THE SEARCH FOR AN INFLUENCE OF TEMPERATURE ON GRAVITATION

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Abstract: There were several careful experimental attempts to find a temperature effect upon gravitational force, from Lavoisier and Rumford (towards the end of the 18th century) to Shaw (20th century). Some positive effects were sometimes reported (Hick, Shaw) but further investigation showed that no influence seemed to exist. For temperatures up to 200 °C, the searched effect was established to be less than 10^{-9}/°C for passive gravitational mass (weight) and less than 10^{-6}/°C for active gravitational mass (attraction). This article describes the history of this search and discusses methodological issues raised by it.

Keywords: gravitation; non-Newtonian effects; experiments on gravitation; temperature and gravitation

1. INTRODUCTION

When one thinks about experimental or observational gravitational research made at the beginning of the 20th century, one usually recalls general relativity and its three “classical tests”: perihelion precession, red-shift and light deflection. Nevertheless, at the turn of the century, there was an intensive research on gravitation (both theoretical and experimental) providing exciting ideas and unexpected empirical results. Only after the success of general relativity, in the 1920’s, those independent lines of research subsided to the background,

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rapidly disappearing (but for sporadic articles) towards 1930. The general outlook of those researches may be learned from several review papers of that time (Drude, 1897; Zenneck, 1901; Poincaré, 1953; Oppenheim, 1920; Poynting, 1900).

The aim of this paper is to provide an analysis of one of those forgotten lines of research: the attempt to find a connection between gravitation and temperature. The article will focus upon experimental work, leaving aside most of the bulky speculative material of the time.[1]

Nowadays, we have learned from the theory of relativity that heating a body will increase its mass, since any energy change ΔE of a system will produce a mass change $\Delta m = \Delta E/c^2$. As a matter of fact, a theoretical prediction of the relationship between heat and inertial mass[2] had been made by Friedrich Hasenöhrl (1874-1915), before the researches of Albert Einstein (1879-1955) on this subject (Hasenöhrl, 1904-1905). The influence of temperature upon weight was discussed for the first time by Max Planck (1858-1947) (Planck, 1907) and it stimulated the first Einsteinian attempt to study gravitation (Einstein, 1907). According to the theory of relativity, an increase of 50 °C of the temperature of 1 kg of water will

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[1] This work was written during the years 1995-1996, while the author was a visiting scholar of the Department of History and Philosophy of Science, University of Cambridge; and a visiting fellow of Wolfson College. It is now published for the first time. The content of the paper has not been updated or complemented, only slight changes were made.

[2] It is important to remark that there are several distinct mass concepts. Inertial mass is the quantity that appears in the dynamical laws of mechanics ($p=m.v$). Gravitational mass is the quantity related to gravitational forces. Two kinds are now distinguished: active gravitational mass is the source of the gravitational field; passive gravitational mass is the quantity in the attracted body that reacts to the external gravitational field. In principle, one of those masses may change without any change of the others. See Reichenbächer, 1923, Bondi, 1957.

produce a mass increase of 2,3x10^{-12} kg – a difference impossible to detect with any available instrument, even now. But the attempts to measure gravitational effects due to heat or temperature did not spring from the theory of relativity: they came either from different theoretical sources or from mere guessing: "what will happen if...". They were part of a broader exploratory approach to experimental gravitation that was especially strong in the late nineteenth and early twentieth centuries.[3]

Although those attempts led to no discovery, they have some relevance for several reasons: (1) they did show that there was no detectable influence of heat or temperature upon gravitation (and negative results are important scientific data); (2) they seemed to yield positive results for some time – a result that stirred the scientific community; and (3) they help us to learn a forgotten chapter of gravitational research.

2. EARLY IDEAS AND EXPERIMENTS TO THE END OF THE 18TH CENTURY

Within the context of Aristotelian physics, heat and cold are respectively linked to lightness and weight. Fire and hot air tend to ascend and are therefore light. This was, perhaps, the oldest proposed correlation between heat and gravitation (weight). Even after the decay of Aristotelian physics and the acknowledgment that hot air was not absolutely light but got its tendency to go up from the surrounding air, this deeply seated and intuitive idea of a negative weight related to heat did not disappear altogether: it reappears, for instance, inside flogiston theories, in the 18th century (see Partington & McKie, 1937-1939).

Another line of thought linking heat to repulsion may be found in Newtonian physics. Isaac Newton (1643-1727) did not

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[3] See my previous paper in this volume: Roberto de Andrade Martins, Experimental studies on mass and gravitation in the early twentieth century: the search for non-Newtonian effects.

accept the Baconian conception of heat as a hidden vibrating motion of the smaller parts of matter. Instead, he speculated about the idea of heat as a repulsive force between atoms. In the second book of Newton's *Principia*, he was able to explain Boyle's pressure-volume relation using a model of static repealing atoms. In this model, these forces were not gravitation: they were short ranged and varied inversely as the distance. Nevertheless, here we find another deeply rooted and intuitive idea (that sometimes spontaneously arises in the minds of physics students): heat as a repulsive force. This conception is able to explain the dilation of heated bodies and might suggest that highly heated bodies might repeal the Earth and would become "light".

