The Search for Gravitational Absorption in the Early Twentieth Century

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Unlike any other known physical influence, it [gravitation] is independent of medium, it knows no refraction, it cannot cast a shadow. It is a mysterious power, which no man can explain; of its propagation through space, all men are ignorant.
Charles Boys (1894)

THE RECEIVED HISTORICAL VIEW OF GENERAL RELATIVITY tells us that, at the end of the nineteenth century, physicists accepted Newton's law of gravitation: there is an attractive force between any pair of bodies in the world, and this force depends only on the masses of those bodies and on their mutual distance. Newton's gravitational law had been able to explain very well the motion of the planets. It had also been confirmed in the laboratory. Something, however, could not be explained: there was an anomaly in the motion of Mercury that seemed incompatible with this theory.

Soon after the development of special relativity (in 1905), Albert Einstein began to study gravitation. In 1916 he was able to formulate the so-called "general theory of relativity." This theory explained the anomalous precession of Mercury's perihelion. Besides that, it predicted two new effects: a wavelength increase of the spectral lines of the Sun due to its gravitational field; and the deflection of star light when it passes near the limb of the Sun, during eclipses. Those effects were confirmed, and hence in the early 1920s general relativity replaced Newtonian gravitation theory.

That, of course, is just a textbook version of what happened. Historians of physics know that things did not really happen that way. General relativity was indeed the successful gravitational theory but many other theories were proposed in the late nineteenth and early twentieth centuries. The three 'classical tests' of general relativity are very significant, but there were several other anomalous (non-Newtonian) astronomical and terrestrial gravitational effects that deserved attention in the early twentieth century. Indeed, if one consults scientific journals of the late nineteenth and early twentieth century, one finds a large number of revolutionary studies on gravitation. Many of them proposed alternative gravitational

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theories. Besides theoretical papers, one finds a lot of experimental work guided by those alternative theories. Those works can be collectively called 'the search for non-Newtonian gravitational effects.'

Since the publication of Whittaker's book (1951–53), historians have been well aware of alternative gravitational theories in the early twentieth century. Two more recent book-length studies covering this period have been published: Woodward (1972) studied the history of gravitational models from Newton's time to the 1920s; Roseveare (1982) discussed gravitational theories from the late nineteenth century to general relativity. Gillies (1987) published a fairly complete bibliography of experimental studies of gravitational force up to the time of his compilation, including most relevant empirical work of the turn of the century.[1] Of course, it is impossible to describe all gravitational research of that period in a single paper. Among all those works searching for non-Newtonian effects, this paper will discuss only *gravitational absorption*.

1. The meaning of gravitational absorption

During the eighteenth century, after the general acceptance and diffusion of Newtonian theory, gravitation was generally regarded as an inexplicable phenomenon. It was accepted as an immediate *action at a distance*.[2] It did not depend on anything that might exist between the attracting bodies. Newton's law of gravitation was very simple, and there was only a single kind of relevant gravitational experiment: to test whether that law was valid or not in the laboratory, measuring the force between two bodies. Of course, this state of affairs did not stimulate any other gravitational experiment.

However, it is also possible to conjecture that gravitation is an interaction requiring some mediate cause in the intervening space. It is well known that Newton himself speculated about the mechanism of gravitation.[3] In his Trinity notebook, after sketching the ether-stream model of gravitation, Newton suggested several relevant experiments that should be tried to test this model:

> Try whether the weight of a body may be altered by heat or cold, dilatation or condensation, beating, powdering, transferring to several places or several heights, or placing a hot or heavy body over it or under it, or by magnetism. Whether lead or its dust spread abroad is heaviest. Whether a plate flat ways or edge ways is heaviest. Whether the rays of gravity may be stopped by reflecting or refracting them.[4] (Newton, cited McGuire & Tamny 1983: 431)

The precise way of understanding and testing the existence of gravitational absorption depends on the chosen gravitational model. According to Newton's

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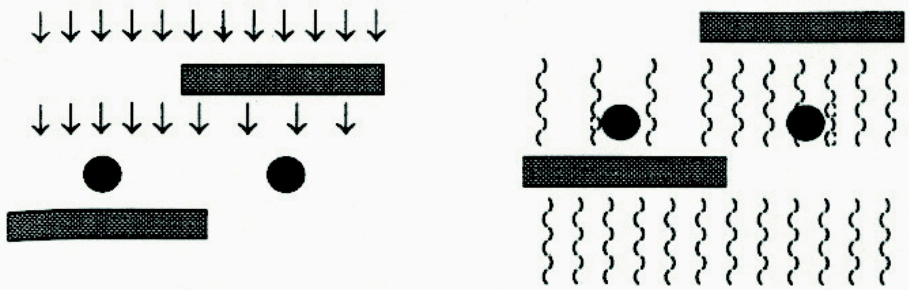
1 The same author has also compiled a more popular bibliography (Gillies 1990)

2 See Fink 1982. There were exceptions, of course, see Woodward 1972: 58–89.

3 Aiton 1969; Hawes 1968; Rosenfeld 1969.

4 It is not known whether Newton checked all those consequences of his early ether model or just thought about them. In his mature gravitational work, nothing of this sort can be found.

early (pre-gravitational) ether stream model, gravity would be produced by a steady flow of ether towards the centre of the Earth. In that case, the weight of a body under a thick roof would be expected to be smaller than outside the cover (Figure 1). On the other side, a thick slab of matter below a body would not change its weight.[5] The total weight of two bodies placed side by side would be larger than that of the same two bodies placed one over the other, because in the second case the upper body would reduce the weight of the lower body.



(a) *(b)*

Figure 1. Two models of gravitational interaction.

The way of testing the existence of gravitational absorption might depend on the model of gravitational interaction that is assumed. *(a)*. If gravity was produced by something coming from space towards the Earth, and if this something could be absorbed by matter, the weight of a body would be reduced by a shield placed above the test body. *(b)*. If, on the other side, gravity was produced by "something" flowing from the Earth towards space (for instance, some kind of gravitational wave), the weight could be reduced by a shield placed below the test body.

Descartes' vortex model, on the other side, explained gravity as being a hydrostatical thrust due to the motion of a subtle matter circulating around the Earth.[6] In this case, a thick roof would produce no weight change, but thick walls *around* a test body would reduce its weight.

Any experimental test of gravitational absorption should take into account the gravitational attraction of the slab of matter that will be checked for its absorption effect. According to the standard (post-*Principia*) Newtonian theory of gravitation, the weight of a body under a thick roof would be reduced, of course—due to the gravitational attraction produced by the roof, and not because of any absorption effect. Therefore, in order to detect the absorption effect it is necessary either to compute (or measure) and take into account such attraction effects, or to eliminate them by a suitable symmetrical disposition of matter around the test

[5] Those predictions, of course, do not take into account the gravitational attraction produced by the matter slabs. In his early speculations on gravity, Newton did not suppose that there was such an attraction.

[6] The models proposed by Huygens and Leibniz, after publication of the *Principia*, also invoked the motion of matter around the Earth.

body (Figure 2). For instance: a hollow box (of spherical, cylindrical or cubic shape) will produce a null gravitational attraction upon a test body placed at its centre. However, if gravity were due to the flow of 'something' between the Earth and the test body, or to something that flows from the space towards the Earth, the walls of the box could reduce that flux and decrease the weight of the test body.

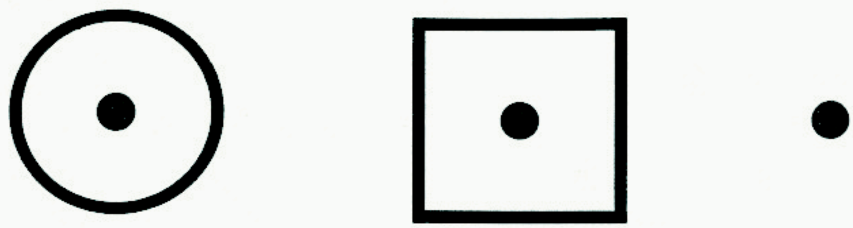


Figure 2. Gravitational shielding.

Independently of the specific model of gravitation that is assumed, if gravitational absorption by matter exists, the gravitational attraction acting upon a body should decrease when it is surrounded by a symmetrical shield. The gravitational attraction of the shield itself upon the test body will be null, in this case.

In the late nineteenth century, there appeared many hypotheses about the cause of gravitation. It was suggested that gravitation could be due to very fast particles traveling throughout all space, or to penetrating ether waves, or to a secondary effect of electromagnetic forces, and so on. In this context, there appeared many suggestions about possible new gravitational phenomena—and speculation led to a rich *experimental* investigation of all sorts of suggested effects.[7] There was no predominant alternative to the action-at-a-distance theory of gravitation. In some cases it was impossible to identify the specific model or hypothesis that suggested the experimental work.

This paper will shortly discuss the main experimental and observational work of this period on gravitational absorption and related effects, with stronger emphasis on the decade following 1910.[8] Laboratory experiments will be described first (Sections 2 to 5), followed by studies of gravitational absorption related to the study of the anomalous motion of the Moon (Sections 6 to 11).

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[7] In 1881, Preston urged the search for new gravitational effects that were suggested by the kinetic theory of gravitation; he wondered "if certain small specific variations may not have escaped notice, owing to their *not having been searched for*, on account of the bias of preconceived ideas" (Preston 1881: 393). For a general view of the astronomical and experimental situation in the early twentieth century, one may consult Poynting 1900, Zenneck 1901 and Oppenheim 1920.

[8] For a general review of theories of gravitation at the end of the nineteenth century, see Drude 1897 and Taylor 1877.

2. Early experiments (to 1910)

Louis Winslow Austin and Charles Burton Thwing (University of Wisconsin) made the first known experimental attempt to test the existence of a change of gravitational force due to interposed matter (Austin & Thwing 1897). They did not present any specific theory of gravitation as a motivation of their search for a new effect. It seems that they were guided by a bare analogy to electromagnetism: electric and magnetic attractions between two bodies are affected by an intervening medium. Could there exist a similar effect for gravitation?

Austin and Thwing studied the effect of interposing screens of different materials between the attracting bodies in a torsion balance (Figure 3). They tested lead, zinc, mercury (because of their high densities), water, alcohol and glycerin (for their high dielectric constants) and iron (because of its high magnetic permeability). No significative change was observed. The experimental error was about 2×10^{-3}. Those experiments were not very sensitive, but they were significant because the authors were looking for something similar to the influence of the medium on electromagnetic forces. By analogy, one could expect that any effects of this kind would be much larger than 1/500.

At the beginning of the twentieth century, some researchers investigated a hypothetical relation between gravitation and radioactivity. The discovery of radioactivity led to the suspicion that several basic physical laws should be changed (Lodge 1912). It was very hard to explain the continuous emission of energy by radioactive substances. One of the several suggested explanations was that radioactive bodies obtain their energy from the gravitational field; therefore their weight should exhibit some kind of anomaly.[9]

The Curies and several other researchers of the time believed that the energy emitted by radioactive bodies came from outside. In their report to the International Congress of Physics of 1900, Pierre and Marie Curie suggested the following explanation:

> According to what has just been said, one could consider the Becquerel rays as a secondary emission due to rays analogous to X rays, that travel through all space and through all bodies.[10] (Curie & Curie 1900: 114)

A similar view was entertained by Lord Kelvin:

> It seems to me, therefore, absolutely certain, that if emission of heat [by radioactive bodies] . . . can go on for month after month, energy must somehow be supplied from without to give the energy of the heat getting into the material of the calorimetric apparatus. I venture to suggest that somehow ethereal waves may supply energy to the radium while it is giving out heat to the ponderable matter around it. (Thomson 1903: 537)

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9 Other explanations included violation of the conservation of energy, or absorption of thermal energy of the environment and emission of that energy under a new form, in violation to the second law of thermodynamics.

10 "Conformément à ce qui vient d'être dit, on pourrait considérer les rayons de Becquerel comme une émission secondaire due à des rayons analogues aux rayons X traversant tout l'espace et tous les corps."

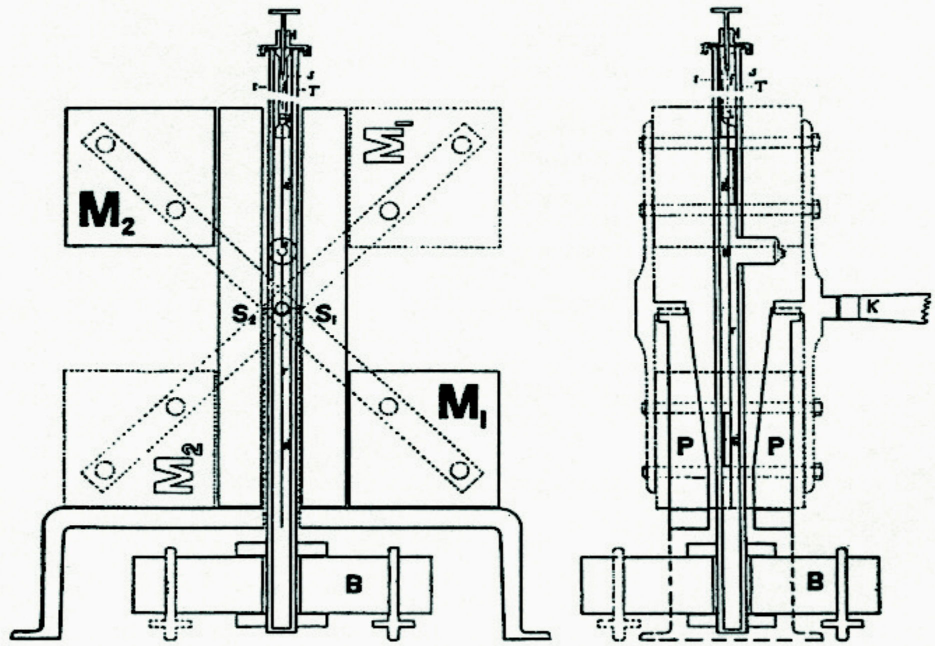

Figure 3. Two vertical sections of Austin and Thwing's apparatus (1897).

