicated. This is easier seen by lengthening the path of the molecules, but it is true independently of the length of path.

Take a gas of very fine molecules of small mean distance apart, and whose mean path is at least say 1000 - to fix the ideas - times the diameter of the solar system. Take any point in space. Then the thesis is that this (over)

point is a "radiant point" in the sense that
it is a self correcting beautiful mechanical adjustment
(when the oblique collisions are chiefly
instrumental) all the matter flying toward and
from ^{or through} the small, not infinitely small, point of
space, moves symmetrically as if along the radii
of a sphere drawn from this point as a centre. The
atoms are continually changing their directions (it is
true) by their ^{mutual} encounters: but it is precisely by
these encounters that the symmetrical motion is
kept up. Now I said, here we have a natural
basis at once for Le Sage's theory, or rather for
what he wants, without any of his postulates. Le
Sage merely postulated that matter shall move
so, by assuming streams of atoms, each stream
of finite length, equally or symmetrically
distributed about the centre of the known
visible universe - streaming through for a finite
time (he fixed 10,000 years) to produce gravity. This,
as you will agree I think, is no natural theory
at all: yet Lord Kelvin accepted it. All the
letter did (an important improvement no doubt)
was to make the atoms perfectly elastic, so that
no energy was destroyed at their encounters with gross
matter or with each other, but translatory motion
was diminished by conversion into vibratory at the
shock of the atoms against gross matter, and then
(this is Lord Kelvin's point) vibratory motion was
turned back into translatory by the collision of
the atoms against others which they might happen
to encounter in their course. But Lord Kelvin says

of Le Sage's theory in the form of 8 postulates,
or "fundamental assumptions" which he quotes: -
"This much is certain, that if hard indivisible
atoms are granted at all, his principles
are unassailable and nothing can be said against
the probability of his assumptions. The ^{only}
imperfection of his theory is that which is inherent
to every supposition of hard indivisible atoms." (Phil.
Mag. May 1873, page 327). So Lord Kelvin
accepts Le Sage's postulates. My application of the
principles of the kinetic theory of gases to Le Sage's
idea, removes these postulates en bloc. I cannot say
however that this has been appreciated, or that I have
got credit for this. My theory in the "dissertation", of
which I venture to send another copy, is more clearly in
fact perfectly clear - by the valuable exposition
and of Prof. Ludwig Boltzmann - expressed no
doubt.

But I am afraid Lord Kelvin may object
to the subject being revived in the Phil Mag (from
a letter exchanged with him ^{perhaps} some 10 years ago). Only
now my views are much ^{more} clearly visible to readers
(in the "dissertation"): so that possibly he or Meissner
Taylor & Francis may think it opportune to call
attention to the problem; as also some paragraphs
in my "dissertation" are decidedly fresh or original. As
said, in my second letter to you ^{in mine}, ^{two} paragraphs ^(in French)
from Lord Kelvin's paper are quoted, but one, was
obscure from very curious punctuation, which an
English translation might rectify. Yours very truly
Prof. J. F. Fitzgerald F.R.S. S. Tolver Preston

P.S. to letter dated Sept. 3, 1894.

Although it is accepted that the distance to which a gas extends (or the consideration that the universe may be infinite) makes no difficulty in applying the principles of the kinetic theory; as it is the same as if the gas were enclosed in a very large finite envelope: I have nevertheless suggested — this was read ^{to the Royal Soc.} of Edinburgh by Dr. C. G. Knott ^{in the course of a paper} introduced by Prof. Tait — that the universe may after all not be uniform throughout, as the idea of an unlimited æther might lead one to imagine.

Without pretending to more than the bare idea itself, I suggested it to be apparently possible to suppose (according to Thomson's theory) a shell — or perhaps two concentric ones stably interconnected — of woven vortex filament containing ^{our} æther or gravit medium: and then outside this a different universe or universes without gravity perhaps, ^{if no such fine gas} even if matter exist outside, even if matter in some shape be imagined outside. All this is

* On Dec. 12, 1893, fact mentioned in Nature, Feb. 1, 1894.

very speculative of course, but legitimately imaginable apparently: and may possibly illustrate the fertility in resources of the Thomson-Helmholtz deductions. There may be too much a tendency to assume unlimited uniformity from the difficulty of imagining a boundary to the æther. The fluid, it is understood of course, would exist extend beyond the shell, but it need only have the peculiar structure representing our æther inside. From its speculative nature, this is put on a separate sheet.

J. Tolson Preston
Seiler Strasse 29
Hamburg. Sept 4. 1894.

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Prof. G. F. Fitzgerald F.R.S