A third important conception is that of heat as a substance[4]. This idea is suggested by the possibility of transferring heat from a body to another and by calorimeter experiments where there seems to be a quantitative conservation of heat – and, remember, the main mark of (Aristotelian) substances, as material causes, was their conservation. Antoine-Laurent Lavoisier (1743-1794) and Pierre-Simon de Laplace (1749-1827) thought that heat (caloric) was a substance, although they did not exclude the possibility of its being just a kind of internal motion (Lavoisier & Laplace, 1780). But the conception of a substance leads to the property of weight – and, indeed, Lavoisier asked himself, about 1770, whether a body would become heavier when hot. In one of his laboratory notebooks (Berthelot, 1902) he referred to Georges-Louis Leclerc de Buffon (1707-1788), who "seems to have proved by experiments deemed by him conclusive, that fire matter weights, and that an incandescent body has from 1/350 to 1/600 of it [fire] in it mass". Nevertheless, Lavoisier stated that Herman Boerhaave (1668-1738) had already shown that an iron

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[4] For a review of heat conceptions towards the end of the 18th century, see Bentham, 1937.

rod does not change its weight when becoming incandescent – and he accepts Boerhaave's result.

At the end of the 18th century, the problem of weight (or lightness) of heat was much discussed, mainly in the context of flogiston theory. The experimental problem was delicate: heated (calcinated) metal increases in weight, even after cooling. This effect was known in the 17th century and was explained by Robert Boyle as due to the weight of heat. It seems that this was a popular subject of inquiry at that time, as we find a description of experiments on [5]the increase of the weight of metals by fire in the Royal Society. We now ascribe this effect to absorption of oxygen and oxide formation, but the role of oxygen was completely understood only after Lavoisier's researches.

Besides that, from a purely physical point of view, a heated body will seem lighter than the same body cold, for three main reasons: i) ascending convection currents of the surrounding colder air, that produce an upwards force upon the body; ii) volume increment that produces a greater upwards aerostatic thrust upon the body; and iii) changes of the moisture attached to the surface of the bodies. For the same reason, a colder body will show heavier than the same body heated. None of those effects will be observed if weight measurements are made in a vacuum – but vacuum weighing is very difficult.

The effect of air currents was studied in the early 17th century by the *Accademia del Cimento* (Tozzetti, 1780, vol. 2.2, pp. 618-619). Some experiments had shown that a hot piece of metal seemed lighter; but it was then observed that placing an incandescent piece of iron *near* the balance would also produce an apparent weight decrease of anything placed at the balance, and it was then easy to find the cause of this effect.

2.1 Lavoisier's ice experiment

Leaving aside several earlier discussions, let us review some experimental attempts to study the problem made at the end of

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[5] See Sprat, *The history of the Royal Society of London*, pp. 228-9: "Experiments on the weight of bodies increased in the fire".

the 18th century. The subject is discussed by Lavoisier (with the collaboration of Laplace) in a paper presented to the French Academy of Sciences in 1783 (Lavoisier, 1783). This work discusses the change of weight of phosphor and sulfur when they are burned. The weight increase is ascribed by Lavoisier to part of the air (oxygen) that combines with phosphor or sulfur; but another explanation is discussed: could not this change be ascribed to the heat that escapes from those substances when they are burned? In this case, the weight of heat should be negative, of course. In order to test this idea, the following experiment is presented: 8 grains of phosphor are burned in a sealed flask; the weight of the closed flask is the same both before and after the combustion (after it is allowed to become cold). The precision of the balance is stated to be 1/4 of grain. Supposing that the whole of the phosphor did really burn, this test shows that the weight change was smaller than 1/32 (or 3%) of the weight. For our standards, this is a poor test, but it was highly relevant, in the context of the experiment, since the changes of weight of sulphur and phosphor, when burned, could be up to 100%.

After this chemical test, they try another approach. In order to test whether heat has any weight, they look for a weight change of a flask with water when the water is frozen. From previous measurements, they had already found that the burning of 92 grains of phosphor will be able to melt 1 pound of ice. Therefore this new experiment (much safer than the burning of a large amount of phosphor in a closed flask) can be readily used to discuss the weight changes of phosphor.

The ice experiment is produced in the following way: a glass flask is filled with 1 pound of water close to its freezing point. The flask is hermetically closed and its weight is found. The flask is then put into a snow and salt bath until the water freezes. The flask is now weighed again. The ice is then melted and the process is repeated several times. No change of weight is observed. A warning is presented that the environment must be at a temperature close to the freezing point, in order to avoid condensation of moisture. As the water is always at similar temperatures, no problem arises from convection currents or changes of volume of the flask.

The sensibility of the test is stated to be of 0.1 grain. As one pound corresponds to 9,216 grains, the change of weight of water as it froze or melted was less than 1/92,000 or about 1,1 x 10^{-5}.

2.2 Fordyce's ice experiment

A similar experiment was made by George Fordyce (1736-1802) (Fordyce, 1785). It was not possible to detect whether he knew Lavoisier's experiment.