Two gold attracted masses (m_1 and m_2) were attached to a glass tube (r). This system was enclosed in a double metallic tube, with paraffin between its walls, to prevent air currents. The attracting masses were lead cubes ($\mathbf{M_1}$ and $\mathbf{M_1}$) that could be moved so as to invert the direction of attraction. The interposed screens ($\mathbf{S_1}$ and $\mathbf{S_1}$) were 3 cm thick, 10 cm wide and 29 cm high.

A few authors connected radioactivity to gravitation. Arthur Schuster (1903) recalled Le Sage's theory of gravitation. In this theory, space is filled with very fast corpuscles that push bodies towards one another. This hypothesis had been revived by Lord Kelvin, but had been objected to by Maxwell, who pointed out that the collisions of corpuscles with matter should produce heat. Schuster remarked that in radioactive bodies we do observe a rise of temperature that could not be explained by known causes. Perhaps it was the effect of Le Sage's corpuscles.

In this context, it was natural that a study was made of the gravitational properties of radioactive bodies.

Adolf Heydweiller (1856–1926) was the first to test the constancy of weight of radioactive substances (Heydweiller 1902). He described a significative weight change of radioactive substances in a few weeks (0.5 mg weight reduction of a 5 g sample). Heydweiller explained the effect as due to the transformation of gravitational energy into radiation. The effect was not confirmed by Dorn (1903).

Robert Geigel conjectured that radioactive bodies might exhibit a strong absorption of gravitational energy, transforming it into radiation. He tested this hypothesis and detected a significative reduction of the weight of a test body (40 μg in 6.5 g) when he put a radioactive material below it (Geigel 1903). The experiment was repeated (Forch 1903a, 1903b and Kaufmann 1903) and was not confirmed.

A few years later, a series of investigations on gravitational absorption was developed in Zurich, under the guidance of Alfred Kleiner. Two of his students (Fritz Laager and Theodor Erismann) and Kleiner himself tried to detect changes of gravitational attraction due to a shield.

Their work was motivated by the previous investigation of Austin and Thwing. Kleiner noticed that the geometry used by Austin and Thwing was not very convenient because the screens could produce forces upon the test bodies when they were not exactly in the middle position. In order to avoid this problem, Laager (1904) used screens in the form of cylindrical shells (Figure 4).

Laager initially observed noticeable effects, but soon detected experimental problems that made his experiment inconclusive. The experiment was repeated by Kleiner (1905) and by Erismann (1908, 1911) using spherical shields (Figure 5). No effect greater than experimental errors (of about 10^{-3}) was observed.

In 1905, Victor Crémieu reported the first positive result (Crémieu 1905a, 1905b, 1905c). He compared the gravitational attractions between bodies in air and in water (Figure 6). There seemed to be an increase of the gravitational attraction in water, of about 7%. Further repetitions of the experiment in improved conditions led to smaller effects of about 2–5% (Crémieu 1906, 1907). After several years of delicate researches, however, Crémieu himself detected a major source of error in his experiments (Crémieu 1910, 1913, 1917a, 1917b). He finally concluded that there was no measurable effect.

All those early attempts to detect a change in gravitational attraction tried to observe what could be described as large, easily detectable changes (10^{-2} to 10^{-3}, except for the radioactivity experiments). They could not detect smaller effects.

Around 1909, Roland von Eötvös also investigated the absorption of gravitation by matter.[11] He used a very sensitive method using a torsion balance and a device called a "gravitational compensator" (Figure 7).[12] He observed that a thickness of 10 cm of lead produced a relative gravitational absorption smaller than 2×10^{-11}(1/50 000 000 000). Eötvös also tried to detect any gravitational anomaly associated with radioactivity. He searched for absorption of gravitation and violation of the principle of equivalence, by radioactive substances. The outcome was negative.[13]

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[11] Those investigations, made with the collaboration of Eötvös's students Jenö [Eugen] Fekete and Dezsö [Desiderius] Géza Sándor Pekár, were published posthumously (Eötvös, Pekár & Fekete 1922). The introduction to the article states that its contents had been written in 1909, and that it had obtained a prize from Göttingen University.

[12] This instrument had already been described by Eötvös in a previous paper (Eötvös 1896). Its original aim was to increase the sensitivity of the balance.

[13] Zeeman (1918) also used a torsion balance to test the same effect, with a similar result. He observed no violation of the principle of equivalence between inertial and gravitational mass for uranila nitrate, with a sensibility of 5×10^{-6}.

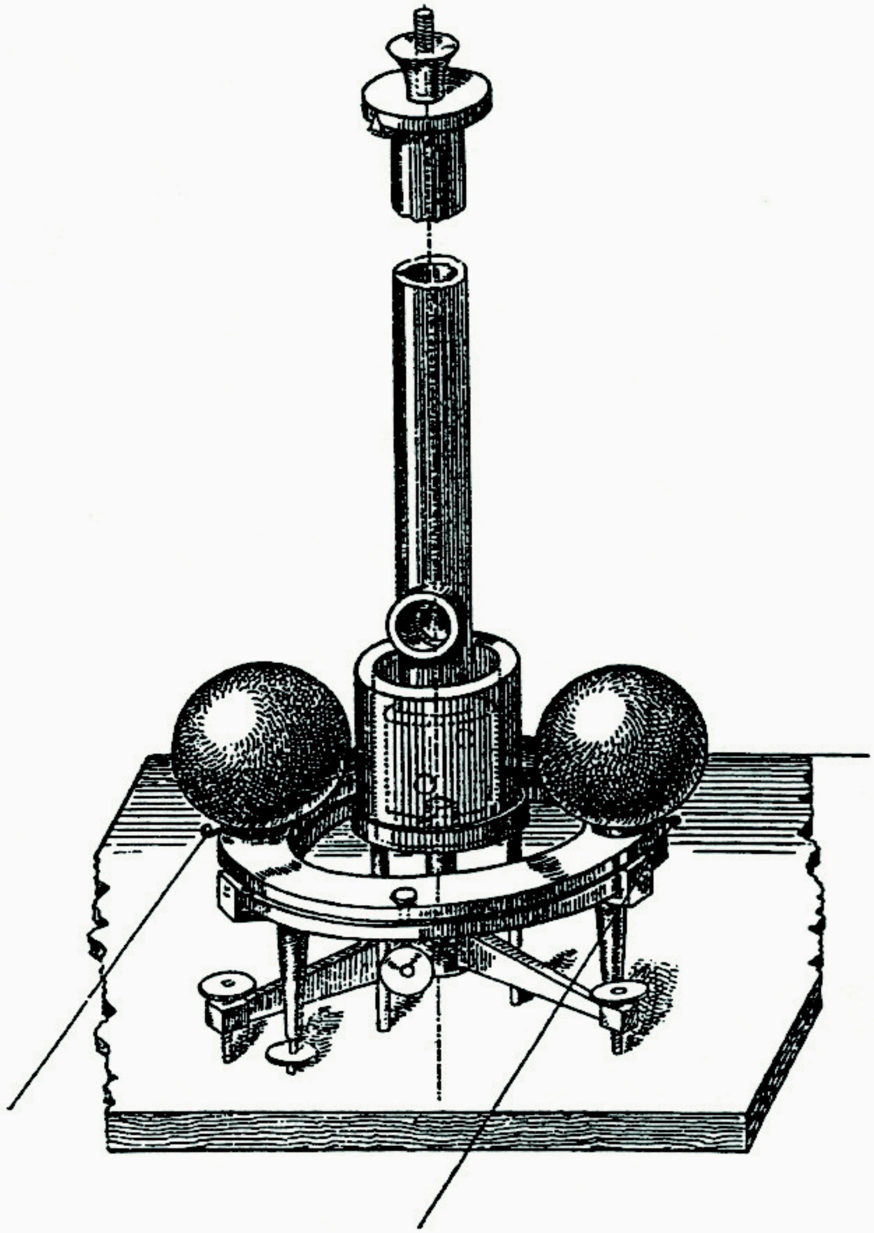

Figure 4. Laager's apparatus (1904).

The torsion balance was enclosed inside a vertical cylinder. The experiment tested the screening influence of this cylinder on the attraction between external bodies and test bodies. The external attracting masses could be moved to two different positions in order to produce a variation of the direction of the gravitational force. Attraction was measured with and without the screen.

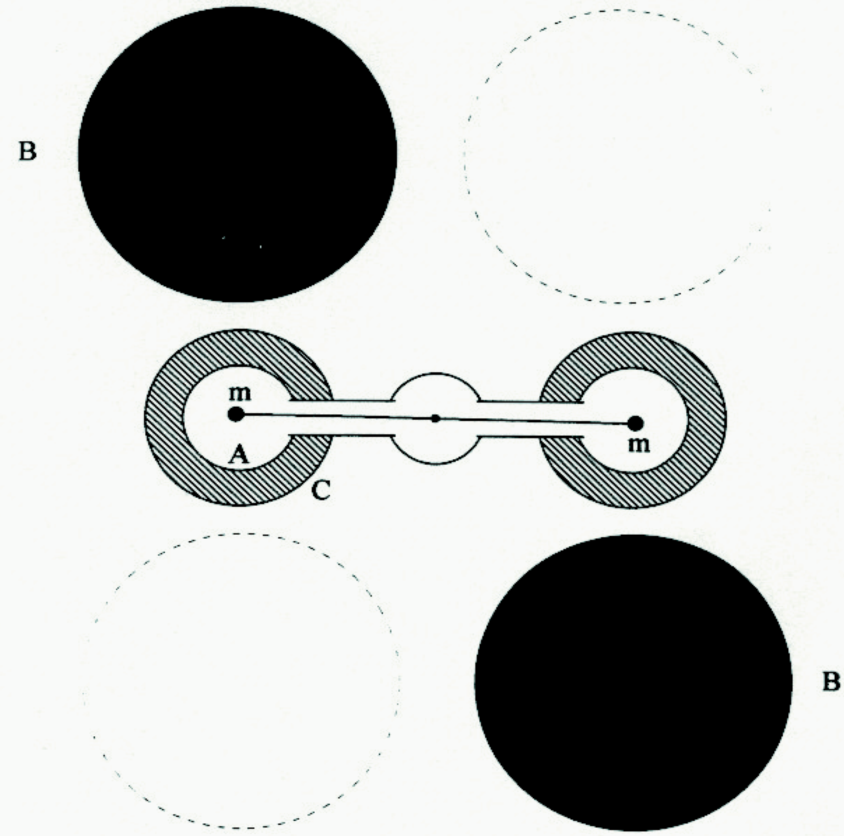

Figure 5. Erismann's apparatus (1908–1911).

This was a modified version of Laagers balance. Each test body (m) was surrounded by a double spherical shell. The inner spherical shell (A) was made of aluminum, the outer one (C) was made of copper. The space between the two metal sheets could be filled with different liquids. Inside the inner shell, a vacuum was made. The attracting spheres B, B could be moved to a different position.

3. Majorana's experiments: positive results

For about ten years, there were no further experiments on the subject. At the end of the 1910s, the Italian physicist Quirino Majorana (1871–1957) brought the search for gravitational absorption back to the laboratory. His experiments on gravitational absorption seem the best ever made. Majorana published the details of his work in several publications in Italian scientific journals (Majorana 1918/9, 1919/20a, 1919–20b, 1921–22). He also published shorter accounts of his researches in French (Majorana 1919a, 1919b, 1921, 1930) and in English (Majorana 1920).[14]

[14] For a modern description of Majoranas work, see Dragoni & Maltese 1994.

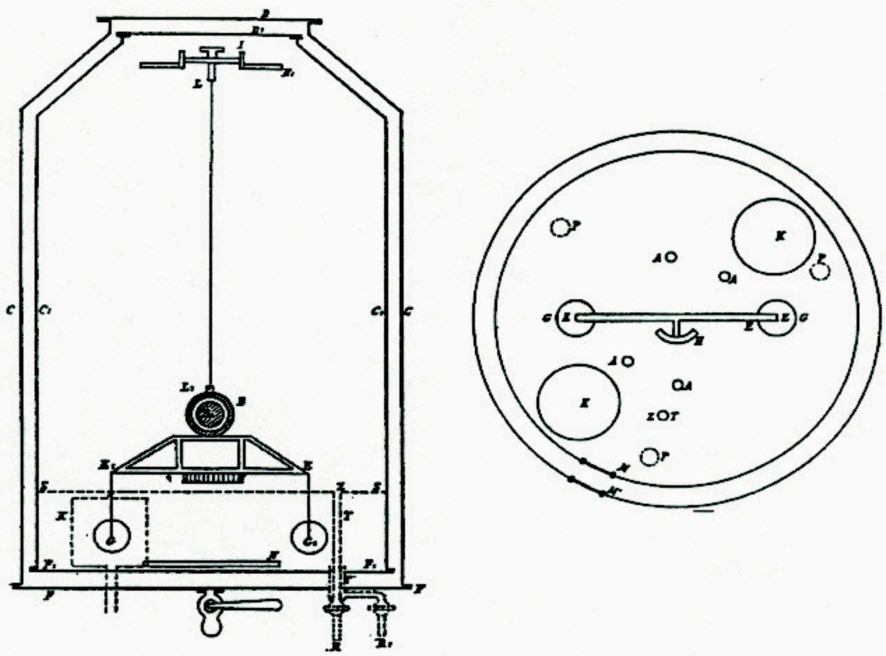

Figure 6. Crémieu's balance (1905–1917).

Test bodies G, G_1 attached to the balance beam EE_1 moved inside water that attained the level SS. Attraction produced by cylinders K, K was measured. Those cylinders were hollow and could be filled with mercury. A double metallic screen CC_1 filled with water helped to protect the balance from temperature changes. The experiments were made in a cave, far from disturbing vibrations.