Fordyce took a glass vessel (its weight was 451 grains) and put inside the vessel 1,700 grains of water, leaving part of the vessel full of air. The vessel was hermetically sealed. Its initial temperature was 37° F. It was put in a refrigerating mixture at 12° F until it began to freeze. Fordyce took the vessel of the mixture, shook and carefully wiped it and measured its weight. The vessel was then put again in the refrigerating mixture, and Fordyce made repeated measurements of its weight as the freezing went on. The weight was found to increase steadily. When all the water had been turned to ice, the system had gained close to 1/5 of a grain. The temperature of the ice inside the vessel was now about the same as the temperature of the refrigerating mixture (12° F). In order to avoid any effect due to temperature differences, Fordyce took the vessel out of the mixture and waited until it began to melt (that is, until its temperature became 32° F). Repeated measurements showed that the weight decreased. When the temperature reached 32° F, the total weight gain, since the beginning of solidification, was about 1/16 of a grain. He waited until all the ice had thawed, and found that the weight returned to its initial value (at the beginning of solidification) but for a very slight increase of about 1/1600 of a grain (one division of the scale of his balance). The reported change was about 1/27,000 or 3,5 x 10^{-5} of the weight of the water.

Fordyce says that the experiment was repeated several times, always with similar results. In the discussion of the effect, he stated:

> The acquisition of weight found on water's being converted into ice, may arise from an increase of the alteration of gravitation of the matter of the water; or from some substance imbibed through the glass, which is necessary to render the water solid.

Fordyce recalls that heat decreases some small-scale forces (cohesion, chemical forces) and suggests an interesting experiment to test whether there was only a force change or a matter transport. In modern terminology it may be described thus: Suppose we have two equal hollow pendulums, one full of ice and the other with an equal mass of water. The period *T* of a pendulum may be computed as

$$T = 2\pi\sqrt{\frac{L.m}{w}}$$

where *L* is its length, *m* the (inertial) mass of the swinging body and *w* its weight. In common cases, this formula reduces to

$$T = 2\pi\sqrt{\frac{L}{g}}$$

where *g* is the gravitational acceleration. But this formula is equivalent to the previous one only if $w = mg$, and this relation could be violated in some cases (if heat decreases the gravitational force without changing the inertial mass). So, if heat decreases the weight without transference of matter, the period of the water pendulum will be greater than the period of the ice pendulum. But if there is any transference of matter in the process, weight changes will be accompanied by proportional inertia changes and the period of the pendulums will be equal. Fordyce even considers the possibility of "absolutely light" matter, that would have inertia but would repel common matter, instead of attracting it. In that case, the change of the period would be twice as large, because a weight increase would be associated to an inertial decrease and both changes would add instead of compensating each other. In modern terminology, Fordyce is proposing a test of the principle of equivalence (or of proportionality of inertial and gravitational mass) comparing water an ice.

Although Fordyce makes some slips in his argument, the essential idea (as exposed here) is correct and very interesting. Nevertheless he does not make the test – and it would not be easy to detect period changes of 10^{-5} at that time.

2.3 Rumford's repetition of Fordyce's experiment

Motivated by Fordyce's previous experiments, Benjamin Thompson, Count Rumford (1753-1814) began in 1787 to study the question (Thompson, 1799). His method was carefully devised in order to avoid spurious results.

He took two very similar flasks that he named *A* and *B*. He put into *A* a weighted amount of water (4107.86 grains Troy), leaving about half the bottle empty; in the other flask (*B*) he put an equivalent weight of "weak spirit of wine" (alcohol). His clever idea was that he could produce the freezing of the water at a temperature such that the alcohol would not freeze; the heat leaving the water when it freezes would be much greater than the heat leaving the alcohol, but all other effects would be highly similar. Comparing the two flasks, it seemed possible to measure any weight effect due to heat.

Both flasks were hermetically sealed, washed, cleaned and dried. They were suspended at the two ends of the arms of a fine balance, inside a room heated to 61°F. After he thought the bottles and their contents had reached this temperature, he equilibrated the balance by using a small silver wire. He waited for 12 hours and observed that there was no alteration of the equilibrium. Then, he removed the whole apparatus to an unheated room (it was winter) where the temperature was 29° F. he left the balance and bottles there for 48 hours and, after this time, he observed that the balance now inclined towards the flask containing water (*A*). This seemed to show that the weight of the water had increased (or the weight of the spirit of wine had decreased). As the greater heat effect should happen with the water (because of its change of state), it seemed that the water had increased its weight.

The weight difference between the flasks was now 0.134 grains (that is, 1/35,904 of the total weight) – an effect of the same order of magnitude as that measured by Fordyce. The water had completely frozen, and the spirit of wine was still liquid. Therefore, a much larger amount of heat had gone out of the water than from the other flask and the observed effect should be more likely ascribed to the water: by losing heat, it became heavier!

Notice that the experiment does indeed avoid spurious effects due to convective currents, water attached to the surface of the flasks and volume changes. The effect seemed real, but Rumford was very surprised by the result and did not publish it. He carefully studied the balance itself, and found no defect on it. He repeated the experiment and observed that the change of weight was reversible; but he noticed that the amount of weight change was not always the same. Repeating the experiment with water and mercury, no effect was observed.

At last, Rumford found out that the observed effect was due to two main reasons: small initial temperature differences between the water and the spirit of wine; and problems associated to moisture at the surface of the flasks. By a more careful repetition of the experiment, the effect was eliminated – there was no weight difference, at a stated sensitivity of 1/1,000,000 or 10^{-6}. Rumford boldly concluded that “all attempts to discover any effect of heat upon the apparent weight of bodies will be fruitless”.

As Rumford himself states in his article, the experiment was a relevant test about the hypothesis of substantiality of heat. As he was already convinced that heat was a kind of motion, he was led to repeat the experiment until he found out its defects. Had he believed heat was a substance, he would probably have published his first results, since they were seemingly free of spurious effects.

3. RADIATION REPULSION: FRESNEL AND CROOKES

Several attempts were made during the 19th century to detect a repulsive effect of heat or radiation. Augustin-Jean Fresnel (1788-1827) attached light bodies to the ends of a torsion balance and concentrated the light of the sun upon one of them, by means of a lens (Fresnel, 1825). He noticed no effect. But, when he repeated the experiment inside an evacuated container, he noticed an apparent strong repulsion between two test bodies heated by the sun’s light. The air pressure was about 1 or 2 mm of mercury. In order to test whether the effect was due to the remaining air, Fresnel increased the pressure to 20 mm of mercury and the repulsion became much weaker. He concluded that the observed repulsion was not produced by the air. Being unable to explain it, Fresnel did not continue his research[6].

Some strong effects of air currents produced by heating were sometimes observed and explained by Claude Servais Mathias Pouillet (1791-1868) (Pouillet, 1849). But Fresnel’s effect was of a different kind.

This effect was elucidated half a century later by William Crookes (1832-1919). In order to make some delicate weighing to measure the atomic weight of thallium (Crookes, 1873), Crookes built a balance that could be operated in a vacuum. He expected it to be free from every systematic error produced by heat, as there would be no air convection or atmospheric thrust. Nevertheless, the balance seemed to show that bodies were lighter when hot than when they were cold. Crookes conjectured

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[6] It seems that a similar experiment was made at the *Accademia del Cimento*, as one may infer from a drawing that, unfortunately, was not accompanied by any written description or explanation. The drawing will be found in Tozzetti, 1780, vol. 3, fig. 304.

that this effect could be due to a repulsive force produced by heat. In order to test this hypothesis, he used a torsion balance inside an evacuated container, as Fresnel had done before him (Crookes, 1874).

Crookes observed an apparent repulsion between hot bodies using this king of apparatus. Even placing a hot body outside the glass container, the same effect was observed. A cold body produced an apparent attraction. At the beginning, Crookes believed that this was a direct effect produced by the hot body itself; but he noticed that the effect varied as he changed the pressure of the residual gas in the container. At last, his continued search led to the discovery that the effect was due to the remaining air itself, (Crookes, 1875-1876; Schuster, 1876; Reynolds, 1876) and it was named "radiometer effect". Crookes' radiometer is now well known and it is not necessary to describe it here. Notice that this effect is completely different and much larger than the direct effect of radiation pressure that was measured some years latter by Pyotr Nikolayevich Lebedev, and by Ernest Fox Nichols and Gordon Ferrie Hull (see Schagrin, 1974).

During the whole of the 19th century, several other naïve attempts were made to detect some kind of temperature attraction or repulsion. Crookes himself describes several of these (Crookes, 1875). Besides the previously described ideas that could lead somebody to conjecture about the existence of such effects, there were new reasons, now. The 19th century was the period when ether theories grew and expanded to all fields. After their success in optics (the revival of the wave theory of light), electricity and magnetism (Faraday's and Maxwell's theories, among others), they also conquered gravitational theoretical thought.

Now, if gravitation is not a direct interaction at a distance but something transmitted by a medium, one might speculate that the interaction between the bodies and the medium or the properties of the medium itself could change as a function of temperature. This was strongly suggested by the so-called "kinetic theories of gravitation", where the gravitational ether was analogous to the kinetic gas model (see specially Taylor, 1876). This theoretical background is a possible explanation of the increased interest in experimental gravitational research, specially towards the end of the 19th century and beginning of the 20th century.

It is interesting to remark that there was also some indirect evidence of an influence of temperature on gravitation. In the *Principia*, Newton developed a mechanical model for reflection and refraction, and in the *Optics* suggested a possible relationship between the forces that bend light rays and gravitational forces. As the index or refraction of glass changes with temperature, John Herapath suggested that heat changes gravitation. "In my own mind I have no doubt of the fact, but we are deficient of direct experiments to prove it." (Herapath, 1847, vol. 1, p. xv).

4. TEMPERATURE ANOMALIES IN MEASUREMENTS OF THE GRAVITATIONAL CONSTANT

The next hint about a possible relation between temperature and gravitation was provided by standard experiments designed to measure the so-called Newtonian constant of gravitation G. In the 19th century, such measurement was usually described as a measurement of the mean density d of the Earth – because one of these parameters can be calculated from the other, provided one knows the gravitational acceleration g and the radius of the Earth R:

$$g = GM/R^2 = G(4/3)\pi R^3 d/R^2 = (4/3)\pi dGR$$

4.1 Cornu and Baille

In 1873, Alfred Cornu (1841-1902) and Jean-Baptiste Alexandre Baille (1841-1918) noticed a difference of about 1% between their measurements for G made in winter and in summer:

summer: $G = 6{,}760\times10^{-8}$ Nm^{2}kg^{-2} $d = 5{,}56$ g.cm^{-3}
winter: $G = 6{,}836\times10^{-8}$ Nm^{2}kg^{-2} $d = 5{,}40$ g.cm^{-3}

The authors explained this difference as due to a small deflection of the balance beam and ascribed no importance to this result (Cornu & Baille, 1873).

4.2 Hicks

Two years latter, William Mitchinson Hicks (1850-1934) analyzed Francis Baily's 1841-2 measurements of the gravitational constant (Baily, 1843; Hicks, 1883-6) in order to search for a temperature anomaly. Baily's experiments were made at temperatures varying from 30° F in winter to 69° F in summer. Hicks carefully classed a large number of observations by temperature ranges. He found a regular correspondence between temperature and the measured value of the mean density of the Earth, as shown in Table 1.