Majorana conjectured that gravitation was due to the flow of gravitational energy from all bodies to the surrounding space. He also supposed that matter is not transparent to gravitational flux. According to him, gravitational energy can be absorbed by matter and transformed into heat. Therefore, each body would undergo a "spontaneous" heating. This effect would be noticeable only for large bodies, because the generation of heat would be proportional to the volume, and emission of heat proportional to the surface of the body. This process would account for stellar energy.[15]

Majorana knew that previous experimenters had tried to detect large variations of gravitation due to interposed matter and had found nothing.[16] For that reason he believed that only the search for a very weak gravitational absorption could lead to positive results.

[15] This idea was not developed in Majorana's early works. It was discussed, however, many years later (Majorana 1954).

[16] Majorana was aware of the experiments of Austin and Thwing, Kleiner, Laager, Crémieu and Erismann.

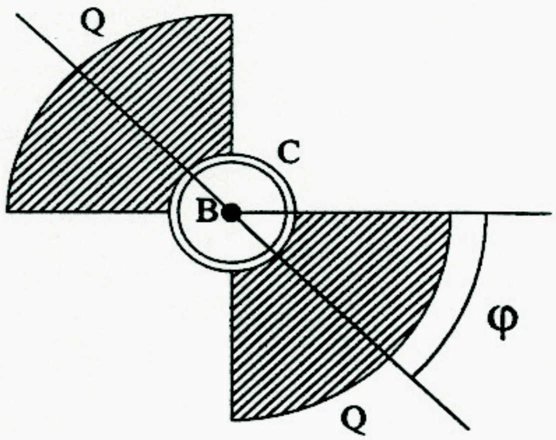

Figure 7. Eötvös' gravitational compensator (1909).

The apparatus was built of two lead masses of equal form, size and weight, symmetrically placed around each of the test bodies of a torsion balance. The test body (B) oscillated inside a hollow cylinder (C) with a diameter of 5 cm. Outside this cylinder, there were two opposite cylindrical quadrants ($Q\,Q$) of cast lead. The angle between the horizontal direction and the line that bisects the quadrants (φ) could be changed. By turning the gravitational compensator around a horizontal axis, the lead bodies could either be interposed between the test body and the Earth, or leave a free way between the test body and the ground. The experiment tried to detect any change of the weight of the test body when the gravitational compensator was turned.

He developed a theoretical analysis to evaluate the upper order of magnitude of the effect that was to be searched for (Majorana 1919/20a, 1919–20b). Let us suppose a homogeneous material medium. According to the simplest absorption hypothesis, a body of mass M placed in this medium would produce at the distance r a gravitational field g equal to:

$$g = \left(\frac{GM}{r^2}\right) e^{-Hr},$$

where H is the characteristic gravitational absorption constant of the medium. Majorana assumed that H does not depend on the chemical composition of the medium. It would be proportional to its density.

Suppose now a homogeneous sphere of radius R. Due to self-absorption of gravitation, the external field of this sphere would correspond to an apparent active gravitational mass M_a different from the sum of the gravitational masses of its parts. If ρ_v is the 'real' density of the sphere, its 'real' mass M_v is simply:

$$M_v = \frac{4}{3}\pi \rho_v R^3,$$

but its 'apparent' or 'effective' active gravitational mass M_a will be equal to ψM_v, where ψ is a factor that takes into account the self-absorption of gravitation.

Majorana computed this factor for a homogeneous sphere and found:[17]

$$\psi = \frac{3}{4}\left\{\frac{1}{RH} - \frac{1}{2}\frac{1}{(RH)^3} + \left[\frac{1}{(RH)^2} + \frac{1}{2}\frac{1}{(RH)^3}\right]e^{-2RH}\right\}.$$

As described above, Majorana assumed the absorption constant H to be proportional to the density of matter, $H = h\rho_v$, where the parameter h was supposed to be a universal constant.

Applying the above computations to the Sun, Majorana was able to evaluate an upper limit to h. The effective or apparent active gravitational mass of the Sun is known from its effect upon the planets. From this effective gravitational mass, it is easy to compute that the medium effective density of the Sun is about 1.41 $\mathrm{g\,cm^{-3}}$. If there is gravitational absorption, the Sun's real mean density must be greater than the above value.

Although the Sun is not homogeneous, Majorana applied the model of a homogeneous sphere to this case. Using values of true density larger than 1.41, he computed by successive approximations the corresponding values of h:

ρ_v ($\mathrm{g\,cm^{-3}}$)	ρ_a/ρ_v	h ($\mathrm{cm^2\,g^{-1}}$)
1.41	1.000	0
2.0	0.705	3.81×10^{-12}
5.0	0.281	7.08×10^{-12}
10	0.141	7.49×10^{-12}
15	0.094	7.63×10^{-12}
20	0.070	7.64×10^{-12}

This computation led to an unexpected result: if the true density of the Sun is supposed to increase to infinity, the absorption constant h approaches a finite value: $7.65 \times 10^{-12}\ \mathrm{cm^2\,g^{-1}}$. That is, if a simple model is applied to the Sun, its known apparent active gravitational mass imposes an upper limit to the value of gravitational absorption. Of course, the Sun is not a homogeneous sphere. However, even with this simple model, it is remarkable that Majorana could reach an upper limit for the constant of gravitational absorption.

Could such a small effect be detected? A simple computation will show that in laboratory conditions the effect would be very small indeed. As a first approximation, the weight of a body inside a spherical shell would suffer a relative reduction of about $hr\rho$, where r is the thickness of the shell. As an instance, take $\rho = 13.6\ \mathrm{g\,cm^{-3}}$ (mercury), $r = 10$ cm and $h = 10^{-11}\ \mathrm{cm^2\,g^{-1}}$. The relative weight reduction would amount to 1.36×10^{-9}, that is, a reduction of about 1 μg

[17] In one of his papers, Majorana presented a different result (Majorana 1919–20b: 314). The equation presented here was that published in his other articles (Majorana 1919/20a: 75, 1919–20b: 420, 1919a: 648, 1920: 494). Poincaré had already studied this theoretical problem and reached an equivalent equation (Poincaré 1906/7: 188).

for a 1 kg body. In order to measure such an effect, it would be necessary to attain a sensitivity 10 times better, that is, to detect changes of 0.1 μg in 1 kg (10^{-10}).

Therefore, if the effect existed, it should be undetectable for laboratory-size bodies using the previously attempted techniques. Instead of using a Cavendish torsion balance as former researchers had done, Majorana decided to use the common analytical balance and to look for weight variations when a test body was surrounded by a thick shield of dense matter, a method that had already been tried by Kleiner, but with much lower sensitivity.

No balance of that time could measure such a small change of weight. Majorana adapted the best analytical balance he could find, taking special care to avoid effects due to temperature change, air currents, etc. The whole experiment was controlled from a distance of 12 m from the apparatus to avoid any influence of the observer upon the balance (heat, air currents, and gravitational attraction). The gravitational shield was placed around the test body in such a way that the shield's resultant Newtonian attraction would be null (Figure 8).

It was necessary to control the position of the test body and of the shield around it, because any change of position between them would produce an effect larger than the hypothetical gravitational absorption. The effects of the shield upon the body attached to the other arm of the balance, and upon the balance itself, could not be neglected. All this was taken into account in Majorana's work.

In his first series of experiments, Majorana used a test body surrounded by a cylinder with about one hundred kilograms of liquid mercury. His balance was so sensitive that measurements could only be made in the first hours after midnight, to avoid vibrations due to street traffic. The best measurement conditions were obtained during a general strike.

It was not necessary to touch the test bodies during the experiment. The two masses attached to the two arms of the balance were kept in their place and the balance was equilibrated. The balance did not maintain a constant equilibrium position, however; its zero point exhibited a slow but detectable drift. The drift was regular, and therefore Majorana supposed that it would be possible to detect weight changes even though the drift could not be eliminated. The observations tried to detect minute changes of the beam position, when the shield was put around the test body or withdrawn. It was necessary to make several measurements in sequence, in order to take into account the equilibrium drift.

Majorana observed a weight reduction of about one part in one thousand million when the test body was surrounded by mercury (Majorana 1919/20a: 83; 1919–20b: 93). After taking into account several systematic errors, he obtained a value for the constant of gravitational absorption h compatible with his theoretical analysis:

$$h = (6.7 \pm 1.1) \times 10^{-12} \mathrm{cm}^2\,\mathrm{g}^{-1}.$$

Two years after the first series of measurements, Majorana repeated the experiment surrounding the test body with nine thousand kilograms of lead. For practical reasons, the lead shield had a cubic form, instead of the cylindrical form used in the case of mercury (Figure 9). He anticipated that the effect now would

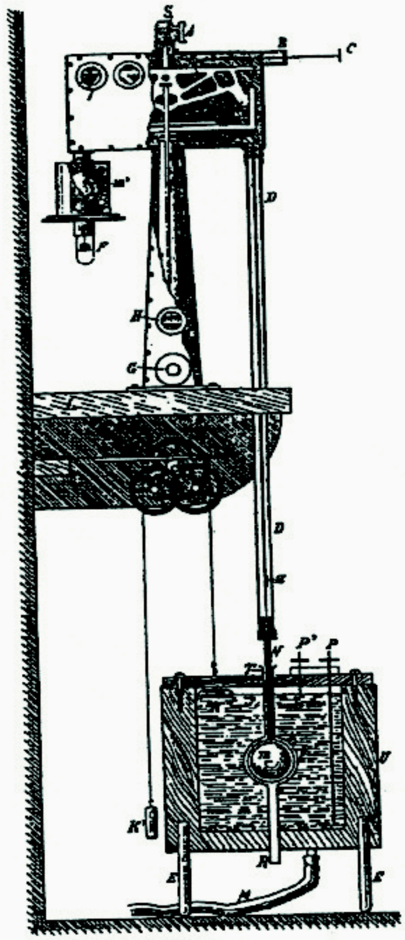

Figure 8. Majorana's first experimental arrangement for the measurement of gravitational absorption (1919–1920).

The balance and test bodies were enclosed in metallic vessels where a vacuum was produced. It was possible to manipulate the balance and the rider (of 10 mg) from outside (C). The oscillations of the balance were measured by a beam of light reflected from a mirror (S) at the top of the balance, through a strong glass wall (A). A deflection of 170 mm of the light beam corresponded to one mg, and it was possible to measure 0.1 mm, corresponding to 1/1 700 mg. Attached to the left side of the balance there was a 1 274 g sphere of lead (m'). Attached to the right side by a long brass thread (about 80 cm) there was a second lead ball (m) of equal mass, enclosed in a brass hollow sphere (V'), and this enclosed inside another brass hollow sphere (V). The second sphere could be surrounded by liquid mercury, contained in a strong wood cylindrical vessel (U). During the measurements, mercury was first introduced in the wooden vessel and then taken out, and any change of equilibrium of the balance was observed. Measurement and control were made at a distance of 12 m from the balance. The balance and vessel were covered by a threefold thick cover made of camel hair, to avoid changes of temperature.

be about 5 times larger than in the earlier experiment. There were, however, new significant experimental problems. The motion of the large mass of lead produced deformations of the whole building where the experiment was made. The deformation produced an angular tilting of about 10″ of the balance. It was necessary to measure and to try to compensate or evaluate all such changes.

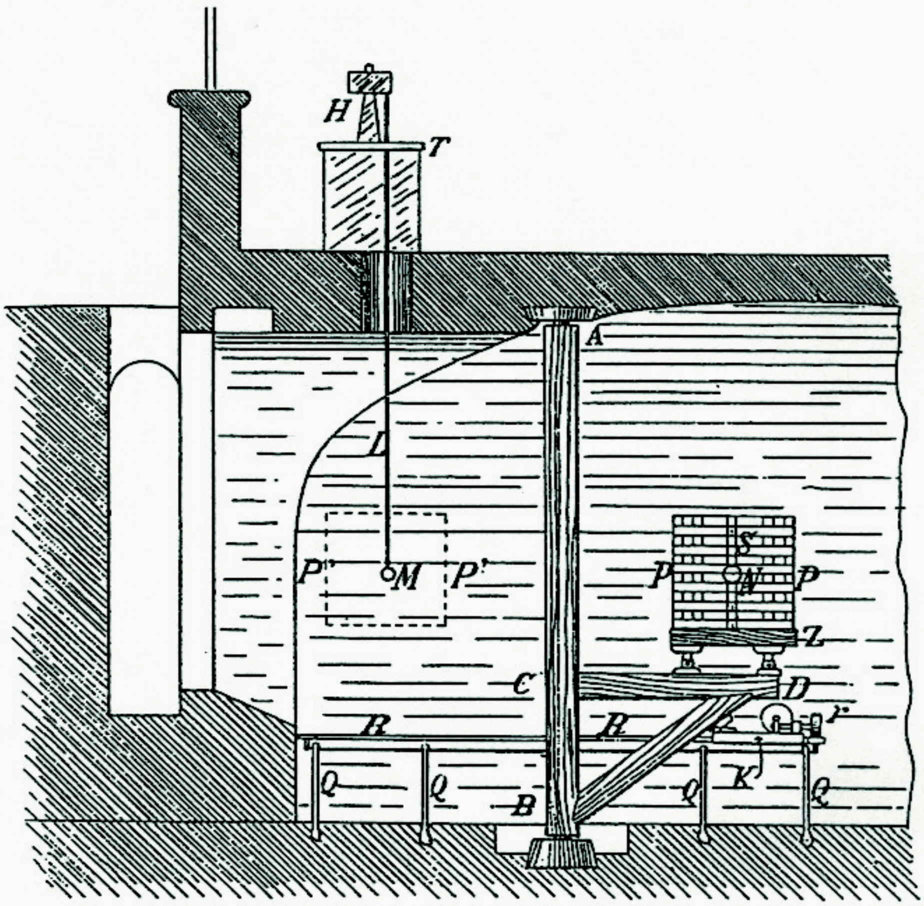

Figure 9. Majorana's second experiment (1921–1922).

The absorption of gravity was produced by a lead cube with dimensions of 95 cm and total weight of 9 603 kg. The cube was formed by two separate half-cubes. They could be moved 3 m away from the test body (*M*), by rotating them around the axis (*AB*) of their supports. The lead cube was mounted in the basement of the building. The balance (*H*) was on the ground floor.

After several corrections, the measured decrease of weight, ascribed to the absorption of gravity, was about half the expected value (Majorana 1921–22: 144). Therefore, in the case of lead, Majorana's measurements led to a different value for the constant h:

$$h = (2.8 \pm 0.1) \times 10^{-12} \mathrm{cm}^2 \mathrm{g}^{-1}.$$

It would be possible to ascribe this difference either to experimental error, or to a dependence of gravitational absorption on the chemical composition of the absorbing body. Majorana did not however choose any of those alternatives. He assumed that his experiments would be reproduced by other scientists, and that those new experiments would elucidate his results.

In this second series of experiments, Majorana tried to decide whether gravity was due to something emitted from the Earth (such as Seeliger's "gravitational rays"), or coming to the Earth through space (such as Le Sage's "ultra mundane corpuscles"). In the first case, the weight of the test body would be decreased by a screen placed ***between*** the Earth and the test body, but not if the screen were placed *above* the test body. In the second case, the converse would be true.

The experiment with the test body inside the lead cube could not distinguish between the two hypotheses. For this reason, Majorana made a new series of measurements, with the test body above and below the lead cube. Now, however, it was necessary to take into account the attraction of the test body by the lead cube.

When the test body was placed 5 cm above the upper surface of the cube (position 1), a weight *increase* of 200 g was observed. When the test body was placed 5 cm *below* the cube (position 3), he measured a weight *decrease* of 204 g (see detailed data in Majorana 1921–22: 224). For comparison, when the test body was in the middle of the cube (position 2), the weight reduction was 2 g. Majorana's conclusion was that the first hypothesis is the correct one, that is, gravitation is produced by something that is emitted by attracting bodies (Majorana 1921–22: 79)[18]

Majorana's experiment is inconclusive, however. Indeed, according to both hypotheses, the change of weight of the body below the cube should be greater than its change of weight above the cube. This can be shown by the following argument:

According to the first hypothesis (gravitational influence emitted from the Earth), when the test body was above the lead cube (position 1), its weight W would increase by F (the attractive force of the cube) and would decrease by f (the absorption by the cube of the gravitational attraction of the Earth). When the test body was below the lead cube (position 3), its weight W would decrease by F (the attraction of the cube).

According to the second hypothesis (gravitational influence coming from space), when the test body was above the lead cube, its weight W would increase by F (the attraction of the cube). When the test body was below the lead cube, its weight W would decrease by F (the attraction of the cube) and would decrease by f (the absorption of the gravitational attraction of the Earth).

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[18] The hypothetical emission of gravitational particles by matter was later further developed by him (see Majorana 1955).

	Test body above the cube	*Test body below the cube*
First hypothesis	$W + F - f$	$W - F$
Second hypothesis	$W + F$	$W - F - f$

Suppose that $F = 200$ g and $f = 4$ g. In this case, the changes of weight would be:

	Test body above the cube	*Test body below the cube*
First hypothesis	196	−200
Second hypothesis	200	−204

In both cases, therefore, the change of weight with the test body below the cube should be greater than with the test body above the cube. Majorana's test could not distinguish between the two hypotheses. This aspect of Majorana's work was not criticized by contemporary scientists, however.

4. Reactions to Majorana's work

After the publication of Majorana's first papers, Albert Michelson wrote to him and stated his intention to repeat his experiments in the Mount Wilson Observatory (Majorana 1921–22: 77). Majorana agreed enthusiastically, but Michelson never reproduced the experiment.

Majorana's first work attracted the attention of astronomers. Henry Norris Russell discussed some consequences of gravitational absorption (Russell 1921). As Eddington had formerly remarked, absorption of gravitation would lead to violation of the principle of equivalence between inertial and gravitational masses. This would produce a violation of Kepler's third law. Russell computed that In the case of Jupiter, the effect would correspond to an increase of 1% in the greater semi-axis. Astronomical measurements, however, showed that the largest possible deviation would be five hundred times smaller than that. Russell concluded that Majorana's measurements of gravitational absorption were incompatible with the motion of Jupiter. The only way out of those problems would be to assume that both the inertial and the gravitational masses were influenced by gravitational absorption.

Russell's final conclusion was that either Majorana had committed systematic errors, or that he had measured another phenomenon: a real mass variation due to the surrounding matter.[19] Russell even tried to link this suggestion to the general theory of relativity, where gravitational effects are not additive.

[19] Several decades after Majorana's work, an attempt was made by H. Grayson and Collin Williams to detect a change of mass induced by nearby matter (Grayson 1978). Their experiment was inconclusive, but their suggested improvements were remarkably similar to Majorana's experimental setup.

Russell's article was discussed by Arthur Eddington. Unexpectedly, Eddington defended Majorana (Eddington 1922), using a semi-relativistic argument. According to the strong principle of equivalence, a freely falling body could not absorb gravitational interaction, since, relative to this body, the gravitational field disappears. For that reason, the effect observed by Majorana in the laboratory would not have the consequences predicted by Russell in the case of the planets.

According to Eddington, the only odd consequence of Majorana's effect would be the possibility of building a gravitational perpetual motion machine, because the gravitational field would no longer be conservative.[20]

Majorana's *experimental method* was never criticized. Indeed, when one reads the detailed account of his measurements, it is very difficult to suggest any source of error that was not taken into account by Majorana himself. Discussion of Majorana's work focused on its consequences and compatibility with other accepted results. Majorana himself always stressed the importance of reproducing his experiments in order to check his results, but nobody ever did.

Majorana's experiments had been performed in the Physics Laboratory of the Turin Polytechnic. At the end of 1921, however, Majorana assumed the chair of Physics at the University of Bologna, as a successor to Augusto Righi. It seems that the new laboratory was better equipped than the former (see Perucca 1954: 359). There Majorana began a new series of experiments on absorption of gravity.

The main difficulty Majorana had found in his experiments was the deformation of the building resulting from displacement of about 10 tons of lead. In order to avoid this problem, Majorana reduced, in Bologna, the weight of lead to only 380 kg. The arrangement of the balance was also different: a cylindrical lead shield was successively placed around each of the two test bodies attached to the balance, in order to double the effect. Majorana stated that there were new sources of error and that it was impossible to derive any reliable value for the coefficient of absorption of gravitation from those measurements (Majorana 1930: 321).

At Bologna, Majorana also tried to improve his mercury experiments. In this case, a new arrangement of the mercury vessels was chosen, so that its whole weight was always applied to the same point of the floor. In 1930, Majorana was still improving the suspension of his balance and could present no quantitative results from this new arrangement:

> The few measurements that have already been carried out seem to give results that confirm the sense of the previously established effect, that is, an absorption of gravitational force. Although I cannot provide today quantitative results on the sought-for effect, I am confident that, with the new apparatus now under test, I will be able, after some time, to say my definitive word *on the subject*.[21] (Majorana 1930: 321)

[20] It is interesting to remark that Newton himself had sketched two different forms of a perpetual motion machine that could be built if gravitation were absorbed or refracted (McGuire & Tamny 1983: 431).

[21] "Les quelques mesures dejà faites semblent donner des résultats qui confirment le sens de l'effet autrefois établi, c'est-à-dire l'absorption de la force de gravitation. Bien que je ne puisse donner aujourd'hui des résultats quantitatifs sur l'effet recherché, j'ai confiance que avec le dernier appareil en cours d'expérimentation je serai à même, dans quelque temps, de dire, pour mon compte, un mot

Majorana's new measurements were never published. What happened? It seems that the results were not completely satisfactory and coherent, and other interests had captured his attention. Around 1930, Majorana was deeply involved in the development of communication by ultraviolet and infrared radiation, for military purposes (see Majorana 1941: 81–82). It seems that his gravitational experiments were successively postponed and never finished. Indeed, in 1941 Majorana was still referring to his Bologna attempts, remarking:

> The effect is of the same order of magnitude as that already observed in Turin. However it was impossible for me to establish its precise value in a definitive way. There are many perturbative causes that act in an erratic way when the experiment is varied. Notwithstanding this, hitherto the existence of the effect has always been confirmed. These are highly delicate researches that require months and years of accurate work for their preparation. If they are developed, they may in the future provide the last word on this interesting subject.[22] (Majorana 1941: 80)

That future time never arrived. Before his death in 1957, Majorana had published several works that refer to gravitational absorption, but he was unable to repeat his experiments (Majorana 1957a, 1957b).

Even with current techniques it would be difficult to attain the necessary sensitivity to repeat Majorana's experiments. It is remarkable, however, that no one has even *attempted* to repeat them.

5. Further laboratory experiments: Brush, Schlomka

Close to the time when Majorana reported his experiments, Charles Francis Brush (1849–1929) also published the description of anomalies ascribed to gravitational absorption. He claimed the detection of large violations of the proportionality between gravitational and inertial mass.

Brush made a series of measurements with a Cavendish balance and found that equal weights of different metals produced different attractions. Aluminum showed the greatest attraction, and bismuth the smallest. Compared to zinc, the attraction produced by aluminum was 30% higher, and that of bismuth was 28% lower (Brush 1921: 50). Brush also made pendulum experiments and measured a difference of about 1/35 000 between the ratio of inertial and gravitational mass for zinc and bismuth (Brush 1921: 56). Measurement in an inertial balance showed to him that the inertial mass of equal weights of those substances differed by one part in 1 300 (Brush 1921: 61).

There was a negligible chance that effects such as those claimed by Brush could have escaped former researchers. His results using the torsion balance

définitif *sur le sujet.*"

[22] "L'entità dell'effetto è dello stesso ordine di grandezza di quello già osservato a Torino. Ma non mi è stato possibile fissarne in modo definitivo, il preciso valore. Molte sono le cause pertubatrici che agiscono in modo incostante al variare delle modalità di esperimento. Ma comunque, mi è apparsa finora confermata sempre la esistenza dell'effetto stesso. D'altra parte si tratta di delicatissime ricerche per la cui preparazione occorrono mesi ed anni di accurato lavoro. Esse, se sviluppate potranno in avvenire dire l'ultima parola sull'interessante argomento."

conflicted with previous results, such as those obtained in measurements of the gravitational constant by Baily, who had tested several different substances and had found no significative difference between them (Baily 1843). Brush's pendulum experiments conflicted both with Bessel's well-known pendulum results (Bessel 1833) and with Eötvös's torsion balance measurements. Although Brush's work was not regarded as a challenge to Newtonian physics, it was easy to check some of his results. His pendulum experiments were repeated and were soon disconfirmed by other authors (Potter 1922 and Wilson 1922). Most of his experiments were never repeated, however—perhaps because they were not as easy to reproduce as the pendulum experiments, and because the disconfirmation of a single set of measurements was enough to show that Brush's work did not deserve further investigation.

Brush's work has been largely ignored by the scientific community. He was regarded as an amateur and his experimental work was not of the same level as Majorana's, for instance.

The latest experimental researches on gravitational absorption that were developed in the period that concerns us here were done by Teodor Johannes Hermann Schlomka. His work was highly original and delicate. Schlomka was aware of earlier works on gravitational absorption. He used a Eötvös torsion balance to search for this effect, but in a new way (Schlomka 1927–30). He measured the gravitational effect of a large cubic mass of iron (1 200 kg) on the torsion balance and tested whether it depended on interposed matter (water). While previous researchers had taken the utmost care to avoid the gravitational attraction of the shield, Schlomka simply measured the effect of the iron cube both with and without intervening water, and the effect of water without the iron cube (Figure 10). He then checked whether the gravitational effects of iron and water were additive or not.

Schlomka reported that the gravitational effect of the iron mass was indeed influenced by its passage through the water prism (Schlomka 1927: 399). He stated that the results were reproducible and were not influenced by temperature. Schlomka claimed that the magnetic properties of iron could not have influenced the results. The anomalous effect was about ten times larger than the experimental errors, in the single measurement reported by him.

One might question whether the use of the Eötvös balance was acceptable in the experimental situation chosen by Schlomka. The theory of the torsion balance requires that, within the dimensions of the instrument, the gravitational field and its first derivatives should be uniform. This condition was not satisfied in Schlomka's experiment and it is difficult to analyse the consequences of this violation of Eötvös's requirements.

At the end of his article, Schlomka promised to reproduce and improve those measurements. It seems, however, that the promise was not fulfilled. It also seems that no other researcher reproduced his experiments.[23]

[23] Schlomka's experiments are certainly easier to reproduce than Majorana's, and it would be interesting to check them.

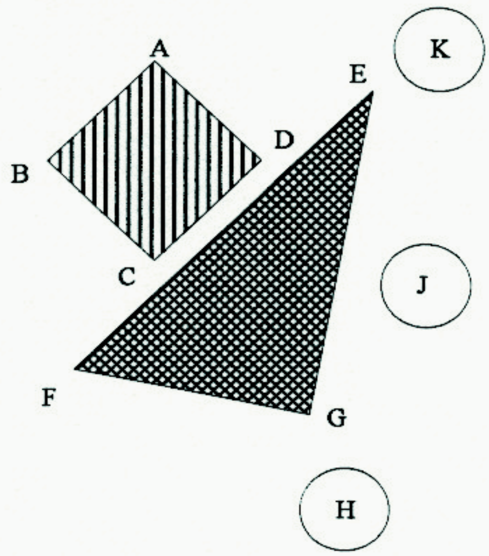

Figure 10. Schlomka's investigation of the effect of intervening matter on gravitation (1927–1930).

$ABCD$ is a large iron mass whose effect was measured by a torsion balance. EFG is a triangular iron water tank with vertical walls. When full, it contained 825 liters of water. Measurements we made with the torsion balance at three different places (H, J, K). Keeping the torsion balance at the same place, the gravitational field was measured with the four different combinations of presence/absence of the iron mass and of water.