Temperature: (°F)	Number of observations:	Mean density of the Earth: (g.cm^{-3})
36	46	5.7296
40±2	128	5.7341
45±2	247	5.6823
50±2	302	5.6799
55±2	187	5.6594
60±2	333	5.6495
65±2	140	5.5935
68	96	5.5828

Table 1. Relation between temperature and computed density of the Earth, according to Hicks' analysis of Baily's data.

Hick's results seemed to disclose a regular increase of gravitational attraction with temperature rise. The observed difference between the values measured at the highest and lowest temperatures amounted to about 3% and could not be ascribed to statistical fluctuations.

Notice that Baily's data, used by Hicks, were obtained with a set of different materials; therefore, the temperature effect was not due to the peculiar behaviour of a particular substance.

Hicks began to search for possible sources of systematic error. He studied the effect of temperature on the density of the air and computed effects arising from this. He showed that taking these effects into account he could increase the coherence of Baily's results; but that affected only the fourth decimal place in his table. He studied other effects and concluded that none of them could explain the observed anomaly.

4.3 Analysis of the results

At this point it is convenient to analyze and compare some of the previously described experiments. Let us suppose that there were no systematic errors. What could one conclude? If Rumford did not observe any change of weight above 10^{-6}, couldn Hicks measure anything as great as 3%?

Rumford's experiment does not show that temperature has no influence on gravitation. It just showed that no sensible effect was noticed of *heat* differences between water and the other tested substance (spirit of wine) or mercury). But if weight depended only on *temperature*, according to some equation as:

$$F = F_0.f(T)$$

Rumford's experiment would be unable to tell anything about $f(T)$. It would be necessary to compare a cold to a hot body, in order to notice any effect.

Notice also that in Rumford's experiments only the temperature of the attracted body changed. The attracting body (the Earth) was always at the same mean temperature. If there did exist a temperature dependence on attraction but if it were a function of both temperatures

$$F = F_0 f'(T_1,T_2)$$

it could happen that no effect would be observed when only one of the temperatures (and specially the temperature of the smaller body) is changed.

Now, in the case of Hick's work, both attracting bodies were at the same temperature. The effect was a very remarkable one (3%), while Rumford's result showed no (relative) change of weight greater than 10^{-6}. But the experiments are not comparable. Although they seem to clash, they are indeed compatible.

Finally, let us compare Hick's work to the measurements of Cornu and Baille. At first sight, they seem incompatible: if both effects are real, one shows a decrease and the other an increase of G as the temperature increases. However, Cornu and Baille's article does not provide detailed information and it is possible that their winter measurements were made in a heated room.

4.4 Geophysical studies

An apparent relationship between temperature and gravitation was fortuitously found in geophysical studies. After the development of accurate pendulum systems, gravity measurements were made as a routine at every place. In the 19th century, measurements were started inside deep pits, in order to detect the regular gravity decrease predicted by the Newtonian gravitational theory. Outside the Earth, gravity increases as we approach the ground; but inside the Earth, gravity decreases to zero as we approach the centre.

A strange result was however noticed: when three independent measurements made in Haton, Pribran and Freiberg were compared, it seemed that the gravity decreases were very different, in different pits, for the same depth (Sterneck, 1899). For each 100 m of depth increase, gravity diminished about 1.4×10^{-5} and 1.5×10^{-5} at Haton and Freiberg, but only 0.9×10^{-5} at Pribran. On the other side, temperature measurements were also made, and it was noticed that temperature varied much more inside the Freiberg pit (about 3.8° C for each 100 m) than at Pribran (about 1.4° C for each 100 m). As a matter of fact, there was a stronger correlation between gravity changes and temperature than between depth and gravity. It seemed that, inside the several pits, equal gravity corresponded to equal temperatures. Of course, the measurements were free from elementary systematic errors (beam dilation, etc.).

As this strange fact was noticed, the Vienna Academy of Sciences decided to study the problem, and Robert Daublebsky von Sterneck (1839-1910) was charged to measure gravity variations at four different pits (Sterneck, 1899). A slight correlation was found between temperature and gravity. For each 100 m depth increase there was a greater gravity decrease corresponding to greater temperature increases: a $-4.3\text{x}10^{-5}$ / °C relative change in *g*. But, as the mean errors were about $3\text{x}10^{-5}$, one could not ascribe much value to those results.

Later on, Sterneck's results were cited by Philip Shaw (Shaw, 1916) as providing an indirect evidence of an influence of temperature upon gravitational attraction. However, if the effects were not spurious, it would be necessary to suppose that slight temperature changes (a few degrees) could be able to produce enormous changes (about 5%) of the gravitational attraction of superficial layers of the Earth's crust. The previous results obtained with torsion balances was sensitive enough to rule out such effect.

5. BALANCE EXPERIMENTS: POYNTING AND SOUTHERNS

5.1 Poynting and Phillips

Hicks' work led, twenty years later, to two experimental searches for temperature influences on weight. The first one was done by John Henry Poynting (1852-1914) and Percy Phillips (c. 1880-c. 1825), in 1905. They dismissed Hicks' results as spurious, although without explaining them. After briefly describing the difficulties of repeating direct measurements of gravitational attraction (with torsion balances) at different temperatures, they choose another method: to search for a weight change when a body is heated or cooled (Poynting & Phillips, 1905).