6. Fluctuations of the motion of the moon

In the 1910s, independently of the laboratory search for the absorption of gravitation, Kurt Bottlinger and other authors investigated the relation between the motion of the Moon and gravitational absorption.

The reasons that led Bottlinger to study gravitational absorption were some unexplained fluctuations in the motion of the Moon and a theoretical conjecture due to Seeliger.

Although the general behaviour and most of the details of the motion of the Moon were explained by Newtonian gravitational theory, there were a few problems that were detected towards the end of the nineteenth century. Simon Newcomb devoted the last decade of his life to the study of these anomalies.

According to Newcomb, the existing gravitational theory was unable to explain all features of the Moon's motion. After taking into account all conceivable gravitational influences, there was a difference between observed positions and theoretical predictions (Figure 11). There was a great fluctuation in the longitude of the Moon, with a period between 250 and 300 years, and other minor

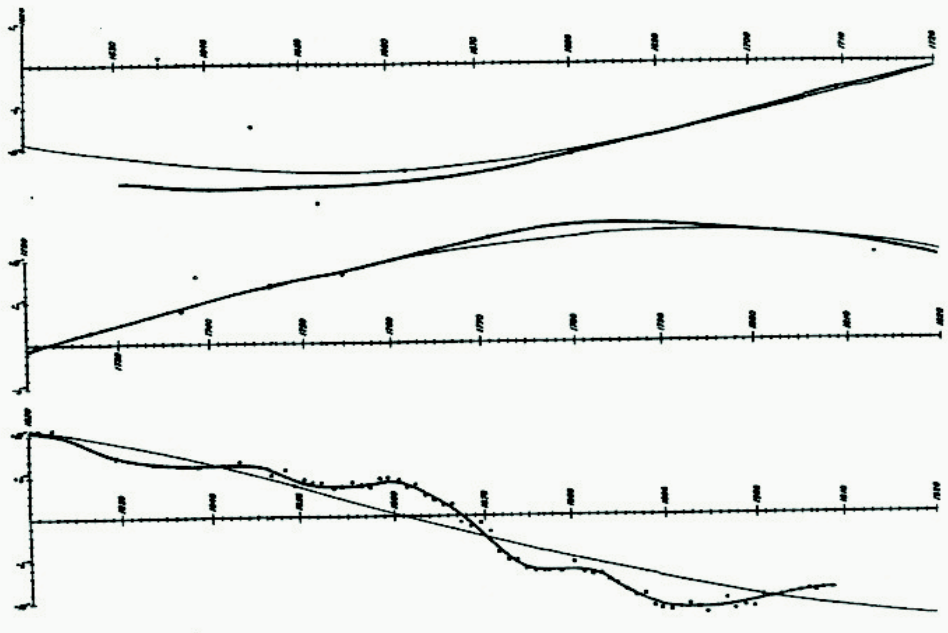

Figure 11. Newcomb's curves for the difference between observed and theoretical longitudes of the Moon from 1610 to 1908.

The whole time interval exhibited a large period fluctuation. From about 1820 onwards, when the number of observations is greater, it was possible to discern smaller period fluctuations.

fluctuations, with periods of about 60 and 20 years.[24]

Notice that the anomalous motion of the Moon is seldom cited in the traditional accounts of the rise of general relativity. As a matter of fact, the anomalous precession of Mercury's perihelion was just *one* of several unexplained astronomical phenomena at the beginning of the twentieth century.

Newcomb summarized the possible explanations of the fluctuations of the motion of the Moon. The first explanation that could be suggested was that the inequalities were only apparent, being due to fluctuations in the speed of rotation of the Earth. Indeed, astronomical observations measured the position of the Moon as a function of time; but astronomical time measurements used the rotation of the Earth as a clock.

24 The "great fluctuation" was described by Newcomb as corresponding to a term:

$$12''.95 \sin[100^\circ.6 + 131^\circ.00(T - 1800.0)].$$

After Newcomb's death, the short- and medium-period oscillations were described by his co-worker Franck Elmore Ross by two terms with periods of 57 and 23 years (Ross 1911):

$$2''.9 \sin[350^\circ.6 + 6^\circ.316(T - 1900.0)] \quad \text{and} \quad 0''.8 \sin[313^\circ + 15^\circ.65(T - 1900.0)].$$

Pendulum clocks were used to measure short time periods, but those mechanical clocks were periodically checked and corrected by astronomical observations of meridian transit of standard stars. For long time periods, time was measured assuming that the angular speed of the Earth (that is, the apparent angular speed of the stars revolving around the Earth) was constant. Therefore, any anomaly of the angular speed of the Earth would falsify time computations and would result in observational anomalies in the speed of celestial bodies. The effect would be especially observable in the case of the Moon, because its motion can be studied much more acurately than those of other bodies of the solar system.

Could the observed fluctuations of the motion of the Moon be explained as mere fluctuations of astronomical time? Newcomb discussed and rejected that explanation.[25] If there were significant fluctuations in the speed of rotation of the Earth, there would be observable consequences on the motion of the planets, and observations of Mercury seemed to preclude the existence of these changes in the speed of the Earth. Besides, it would be very difficult to explain oscillations in the speed of the Earth of the required magnitude and period. The rotation of the Earth is influenced by tidal friction, but this effect cannot produce *fluctuations* of its angular velocity. If the Earth could undergo significant periodic changes in its moment of inertia, its angular velocity would also change and it would be possible to explain the apparent fluctuations of the motion of the Moon. However, no cause was known that could produce changes of the required magnitude.

This is the reason why, after describing these residual fluctuations of the motion of the Moon, Newcomb remarked:

> I regard these fluctuations as the most enigmatical phenomenon presented by the celestial motions, being so difficult to account for by the action of any known causes, that we cannot but suspect them to arise from some action in nature hitherto unknown. (Newcomb 1909: 168)

7. Bottlinger's theory of the Moon

In 1909, Hugo von Seeliger suggested that the attraction between the Moon and the Sun could decrease during lunar eclipses, due to absorption of gravity by the Earth (Seeliger 1909: 12).[26] One of his students, Kurt Felix Ernst Bottlinger (1888–1934),[27] used this hypothesis to explain the anomalies of the motion of the Moon (Bottlinger 1912a, 1912b). Bottlinger assumed that gravitation was produced by

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[25] Newcomb had presented the comparative data on the Moon and Mercury in a previous paper, where he concluded: "The evidence seems almost conclusive that the very improbable deviations in the Earth's rotation inferred from the observation of the Moon are unreal, and that the motion of our satellite is really affected by causes which have, up to the present time, eluded investigation" (Newcomb 1903: 318).

[26] In former papers, Seeliger had already studied astronomical consequences—especially perihelion precession—due to hypothetical modifications of Newton's law of gravitation. He had particularly studied a modification with an exponential term, representing gravitational absorption, and its suitability for cosmological theories (Seeliger 1895, 1896).

[27] For an account of Bottlinger's life and work, see Schneller 1934.

"gravitational rays" emited by all bodies. Some of the gravitational rays from the Sun would be absorbed by the Earth during lunar eclipses. That would affect the motion of the Moon (Figure 12).

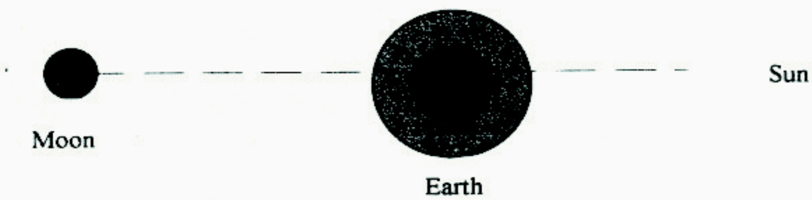

Figure 12. Seeliger's hypothesis (1909).

When the Earth is between the Moon and the Sun, during lunar eclipses, the gravitational attraction between Sun and Moon could suffer a reduction due to gravitational absorption by the Earth. Bottlinger studied the influence of eclipse details upon the supposed effect. It was necessary to take into account that the Earth is not homogeneous and that at each eclipse the Moon traverses a different path.

Bottlinger assumed the absorption of gravitational force to be proportional to the amount of matter between attracting bodies, according to an exponential law similar to that of light or X-rays.

$$F = F_0\, e^{-\lambda d},$$

where F_0 is the value of the force computed according to Newtonian theory, λ is a coefficient of absorption of gravitation (proportional to the density of matter) and d is the distance traversed by gravitation in the absorbing medium.

Using data about duration and position of lunar eclipses during one century, and assuming a simple inner model of the Earth, Bottlinger computed the mass interposed between Sun and Moon in the case of each eclipse. He was able to develop a quantitative prediction of perturbations that would be produced by gravitational absorption, with a single adjustable parameter—the coefficient of absorption of gravitation by matter. In this way, Bottlinger computed the effect of all lunar eclipses, from 1830 to 1910.

The main effect, according to Bottlinger's theory, would be a fluctuation in lunar longitude, instead of an accumulative secular effect. Bottlinger compared his results to Newcomb's residues (Figure 13).

There was a nice qualitative agreement. The maxima and minima occurred at about the same years. Quantitative comparison allowed Bottlinger to compute that the maximum relative decrease of gravitational attraction between the Moon and the Sun was about 1/60 000. This ocurred when the gravitational rays travelled through the centre of the Earth. He also computed the corresponding value of the absorption constant for a substance with unit density (1 $\mathrm{g\,cm^{-3}}$): $\lambda = 3 \times 10^{-15}$ $\mathrm{cm^{-1}}$.[28]

[28] Bottlinger was probably unaware of Eötvös's unpublished results. It is relevant to remark, however, that the gravitational absorption computed by Bottlinger is much smaller than the sensitivity of Eötvös's experiment; therefore, it is compatible with Eötvös's results.

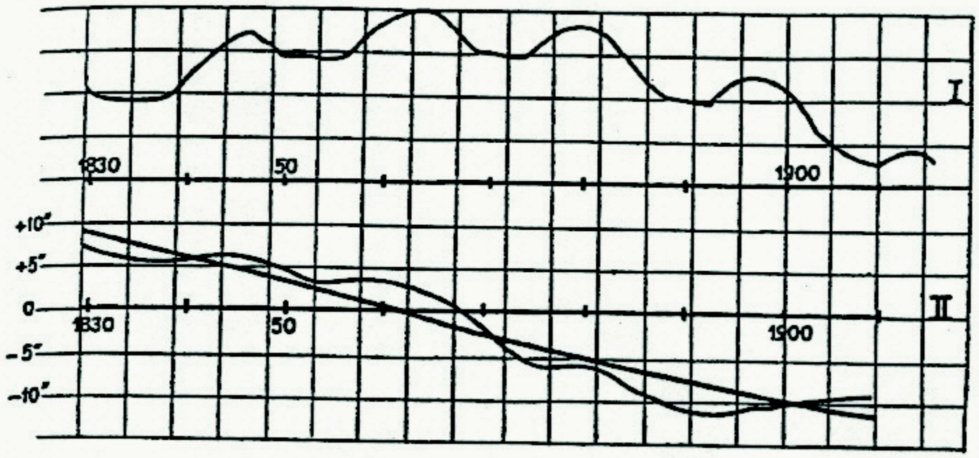

Figure 13. Bottlinger's comparison of theoretical and observed fluctuations in lunar latitude (1912).

Bottlinger compared the theoretical fluctuations of the longitude of the Moon due to gravitational absorption (I) to Newcomb's fluctuations (II). There was a general agreement between the positions of the maxima and minima.

Bottlinger's results were soon discussed by Willem De Sitter (De Sitter 1912). De Sitter had already studied gravitational absorption, but had not published his results. De Sitter did not criticize the basic hypothesis used by Bottlinger, but rather details of the theory. He stressed a few delicate points of Bottlinger's computation. The effects of successive lunar eclipses tend to cancel each other and therefore the effect computed over a large period of time is the sum of a series of positive and negative terms that do not differ much from one another. For that reason, it is necessary to compute highly accurate values for the absorption at each eclipse. De Sitter detected delicate aspects of some approximations in Bottlinger's theory that could lead to significant cumulative errors.

De Sitter compared his own previous unpublished studies to Bottlinger's. He noticed that their hypotheses and approximations were slightly different. There was a general agreement between the fluctuations found by Bottlinger and those computed by De Sitter. In both cases, the maxima and minima showed an agreement with those of the residues of Newcomb's analysis, for the nineteenth century.

There was, however, an important difference: from 1870 onwards, the perturbation computed by Bottlinger produced increasingly *negative* results (Figure 14). On the contrary, the effect computed by De Sitter led to increasingly *positive* values. This showed that slight changes in the assumptions produced relevant changes in the results.

It seems that in 1912 De Sitter believed that absorption of gravity could turn out to be the solution of the problem of lunar motion and that it could explain both small-period fluctuations and Newcomb's long-period term. Indeed, at one point of his paper, De Sitter remarked:

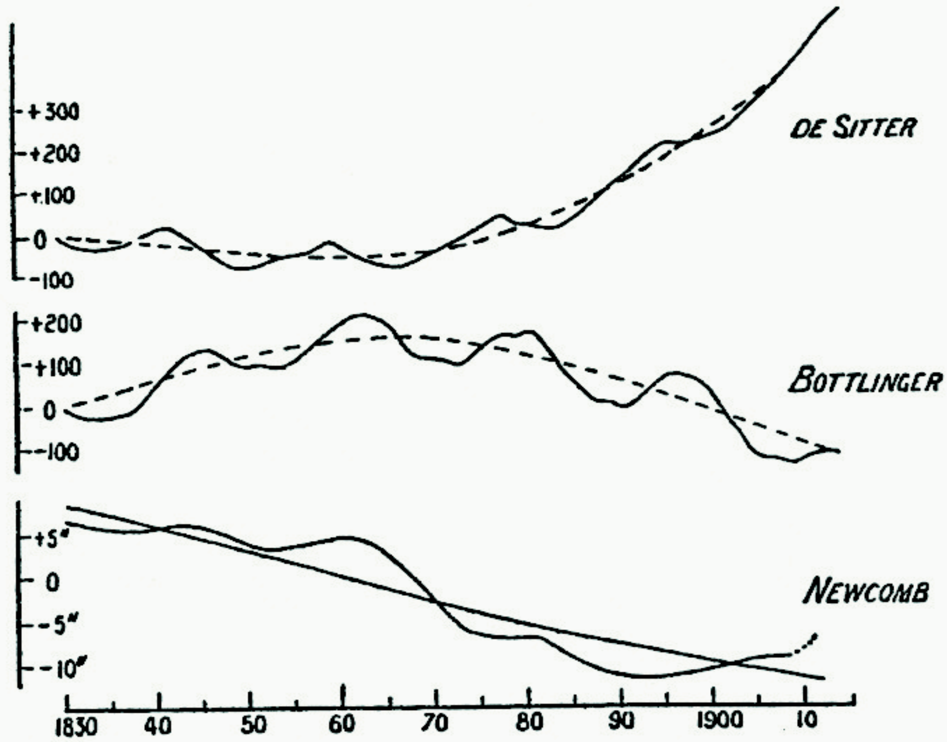

Figure 14. De Sitter's theory vs. Bottlinger's (1912).

De Sitter computed the effect of the supposed gravitational absorption using a different method. He also obtained fluctuations that roughly corresponded to those obtained by Bottlinger. However, there was a different secular (or long period) effect that was completely different in De Sitter's and Bottlinger's works.

> And perhaps we may entertain a slight hope that the slow 'undercurrent' will not only prove to be not incompatible with the observations, but may even be the explanation of the great fluctuation of 273 years' period. (De Sitter 1912: 393)

The final paragraph of De Sitter's paper is:

> However this [may] be, whether Dr. Bottlinger's results are confirmed or not, he must be congratulated on having completed a fine piece of work, which may ultimately prove to be of great importance for our intelligent understanding of natural phenomena. (De Sitter 1912: 393)

In a second paper presented in November of the same year, De Sitter presented a new detailed study of the effect of gravitational absorption on the motion of the Moon (De Sitter 1913). In his new work, De Sitter developed a more elaborate theory of lunar motion and used two different hypotheses about the internal structure of the Earth.

The results confirmed that any non-periodic effect was strongly dependent on the detailed assumptions about the structure of the Earth. Besides, it showed that the periodic fluctuations predicted by the theory were essentially the same, notwithstanding the use of different models of the Earth.

De Sitter compared the new theoretical predictions to observations and found significant differences between theory and the observed fluctuations when the whole period from 1700 to 1910 was used. Even over a shorter period, the concordance between predicted and observed fluctuations seemed to him poor (Figure 15). He concluded there was no reason to accept the existence of gravitational absorption by the Earth.

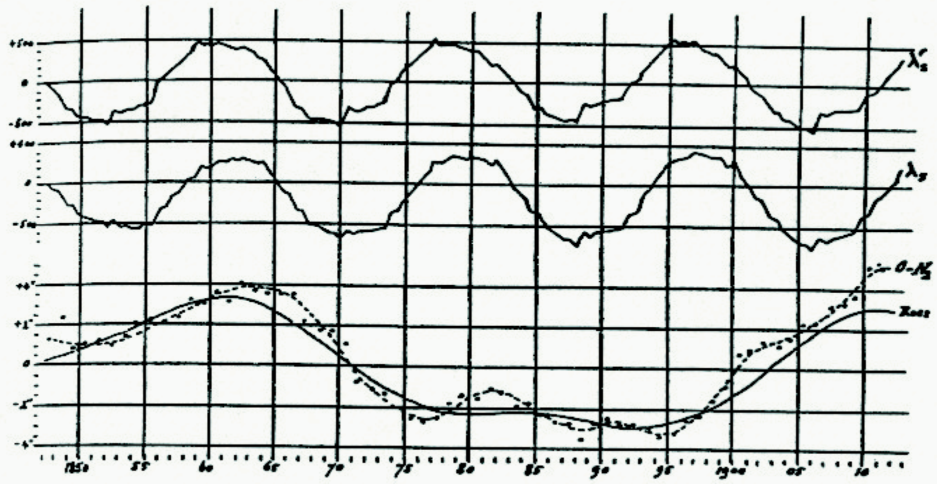

Figure 15. De Sitter's two hypotheses (1913).

In his second work, De Sitter improved the method of computing the effect of gravitational absorption on the Moon's motion and used two different hypotheses about the distribution of matter inside the Earth. In both cases the short period fluctuations (λs and λ's) were very similar. De Sitter concluded that those theoretical fluctuations did not agree with observed fluctuations ($0\text{-}N_2$).

De Sitter was the only astronomer who took the trouble to discuss in detail and re-analyse Bottlinger's work. Other astronomers presented only short comments on the issue.

William Campbell regarded Bottlinger's work as a competitive solution of the problem of lunar theory.

> Bottlinger and De Sitter have recently investigated the hypothesis that the mutual gravitational attractions of two bodies may be influenced by the passing of a third body between the first two. ... There is some evidence that this hypothesis is an approximation to a fact of nature. (Campbell 1913a: 47)

The general opinion, however, was that the hypothesis would not provide an answer to the difficulties. Arthur Eddington was sceptic regarding the possibility of gravitational absorption (Eddington 1915). He presented another difficulty: if that effect existed, there would be no proportionality between inertial and gravitational mass and Kepler's laws would be violated. However, such a violation did not seem to exist.

In a later work (Bottlinger 1914), Bottlinger studied the consequences of two new aspects of the hypothetical absorption of gravitation:

(a) influence of solar eclipses upon the rotation of the Earth;
(b) influence of the absorption of gravitation on the motion of Mars's satellites.

In solar eclipses, the shadow of the Moon (and the corresponding assumed absoption of gravitation) traverses a small path on the surface of the Earth. When the eclipse is not a central one, the assymmetry of the solar eclipse will result in a change of the angular momentum of the Earth. That change would be so small, that it could not be detected by any clocks existing at that time. As astronomical time was reckoned according to the rotation of the Earth, the change of this rotation would be interpreted as a change in the speed of the other celestial bodies. The effect would be particularly observable in the case of the Moon, because its motion can be studied much more accurately than that of other bodies of the solar system.

Bottlinger computed the effects of both lunar and solar eclipses. He added their effects and compared his theory to observation. There was now a better agreement, and a new and smaller coefficient of absorption of gravitation could be computed ($\lambda = 1.3 \times 10^{-15}$ cm^{-1}).

Bottlinger also applied the same theory to the study of the motion of Phobos. The small distance between this satellite and Mars would lead to strong effects due to gravitational absorption. Anomalies in the motion of Phobos could amount to 36′ in longitude, with a period of 11 years. Available astronomical data did not allow the checking of this consequence of the theory. Bottlinger suggested the necessity of new observations, but it seems that the comparison was never made.

Majorana never referred to Bottlinger's studies. It is relevant to remark, however, that Bottlinger had obtained a coefficient of gravitational absorption about 500 times smaller than Majorana's and hence compatible with the limit computed by Russell from the behaviour of the planets.[29]

8. See's explanation of long-period fluctuations

Bottlinger's work could only account for short-period fluctuations. A few years later, the American astronomer Thomas Jefferson Jackson See tried to improve Bottlinger's work and to explain all fluctuations.

See is known for his 'capture theory' of the formation of the solar system, published in 1910. He might be characterized as a crank astronomer, as his work

[29] An anonymous reviewer of *The Observatory* (1920) remarked that Majorana's value for the absorption of gravitation was much higher than Bottlinger's and so there would be a difficulty in reconciling the former value with the observed motion of the Moon.

contains unorthodox views on every aspect of astronomy (Ashbrook 1962). Shortly after publication of Bottlinger's researches, he began to work on new ideas about electromagnetism and gravitation, including the hypothesis of gravitational absorption. His first results on this subject were published in 1917 as a series of 6 independently printed bulletins, afterwards collected as a book named *Electrodynamic Wave-Theory of Physical Forces* (See 1917).

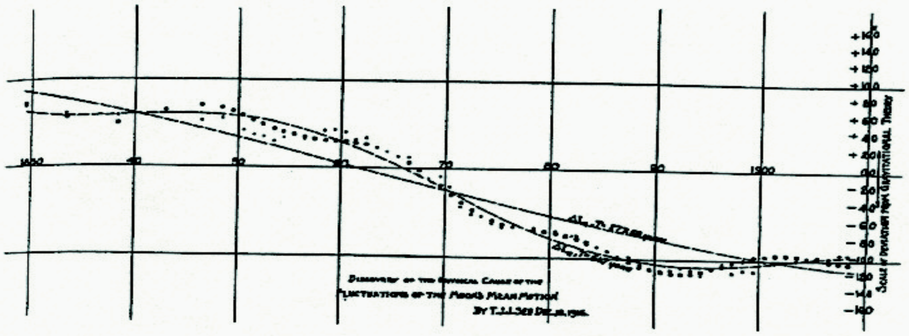

Figure 16. Fluctuations in the Moon's mean motion according to See (1917).

See's comparison between theoretical and observed fluctuations of the motion of the Moon. The small circles represent the difference between observed longitudes and Newtonian predictions (Newcomb's residues). The larger, open circles represent See's theoretical fluctuations. The curves corresponding to the longer period (277.59 years) and medium period (61.7 years) fluctuations are also shown.

Without elucidating the details of his work, See presented an impressive graphical comparison between his theory and observed fluctuations of the motion of the Moon (Figure 16). The agreement is striking. Besides, See presents exact values for the periods of the fluctuations: 18.0293 years, 61.7006 years and 277.590 years (See 1917: 4). The amplitudes and phases of the fluctuations were also exactly presented in his formulae:

$$\Delta L_1 = 1''.0 \sin[19^\circ.9675(t - 1800.0) + 239^\circ.42]$$

$$\Delta L_2 = 3''.0 \sin[5^\circ.83597(t - 1800.0) + 126^\circ.35]$$

$$\Delta L_3 = 13''.0 \sin[1^\circ.29691(t - 1800.0) + 100^\circ.6]$$

To arrive at these terms, See used vague analogies between light and gravitation. He assumed that the hypothetical gravitational waves would suffer refraction—"and perhaps absorption"—when they passed through the Earth (Figure 17). This would produce a weaker gravitational attraction between the Sun and the Moon during eclipses (See 1917: 87–91). Qualitatively, the hypothetical effect is similar to the one envisaged by Bottlinger, and See reproduces many of his equations.

However, it would be impossible to work out See's hypothesis in a quantitative way, because the effect would depend on several unknown details: refraction would depend on the precise constitution of the Earth, on the spectral composition of gravitational waves from the Sun, on the index of refraction and dispersion of each kind of gravitational wave through matter, on the absorption of gravitational waves, etc. Besides, the effect would not be confined to the Earth's shadow, but would also affect nearby regions.

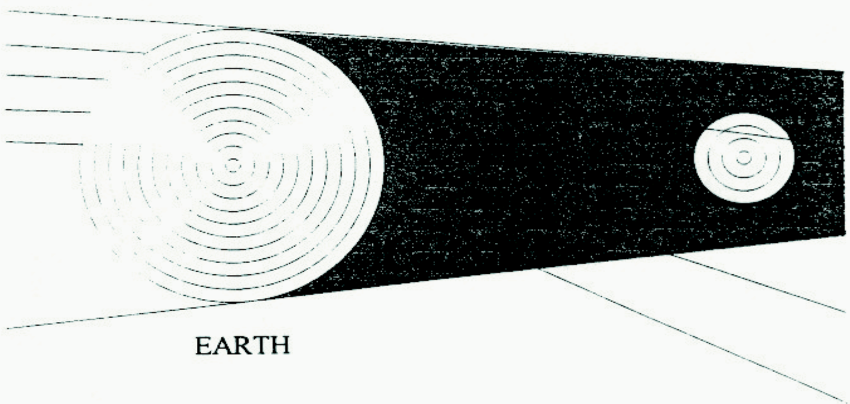

Figure 17. See's gravitational waves.

According to See, the gravitational waves emitted by the Sun would suffer refraction and absorption inside the Earth. This would affect the force between Sun and Moon even when the Moon is not in the shadow of the Earth. The effect could not be computed without detailed knowledge of the spectral composition of gravitational waves, of the inner constitution of the Earth, the index of refraction (and dispersion) of gravitational waves, and coefficient of absorption of gravitation by matter.

How, then, did See arrive at his exact theoretical evaluation of the Moon's fluctuations? That was very simple. He revised all known cycles associated with the motion of the Moon (See 1917: 101–107) and chose the periods that best suited the observed fluctuations. The amplitude and phase were taken from Newcomb's empirical representation of the fluctuations.

On close analysis, See's work was rejected as groundless. Its apparent success was just due to the use of Newcomb's empirical data (Jeffreys 1918). Instead of improving Bottlinger's work, it probably helped to decrease the interest of astronomers on this subject.

9. Einstein's explanation of the Moon's fluctuations

Shortly after the time when Bottlinger developed his theory, Albert Einstein formulated the general theory of relativity. It is well known that one of the successes of Einstein's theory was to explain the anomalous precession of Mercury's perihelion. However, there were other important anomalies, such as the fluctuations of the Moon. The theory of relativity was incompatible with gravitational absorption; although it was a non-linear theory, nothing similar to absorption appears in the field equations. Therefore, Einstein could not accept Bottlinger's theory and he was naturally led to look for another explanation of the fluctuations in the Moon's longitude.

Einstein's attempt to explain this phenomenon was a classical one (Einstein 1919a). He tried to ascribe it to fluctuations of the rotation of the Earth. The changes of rotation of the Earth were due, in Einstein's opinion, to tidal effects.

Tides produced by the Moon and the Sun produce a change of the moment of inertia of the Earth. That, in its turn, must produce a change in the speed of rotation of the Earth. If the rotation of the Earth is not uniform, astronomical observations of the Moon will exhibit anomalies, because astronomers measure time assuming that the rotation of the Earth is uniform. Periodic changes of the moment of inertia of the Earth will lead to periodic apparent fluctuations of the motion of the Moon.