The test body was attached to one arm of a balance; at the other side, a counterpoise was kept at a constant temperature. The difficult aspect of the experiment was to avoid spurious effects such as those above described: convection currents, radiometer pressure, and so on. The apparatus was carefully devised and deserves a short description (Fig. 1).

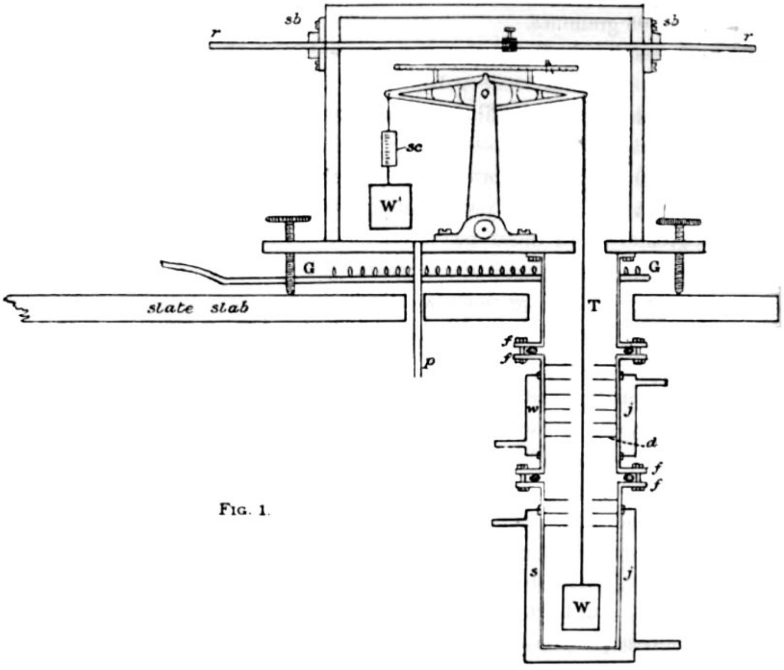

FIG. 1.

W, weight of which the temperature is to be raised, W′ counterpoise.
T, tube in which it hangs, with a number of diaphragms with ½-inch holes.
sj, steam jacket, replaced by liquid air jacket.
ff, flanged joint with lead washer.
wj, water jacket.
p, pipe to the exhausting pump.
sc, scale read by a microscope not shown.
rr, rider rod passing through stuffing boxes, sb, enlarged in fig. 2.
gg, gas burners to heat the base plate before sealing up.

Fig. 1. The apparatus used by Poynting and Phillips (1905, p. 448).

At the left arm of the balance a solid gun metal cylinder *W'* is hung by a short thin wire. *SC* is a scale that can be read by a microscope (not shown in the figure) in order to read the equilibrium position of the balance. From the right arm is hung the test body *W*, of the same weight (266 g) and material as *W*', hung to the balance by a long wire. The wire passes through several pierced screens designed to act as heat screens and convection holders.

The brass tube *T* where the test body *W* was enclosed was surrounded by two cooling (or heating) jackets. The upper one (*wj*) was a water jacket intended to keep the upper part of the tube at room temperature. The lower one (*sj*) was intended to produce temperature variations in *W* by passing either steam, or water, or liquid air.

The whole apparatus was air-tight and during the measurements the system was evacuated by the pipe *P* until the air pressure reached a fraction of a millimeter of mercury. This was done in order to avoid any spurious effects due to convection or air thrust.

The experiment consisted of observing eventual weight changes when the temperature of *W* was changed. When steam of liquid air were used to heat or cool down the tube *T*, large effects were observed, but they disappeared as soon as the temperature became constant. There remained a small weight decrease, when the test body was heated by steam (to 100° C): a mean change of -0.055 mg (of othe total 266 g). When it was cooled by liquid air (to -186° C) the observed change was only 0.0016 mg. The authors suspected that the measured amount was due to some systematic error and tried to detect it using *hollow* test bodies: any superficial effect (such as residual air convection, radiation or radiometric pressure, etc.) should be the same with solid and hollow test bodies; but real weight changes should be different.

Changing *W* an *W*' by similar hollow cylinders (58 g) of the same volume, size and material as the first ones, the measurements were repeated. The observed weight changes were very similar to the preceding ones (-0.058 mg and 0.0007 mg, respectively). Therefore, if there was any real weight change it could amount to only 0.003 mg (when steam was used) or 0.001 mg (when liquid air was used). The authors conclude that, if there is any influence of temperature on weight, it must be less than one part in 10^9 per 1 ° C.

5.2 Southerns

The experiment made by Leonard Southerns (1878-1962), although published two years latter than Poynting's (Southerns, 1907), was an independent research, using an apparatus built by Hicks. There was one very important difference: instead of external heating of the test body, as used by Poynting and Phillips, Southerns used a well insulated weight that could be rapidly heated from inside, by an electric current. In this way, most of the spurious effects could be avoided, because the external surface of the body would become hot only after some time delay. The experiment was made in reduced pressure conditions.

No regular effects were observed, although some transient variations, during and shortly after heating, were observed. The temperature differences were smaller than in Poynting's experiments and the author concluded that if there is any influence of temperature upon weight, it is less than 1 part in 10^8 per 1° C. Although Poynting's work had produced a better result, Southerns' experiments are relevant as bringing independent confirmation of this null result.