Einstein's argument presupposes that there is no irregularity in the motion of the Moon, that is, it assumes that the Moon follows Newton's gravitational theory. At any given time t, the Moon would be in its 'correct' place. Suppose, now, that the rotation of the Earth fluctuates around its median angular motion. The angular position of a reference point on the surface of the Earth at time t is $\varphi = \omega t + \delta\varphi$. At this time t, if the Earth rotated with an uniform angular speed, the longitude of the Moon would be φ. However, due to the irregularity of the motion of the Earth, the longitude of the Moon measured relative to the Earth at time t will be $\varphi' = \varphi - \Delta\varphi$, where $-\Delta\varphi$ is the difference between the mean motion and the actual motion of the Earth at time t. Therefore, relative to the Earth, the motion of the Moon will fluctuate around its theoretical (Newtonian) value with an amplitude equal to the amplitude of the fluctuation of the motion of the Earth.

Einstein computed the effect of the tides. In his theory, there was only one adjustable parameter: the mean amplitude of tides. He assumed a value of 1.5 m and obtained fluctuations of the Moon's motion with amplitude of $1''$. He also computed the times of maxima and minima of those fluctuation, and they agreed with observed maxima and minima. The theoretical values of the fluctuations were smaller than observed ones, but Einstein ascribed the difference to the value he used for the moment of inertia of the Earth. Therefore, it seemed to Einstein probable that this was the correct explanation of the fluctuations of the Moon.

Einstein was not the first to try to explain the fluctuations of the Moon in this way. However, all previous attempts had obtained too small effects. Indeed, Einstein was wrong.

Immediately after the publication of Einstein's work, an astronomer named Albert von Brunn detected his mistake (Brunn 1919). Einstein had not taken into account the methods really used by astronomers in their measurements: "The explanation seems to be founded upon a mistake concerning the method of time determination in astronomy."[30] If things worked as Einstein supposed, then *all* celestial bodies (including the stars, Sun and planets) would exhibit the same fluctuations, with the same period and amplitude.[31] These fluctuations do not exist. By correcting Einstein's assumptions, Brunn showed that the periodical fluctuations of the Moon would have an amplitude 27 times smaller than those computed by Einstein. Therefore, Einstein's proposal could not explain the observed fluctuations.

In a note to Brunn's article, Einstein acknowledged his mistake:

> Herr von Brunn's criticism is completely well founded. Since my mistake is not devoid of a certain objective interest, I want to characterize it shortly too. My reflection would be correct, if the astronomers used the Earth itself as a spatial reference body, in connection with a particular clock for time measurement. In fact, the astronomers use the stellar heaven as a coordinate system for spatial measurements, and the rotation of the earth relative to the stars as a clock. Therefore, an irregularity of the rotation of the Earth relative to the time measurement can only be displayed in the way shown by Herr Brunn.[32] (Einstein 1919b: 711)

10. Einstein's mistake

What was wrong in Einstein's argument? The whole problem was the use of *absolute time t* in the argument. Einstein implicitly assumed that there was a way of measuring time *independently of the rotation of the Earth*. Of course, nowadays we can use atomic clocks, but in 1919 astronomers measured time using the rotation of the Earth relative to the stars as their clock. Even at that time, it was possible, of course, to *think* about some absolute (Newtonian) time and deduce consequences, but it would be necessary to analyse what *observable* effects could be measured by astronomers.

Suppose there exist a sidereal astronomer and a terrestrial astronomer. Suppose further that both use the same theory for computing the motion of the Moon, that is, they use the same formula $\phi = \phi(t)$ to compute its longitude. Both measure angles relative to the same set of 'fixed' stars. The sidereal astronomer uses an

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30 "Die Erklärung scheint auf einem Irrtum über die Methode der Zeitbestimmung in der Astronomie zu beruhen" (Brunn 1919: 710).

31 "If this understanding was correct, the apparent right ascension of the stars, as also the longitudes of the Sun and the planets, would all exhibit the same essential periodicity as the Moon's longitude" ["Wäre diese Auffassung richtig, so würden so würden offenbar die Rektaszensionen aller Gestirne und damit auch die Längen der Sonne und der Planeten alle im wesentlichen die gleiche Periodizität zeigen wie die Mondlänge"] (Brunn 1919: 710).

32 "Herrn von Brunns Kritik ist durchaus begründet. Da mein Irrtum nicht ohne ein gewisses objektives Interesse ist, will auch ich ihn noch einmal kurz charakterisieren. Meine Betrachtung wäre richtig, wenn sich die Astronomen der Erde als räumlichen Bezugskörpers in Verbindung mit einer besonderen Uhr als Zeitmaß bedienten. In Wahrheit dient den Astronomen der Fixsternhimmel als Koordinatensystem für die räumlichen Messungen, die Drehung der Erde relativ zu den Fixsternen als Uhr. Deshalb kann eine Ungleichmäßigkeit der Erddrehung nur Fehler bezüglich der Zeitmessung herbeiführen, wie Herr Brunn zutreffend ausgeführt hat."

absolute clock and the terrestrial astronomer uses the rotation of the Earth as a clock. That is, he assumes that the rotation of the Earth obeys a simple law:

$$\varphi = \omega t',$$

where the angular speed ω of the Earth is assumed to be constant and t' is the time measured by the terrestrial astronomer.

The sidereal astronomer observes that the rotation of the Earth is not uniform: its rotation is described by this astronomer as:

$$\varphi = \omega t + \Delta\varphi(t),$$

where ω is the *mean* angular velocity of the Earth. There will be a difference between the times assigned by the sidereal and terrestrial astronomers to any event, since $\omega t' = \omega t + \Delta\varphi(t)$; therefore $t' = t + \Delta\varphi(t)/\omega$.

Suppose that the sidereal astronomer computes the position of the Moon at a time $t = T$ and obtains the longitude ϕ that agrees with observation. What will the terrestrial astronomer find at this same time?

When the terrestrial astronomer computes the position of the Moon at time T, he obtains the same value $\phi(T)$ as the sidereal astronomer, because both use the same formula. However, when the Moon passes by this position $\phi(T)$, the terrestrial clock will not measure time $t' = T$, but a time $t' = T + \Delta\varphi(T)/\omega$, because of the irregularity of the rotation of the Earth. The terrestrial astronomer will conclude that the Moon is behind (or ahead) of its theoretical position, because at time $t' = T + \Delta\varphi(T)/\omega$ the Moon should be in the position

$$\phi\left[T + \frac{\Delta\varphi(T)}{\omega}\right] \approx \varphi(T) + \frac{\Delta\varphi(T)}{\omega}\frac{d\phi}{dt}.$$

If the motion of the Earth fluctuates with an amplitude a and the angular velocity of the Moon is $d\phi/dt = \omega'$, then the observed longitude of the Moon will fluctuate around its theoretical position with an amplitude $a' = a\omega'/\omega$, when observed by the terrestrial astronomer.

Notice that the fluctuation of the position of the Moon, $\Delta\phi$, will be *different* from the fluctuation of the Earth, $\Delta\varphi$, because the angular speed of the Moon, ω' is different from the angular speed of the Earth, ω. As ω' is about 27 times smaller than ω, the amplitude of the observable fluctuation of the Moon would be about 27 times smaller than the fluctuation in the Earth's rotation. If the amplitude of the oscillations of the Earth's motion is 2″, the corresponding fluctuations of the Moon would amount to only 2″/27.

If we apply the same argument to other celestial bodies (the Sun and planets), it will be easily perceived that their observable fluctations due to the fluctuation of the rotation of the Earth will be much smaller that that of the Moon, because, relative to the Earth, their angular velocities are always much smaller than ω'.

The argument presented here is a didactic reconstruction of Brunn's very short correction of Einstein's mistake. It seems that Brunn did not care to discuss the argument in detail because astronomers were well aware of all those distinctions. Einstein's mistake was due to his lack of acquaintance with astronomical methods of measurement.

11. Final explanation of lunar fluctuations

In the long run, lunar fluctuations were explained away. A general consensus was reached around 1940: the motion of the Earth is irregular—but the changes of the rotation of the Earth were not ascribed to the tides (Spencer Jones 1939). As a result of this interpretation, the rotation of the Earth could not be retained as the standard of time determination. A new measurement of time was introduced in astronomy: so-called *ephemeris time*, defined as the time parameter that complies with gravitational theory. It was adopted by the International Astronomical Union in 1955 (Spencer Jones 1955). In principle, it was associated to the motion of the Earth around the Sun (or the apparent motion of the Sun). In practice, however, ephemeris time was determined from observations of the Moon. The adopted definition used a correction equation that included a parameter B which was the empirical fluctuation in the Moon's longitude that is, the difference between its observed position and the motion predicted by gravitational theory (Spencer Jones 1956: 22).

Therefore, the fluctuations of the motion of the Moon disappeared *by definition*: the adoption of the new definition of time used the motion of the Moon itself as a clock. According to that clock, of course, the motion of the Moon is completely regular.

De Sitter himself greatly contributed to the establishment of this standard interpretation. He studied the motions of the Sun, Mercury and Venus and showed that all of them exhibited longitude fluctuations similar to those of the Moon, and proportional to their mean motions (De Sitter 1927, 1928). The agreement was especially good for the long- and medium-period terms and was better in the cases of Mercury and Venus than in the case of the Sun (Figure 18).[33] Harold Spencer Jones always supported this explanation of the fluctuations (Spencer Jones 1926, 1939). However, up to 1932, this explanation was not free from problems (Fotheringham 1927, 1932).

Around 1920 there was no quantitative explanation for the Moon's short-period fluctuations better than Bottlinger's. In that year, Ernest Brown, one of the leading authorities in lunar theory, reviewed the question (Brown 1920). Up to that time, a correlation had been found between the irregularities of the lunar motion and those of Venus and Mercury, but these seemed smaller than expected. Besides that, no cause was known that could produce fluctuations in the rotation of the Earth of the required magnitude: "There is, I think, a growing conviction that the Earth's average rate of rotation has not sensibly changed within historic times"

[33] See also Dyson & Cullen 1929. The correspondence between the fluctuations of the Sun and the Moon was at most as good as the correspondence between Bottlinger's theory and the observed lunar fluctuations.

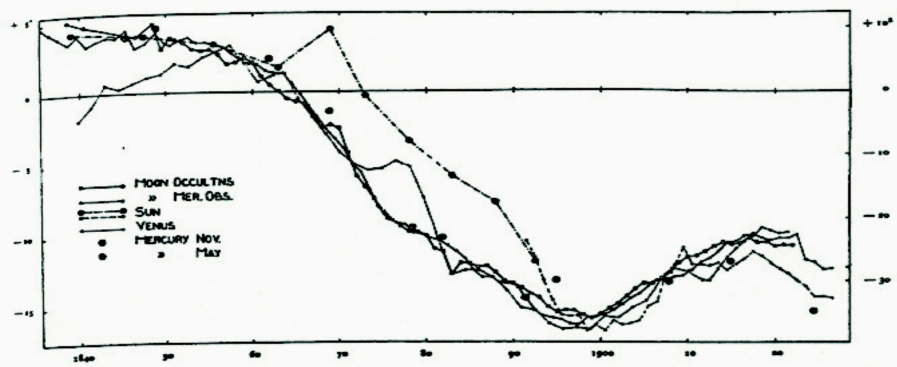

Figure 18. De Sitter on fluctuations of the Moon, Sun, Venus and Mercury (1927).

De Sitter's comparison between fluctuations of the motions of the Moon (measured by occultations and meridian observations), Sun, Venus, and Mercury, from 1840 to 1920. In the case of the Sun, agreement is poor. There was, however, a general agreement between the fluctuations of Venus, Mercury and Moon specially for the long period fluctuation.

(Brown 1920: 100–101). Brown's conclusion was that some unknown cause was producing real changes in the motions of the Moon and the planets.

> The irregular character and comparatively great magnitude of these fluctuations suggests that there is some set of forces acting on the bodies of the solar system which are related to the known irregular changes in the condition of the Sun. (Brown 1920: 101)

In the decade 1910–1920, Bottlinger's theory was an interesting, quantitative attempt to explain short period fluctuations of the Moon. It did not account for all known facts, but no other explanation did, either. However, most astronomers dismissed it without a detailed analysis and kept waiting for a classical explanation of the phenomenon.

12. Conclusion

Nowadays, we believe that gravitational absorption does not exist. From the concordance between gravitational theory and motions observed within the solar system,[34] as also from geophysical measurements,[35] an upper limit was reached for the constant of absorption of gravitation: it must be smaller than Bottlinger's value: $\lambda < 10^{-15}\ g^{-1}cm^2$.

According to present scientific knowledge, the similarity between Bottlinger's theoretical curve and the observed fluctuations of the Moon was due to chance.

[34] See Steenbeck & Treder 1984, especially pp. 16–25).

[35] See Bocchio 1971 and Groten 1972. See also Cook 1988: 719.

Also, according to current knowledge, Majorana measured nothing but experimental errors. Indeed, both in the old gravitational experiments and in recent ones, it is usual to find unexplained systematic effects (Cook 1987, 1988). As Cook has put it, "it is difficult to attain an adequate understanding of experiments at the limit of available techniques" (Cook 1987: 76). Majorana was certainly pushing the sensibility of weight measurements to its limit. Although he was a careful experimenter, some systematic error might be responsible for his results.

From the historical point of view, it is relevant to understand why those investigations of gravitational absorption did not receive much attention at the time they were published. Some of the attempts to detect anomalous effects produced null results and were not very exciting—they were forgotten. Some of the research that produced anomalous results was soon reproduced and errors were detected. This was the case with Brush's enormous violations of the principle of equivalence and Heydweiller's transformation of gravitation into radioactive energy. Some of the authors of these results could be classified as cranks and their works could be dismissed without detailed analysis. This was the case of See's wave theory of gravitation.