6. THE DETECTION OF A POSITIVE EFFECT: SHAW

After all those attempts, it might seem that it was useless to make other tests. But Poynting's and Southerns' experiments could only change the temperature of the attracted test body – not of the attracting body (the Earth). Therefore, it was desirable to study a situation where the temperature of the attracting body could be controlled. This kind of test was done by Philip Egerton Shaw (1866-1949), and, as will be described, it led at first to the observation of a positive effect. His final results were stated thus: "The conclusion is that there is a temperature effect of gravitation. When one large mass attracts a small one, the gravitative force between them increases by about 1/500 as the temperature of the large mass rises from, say, 15 °C to 215 °C" (Shaw, 1916a).

Shaw's main article on this subject (Shaw, 1916a) was a massive 44 pages long paper published in the *Philosophical Transactions of the Royal Society* for 1916. The article was presented by Sir Charles Vernon Boys (1855-1944) – the man who had greatly improved the measurements of gravitational attraction, 20 years earlier (Boys, 1894). Both the names of Boys and of the Royal Society helped to give a great impact to this article.

Shaw's paper is very well written. It begins by discussing previous theoretical and experimental knowledge of the subject; describes his own careful method, the apparatus, observations and results; and presents a final discussion and conclusion.

Shaw provided an interesting review of previous evidence both for and against the temperature effect. Besides discussing the contributions of several of the authors referred to in the present paper, he analysed three indirect evidences:

i) There are two different methods of measuring G (or the mean density of the Earth): either by the use of the torsion balance, or by the measurement of the gravitational effect of large masses of the Earth's crust (either mountains, or making measurements in mine pits, for instance). Shaw pointed out that the temperature of mountain masses and superficial shells of the Earth's surface is above ordinary laboratory temperatures and that these "Earth" methods lead to a mean density of the Earth of about 5.4 g.cm^{-3}, while laboratory measurements lead to a value of 5.51 g.cm^{-3}. This difference could be interpreted as an *increase* of gravitational force with temperature.

ii) Shaw analysed Boys' experiments in the same way as Hicks had analyzed Baily's. He found a seemingly regular rise of G (or a decrease of the mean density d of the Earth) as the temperature increased. The change was about 1/1,000 for a rise in temperature of 1 °C.

iii) Measurements of *G* in mines, made by von Sterneck, had shown different results in mines with different temperature gradients. Shaw analysed the data and showed that they seem to point again to an increase of gravitation with temperature.

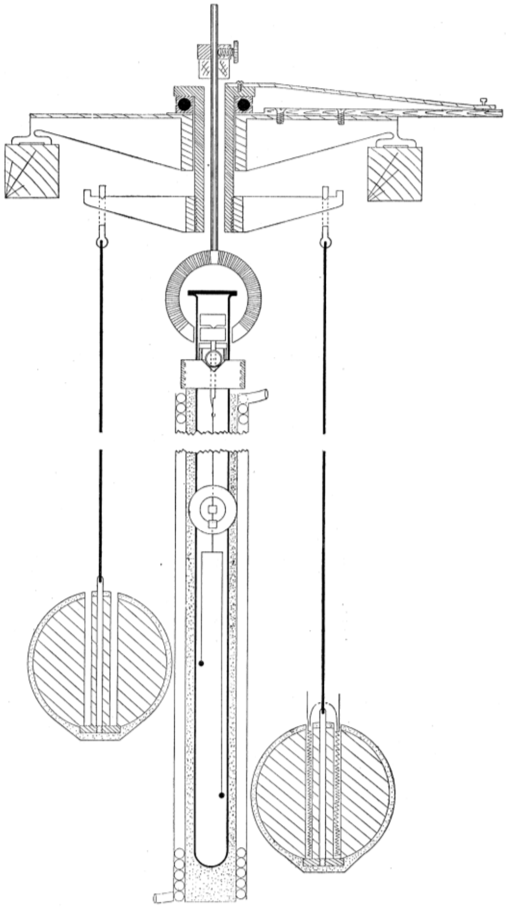

Fig. 10. Shows the final apparatus ($\frac{1}{8}$ full size) with limp suspension. This is the best arrangement. For clearness no lagging outside the water jacket is shown. The large amount cut out half way up the diagram will be appreciated by comparison with fig. 9, which has identical essential parts.

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Fig. 2. The final form of the apparatus used by Shaw (1916a, p. 365).

Shaw decided to make an experiment measuring the force of attraction using a torsion balance (Fig. 2). The attracted masses were kept at the same temperature, inside an insulated case. Only the attracting bodies were heated and cooled. Their temperatures varied from room temperature (about 15 °C) to more than 200 °C. He carefully described several precautions and possible sources of error. He worked for eight years, improving the apparatus and making several tests. After a series of measurements, Shaw concluded that there is a regular increase of gravitational attraction of about 1.2×10^{-5}/°C, when the attracting body is heated.

7. REACTIONS TO SHAW'S RESULTS AND FURTHER EXPERIMENTS

A strong reaction against Shaw's results appeared very soon in *Nature*'s "Letters to the editor". From June 1916 to April 1917, when the discussion disappeared, 13 letters were published on this subject, by Joseph Larmor, Edwin Barton, Frederick Lindemann and Charles Burton, and George Todd, with replies by Shaw (Barton, 1916, 1917; Larmor, 1916a, 1916b, 1917; Lindemann & Burton, 1917; Lodge, 1917; Todd, 1917a, 1917b; Shaw, 1916b, 1917a, 1917b, 1917c). As it usually happens in those cases, the discussion was limited to theory: nobody criticized, made suggestions or repeated Shaw's experiments. The discussion exposed a general confusion between several mass concepts – specially those now called "passive" and "active" gravitational mass. At the end of the discussion, as at the very beginning, it was not clear whether Shaw's results were compatible or not with the basic laws of physics, or what change they would entail, if confirmed.

Meanwhile, Shaw, with the assistance of Cecil Hayes, repeated the experiment (Shaw & Hayes, 1917) in order to answer to a criticism presented at the meeting of the Royal Society, when the firs paper was read. It was suggested that convection currents around the large attracting body could change its position (towards the attracted bodies) and produce an apparent increase of gravitational force. A displacement of only 0.15 mm would be sufficient to account for the observed effect. In order to check this possible source of error, Shaw devised a method to measure the precise position of the attracting masses. He observed a displacement of the hot bodies – but it was an *outwards* displacement, of about 0.01 mm. This could not account for the observed effect. Indeed, this displacement showed that the effect was greater than that previously ascertained. As a result of this test, Shaw presented the result:

$$a = + (1.3 \pm 0.05)\text{x}10^{-5}/°\text{C}$$

for the temperature coefficient of gravitational attraction (a).

At the presentation of this second paper at the Physical Society, a few experimental doubts and suggestions were made by Charles Vernon Boys, Frederick Escreet Smith and Clifford C. Paterson, but they were easily replied by Shaw (Shaw & Hayes, 1917, p. 170). Nevertheless, Shaw perceived that a possible source of error was the displacement of either the large or the small masses – and decided to improve his apparatus.

After six more years of work he presented his final paper on this subject (Shaw & Davy, 1923a, 1923b). With the assistance of Norman Davy (1893-1973), the apparatus was improved.[7] The supports and suspending systems of the masses were changed. The experiment was carried on, and the observed effect was now very small: about $0.2\text{x}10^{-5}$ / °C, or possibly zero. Shaw's final conclusion was that, for the studied temperature range (from 0 °C to 250 °C) there is no temperature effect and G is constant.

No other papers were published on this subject, after this one. From this time onwards, it was generally agreed that gravitation does not depend on temperature – or, at least, that no large effect exists.

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[7] I was unable to consult Norman Davy's MSc dissertation on this subject: *The effect of temperature on gravitative attraction*. University of London, 1929. It probably presented new improvements and results, but no paper was published afterwards containing those final researches on the subject.

8. CONCLUSION

At first sight, this long and arduous search led to nothing. But this is not strictly true. A null result is not equivalent to no result. Even before Shaw’s experiments, several scientists supposed that the measured effect was spurious; nevertheless, Lindemann and Burton commented:

> In conclusion, we should like to express our admiration for Dr. Shaw’s experimental work. We feel that as the result of such an elaborate research a null result is quite as important as, if less sensational than, a positive one. To have reduced -3the apparent temperature coefficient of gravity from the 10 deduced from Prof. Boys’ measurement to 1/80 of that value is certainly no mean achievement (Lindemann & Burton, 1917).

It is possible to list several methodological rules that were fulfilled by those researches and that show their scientific value:

a) Once someone (or several persons) belonging to the scientific community strongly suggest the possible existence of a measurable effect, it is desirable to search for such an effect. If it is found, good: a new physical phenomenon was discovered. If it is not found, this is also a valuable result, as it will bind speculative theory building and increase our knowledge about some constancies or conservations in nature.

b) Even if no theory suggests some correlation or effect, it is desirable to look for them by increasing the sensitivity of measurement procedures, by varying the conditions and bu producing greater changes of the independent parameters. The results of this search are of the same kind as in the former case.

c) In any circumstances, it is also desirable to develop new experimental techniques that allow the study of new situations, new effects, new materials, with increased accuracy and sensitivity, and so on. It is also desirable to suggest, analyze and test possible systematic errors in previously developed techniques, trying to measure, compensate and eliminate such errors, if they exist.

All those *desiderata* were fulfilled in the search for a temperature influence upon gravitation. They were certainly useful, increasing both our knowledge about nature and by the development of new techniques. One might criticize those researches by pointing out that too much time and money was lost and no effect was found. But in any relevant scientific research (an specially in fundamental research) it is not possible to know beforehand what the result will be like. Had Hicks' and Shaw's first results been confirmed, this would have been a very important discovery.

It is also relevant to remark that, after the general acceptance of General Relativity, there were strong theoretical grounds for stating that no large temperature influence on gravitation would exist. Indeed, since the source of gravitational effects, within General Relativity, is the stress-energy tensor, temperature may affect gravitational forces, since by varying temperature the energy of the field producing body may change. The effect will, of course, depend on the substance and on several other factors. For water, the effect should be about $4.6\text{x}10^{-14}/°\text{C}$ – that is, eight orders of magnitude below the sensitivity of Shaw's experiments. It is very likely therefore that the acceptance of General Relativity in the 1920's (specially after the eclipse light-deflection test and the agreement between theoretical and empirical astronomical red-shifts) was a decisive reason for the disappearance of this kind of experimental research.

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