It is not so easy to understand why Bottlinger's and Majorana's explorations were also dismissed or did not receive much attention. They were high-quality work, but were not linked to the mainstream of gravitational research of the time. Their motivation was an old-fashioned corpuscular (or wave) theory of gravitation. That style of theory had been very popular among outstanding scientists at the end of the nineteenth century, but now had been replaced by another kind of theory. Since the beginning of the twentieth century, Poincaré, Lorentz, Abraham, Einstein, Nordström, Mie and several other physicists were striving to develop a relativistic theory of gravitation.[36] These new theories of gravitation did not appeal to mechanical models or analogies: they used sophisticated mathematics and their primary aim was to provide a unified relativistic description of gravitation and electromagnetism. From the point of view of this line of research, gravitational experiments or explanations grounded upon old models and analogies were mere child's play.

Majorana was unfortunate enough to publish his results at the time when all the world was celebrating Einstein's successful prediction of the bending of light rays near the Sun.[37] Besides the three classical tests of general relativity, there seemed to be no new phenomenon that could be observed. As an effect of widespread acceptance of general relativity in the 1920s, for some decades gravitational research was transformed into a mathematical subject and experimental gravitation came close to extinction. This might explain both the lack of reproduction of anomalous results, and the general oblivion of these interesting investigations.

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[36] For a contemporary statement of theoretical gravitational research before the full development of general relativity, consult Abraham 1914.

[37] Of course, the relation between eclipse observations and theory was not as simple as usually assumed (see, for instance, Moyer 1978, Earman & Glymour 1980), but for the general scientific and non-scientific public the 1919 eclipse observations seemed a crucial test of general relativity.

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REFERENCES

ABRAHAM, Max. (1914). "Neuere Gravitationstheorien." *Jahrbuch der Radioaktivität und Elektronik* **11**: 470–520.

AITON, E. J. (1969). "Newton's Aether-Stream Hypothesis and the Inverse Square Law of Gravitation." *Annals of Science* **25**: 255–260.

[ANONYMOUS]. (1920). "Experimental Researches on Gravitation." *The Observatory* **43**: 286–287.

ASHBROOK, Joseph. (1962). "The Sage of Mare Island." *Sky and Telescope* **24**: 193, 202.

AUSTIN, Louis Winslow & THWING, Charles Burton. (1897). "An Experimental Research on Gravitational Permeability." *Physical Review* **5**: 294–300.

BAILY, Francis. (1843). "An Account of Some Experiments with the Torsion Rod, for Determining the Mean Density of the Earth." *Memoirs of the Royal Astronomical Society* **14**: 1–120, i–cclviii.

BESSEL, Friedrich Wilhelm. (1833). "Versuche über die Kraft, mit welcher die Erde Körper von verschiedener Beschaffeneheit anzieht." *Astronomische Nachrichten* **10** (223): col. 97–108.

BOCCHIO, Franco. (1971). "Old and New Approaches in the Study of Gravitational Absorption." *Geophysical Journal of the Royal Astronomical Society* **24**: 101–102.

BOTTLINGER, Kurt Felix Ernst. (1912a). "Die Erklärung der empirischen Glieder der Mondbewegung durch die Annahme einer Extinktion der Gravitation im Erdinnern." *Astronomische Nachrichten* **191**: 147–150.

——— (1912b). *Die Gravitationstheorie und die Bewegung des Mondes*. Freiburg: C. Trömer.

——— (1914). "Zur Frage der Absorption der Gravitation." *Sitzungsberichte der Königlichen Bayerischen Akademie der Wissenschaften zu München. Mathematische-physikalische Klasse* **44**: 223–239.

BOYS, Charles Vernon. (1894). "On the Newtonian Constant of Gravitation." *Nature* **50**: 330–334, 366–368, 417–419, 571.

BROWN, Ernest William. (1920). "The Problem of the Moon's Motion." *Publications of the Astronomical Society of the Pacific* **32**: 93–104.

VON BRUNN, Albert. (1919). "Zu Hrn. Einsteins Bemerkung über die unregelmässigen Schwankungen der Mondlänge von der genäherten Periode des Umlaufs der Mondknoten." *Sitzungsberichte der Preussischen Akademie der Wissenschaften* (Berlin): 710–711.

BRUSH, Charles F. (1921). "Discussion of a Kinetic Theory of Gravitation II, and Some New Experiments in Gravitation." *Proceedings of the American Philosophical Society* **60**: 43–61. [Continued in **61**: 167–183; **62**: 75–89; **63**: 57–61; **64**: 36–50].

CAMPBELL, William Wallace (1913). "A Possible Absorption of Gravitational Energy." *Publications of the Astronomical Society of the Pacific* **25**: 46–47.

COOK, Alan (1987). "Experiments on Gravitation." In *Three Hundred Years of Gravitation*. Stephen Hawking and Werner Israel, eds. 51–79. Cambridge: Cambridge University Press.

——— (1988). "Experiments on Gravitation." *Reports of Progress in Physics* **51**: 707–757.

CRÉMIEU, Victor. (1905a). "Attraction observée entre gouttes liquides suspendues dans un liquide de même densité." *Comptes Rendus des Séances de l'Académie des Sciences de Paris* **140**: 80–83.

——— (1905b). "Recherches sur la gravitation." *Comptes Rendus des Séances de l'Académie des Sciences de Paris* **141**: 653–656, 713–715.

——— (1905c). "Recherches expérimentales sur la gravitation." *Bulletin de la Société Française de Physique* **3**: 485–99, 103. [Reprinted in *Journal de Physique Théorique et Appliquée* (4) **5** (1906): 25–39].

——— (1906). "Recherches sur la gravitation." *Comptes Rendus des Séances de l'Académie des Sciences de Paris* **143**: 887–889.

——— (1907). "Recherches comparées sur les forces de gravitation dans les gaz et les liquides." *Bulletin de la Societé Française de Physique* **1**: 33–47, 14–15. [Reprinted in *Journal de Physique Théorique et Appliquée* (4) **6** (1907): 284–298].

——— (1910). "Sur une erreur systématique qui limite la précision de l'expérience de Cavendish. Méthode nouvelle pour l'étude de la gravitation." *Comptes Rendus des Séances de l'Académie des Sciences de Paris* **150**: 863–866.

——— (1913). "Effects de la flexion aux points d'attache du fil d'une balance de torsion." *Comptes Rendus des Séances de l'Académie des Sciences de Paris* **156**: 617–620.

——— (1917a). "Recherches expérimentales sur la gravitation." *Comptes Rendus des Séances de l'Académie des Sciences de Paris* **165**: 586–589, 688.

——— (1917b). "Nouvelles recherches expérimentales sur la gravitation." *Comptes Rendus des Séances de l'Académie des Sciences de Paris* **165**: 670–672.

CURIE, Pierre & CURIE, Marie (1900). "Les Nouvelles Substances radioactives et les rayons qu'elles émettent." In *Rapports présentés au Congrès International de Physique réuni à Paris en 1900 sous les auspices de la Société Française de Physique*. Vol. 3: 79–114. Charles-Édouard Guillaume and Lucien Poincaré, eds. Paris: Gauthier-Villars.

DE SITTER, Willem. (1912). "Absorption of Gravitation." *The Observatory* **35**: 387–393.

——— (1913). "On Absorption of Gravitation and the Moon's Longitude." *Proceedings of the Royal Academy of Amsterdam* **15**: 808–824, 824–830.

——— (1927). "On the Secular Accelerations and the Fluctuations of the Longitudes of the Moon, the Sun, Mercury and Venus." *Bulletin of the Astronomical Institutes of the Netherlands* **4**: 21–38.

——— (1928). "On the Rotation of the Earth and Astronomical Time." *Nature* **121** (supplement): 99–106.

DORN, Ernst. (1903). "Versuch über die zeitliche Gewichtsänderung von Radium." *Physikalische Zeitschriften* **4**: 530–531.

DRAGONI, Giorgio & MALTESE, Giulio. (1994). "Quirino Majorana e l'assorbimento gravitazionale." *Giornale di Fisica* **35**: 245–292.

DRUDE, Paul. (1897). "Über Fernewirkungen." *Annalen der Physik* **62**: i–xlix, 693.

DYSON, Frank W. & CULLEN, R. T. (1929). "Variability of the Earth's Rotation." *Monthly Notices of the Royal Astronomical Society* **89**: 549–551.

EARMAN, John & GLYMOUR, Clark. (1980). "Relativity and Eclipses: the British Eclipse Expeditions of 1919 and Their Predecessors." *Historical Studies in the Physical Sciences* **11**: 49–85.

EDDINGTON, Arthur Stanley. (1915). "Some Problems of Astronomy. XIX: Gravitation." *The Observatory* **38**: 93–98.

——— (1922). "Majorana's Theory of Gravitation." *Astrophysical Journal* **56**: 71–72.

EINSTEIN, Albert. (1919a). "Bemerkung über periodische Schwankungen der Mondlänge, welche bischer nach der Newtonschen Mechanik nicht erklärbar schienen." *Sitzungsberichte der Preussischen Akademie der Wissenschaften* (Berlin): 433–436.

——— (1919b). "Bemerkung zur vorstehenden Notiz." *Sitzungsberichte der Preussischen Akademie der Wissenschaften* (Berlin): 711.

Eötvös, Roland. (1896). "Untersuchungen über Gravitation und Erdmagnetismus." *Annalen der Physik und Chemie* (3) **59**: 354–400.

Eötvös, Roland, Pekár, Desiderius & Fekete, Eugen. (1922). "Beiträge zum Gesetze der Proportionalität von Trägheit und Gravität." *Annalen der Physik* (4) **68**: 11–66.

Erismann, Theodor. (1908). "Zur Frage nach der Abhängigkeit der Gravitationskraft von Zwischenmedium." *Vierteljahresschrift der Naturforschenden Gesellschaft zu Zürich* **53**: 157–185.

——— (1911). "Sur la dépendance de la force de gravitation du milieu intermédiaire à travers lequel elle s'exerce." *Archives des Sciences Physiques et Naturelles* **31**: 36–45.

Fink, Karl J. (1982). "*Actio in distans*, Repulsion, Attraction: the Origin of a 18th Century Fiction." *Archiv für Begriffsgeschichte* **25**: 69–87.

Forch, Carl. (1903a). "Bewirken radioaktive Substanzen eine Absorption von Gravitationsenergie?" *Physikalische Zeitschrift* **4**: 318–319.

——— (1903b). "Weitere Versuche zur Frage: Bewirken radioaktive Substanzen eine Absorption von Gravitationsenergie?" *Physikalische Zeitschrift* **4**: 443–445.

Fotheringham, J. K. (1927). "Changes in the Length of the Day." *Nature* **119**: 318–319.

——— (1932). "The Determination of the Accelerations and Fluctuations in the Motions of the Sun and Moon." *The Observatory* **55**: 305–316.

Geigel, Robert. (1903). "Über Absorption von Gravitationsenergie durch radioaktive Substanz." *Annalen der Physik* (4) **10**: 429–435.

Gillies, George T. (1987). "The Newtonian Gravitational Constant." *Metrologia—International Journal of Scientific Metrology* **24** (supplement): 1–56.

——— (1990). "Resource Letter MNG-1: Measurements of Newtonian Gravitation." *American Journal of Physics* **58**: 525–534.

Grayson, H. W. (1978). "An Experiment to Test the Gravitational Field of a Mass at its Center." In *The Theory of Relativity Revisited.* H. W. Grayson, ed. 253–261. Philadelphia: Dorrance.

Groten, Erwin. (1972). "The Problem of Gravitational Absorption." *Geophysical Journal of the Royal Astronomical Society* **27**: 447–448.

Hawes, Joan L. (1968). "Newton's Revival of the Aether Hypothesis and the Explanation of Gravitational Attraction." *Notes and Records of the Royal Society of London* **23**: 200–212.

Heydweiller, Adolf. (1902). "Zeitliche Gewichtsänderungen radioaktiver Substanz." *Physikalische Zeitschrift* **4**: 81–82.

Jeffreys, Harold. (1918). "A Theory of Physical Forces." *The Observatory* **41**: 217–220.

Kaufmann, Walter. (1903). "Bemerkungen zu der Arbeit des Hrn. R. Geigel: Über die Absorption von Gravitationsenergie durch radioaktive Substanz." *Annalen der Physik* (4) **10**: 894–896.

Kleiner, Alfred. (1905). [Influence des milieux sur la gravitation]. *Archive des Sciences Physiques et Naturelles* (4) **20**: 420–423.

Laager, Fritz. (1904). *Versuch mit der Drehwage die Abhängigkeit der Gravitation von Zwischermedium nachzuweisen.* Bern: Haller.

Lodge, Oliver Joseph. (1912). "The Discovery of Radioactivity and Its Influence on the Course of Physical Science." *Journal of the Chemical Society* **101**: 2005–2031.

McGuire, J. E. & Tamny, Martin. (1983). *Certain Philosophical Questions: Newton's Trinity Notebook.* Cambridge: Cambridge University Press.

MAJORANA, Quirino. (1918/9). "Nuove ipotesi cosmogoniche e nuovo fenomeno gravitazionale." *Atti della Reale Accademia delle Scienze di Torino* **54**: 667–669.

——— (1919/20a). "Sulla gravitazione." *Atti della Reale Accademia delle Scienze di Torino* **55**: 69–88.

——— (1919–20b). "Sulla gravitazione." *Rendiconti della Reale Accademia dei Lincei. Classe di Scienze Fisiche, Matematiche e Naturali* (5) **28** (2° Semestre): 165–174, 221–223, 313–317, 416–421, 480–489; **29** (1° Semestre): 23–32, 90–99, 163–169, 235–240.

——— (1919a). "Sur la gravitation." *Comptes Rendus des Séances de l'Académie des Sciences de Paris* **169**: 646–649.

——— (1919b). "Expériences sur la gravitation." *Comptes Rendus des Séances de l'Académie des Sciences de Paris* **169**: 719–721.

——— (1920). "On Gravitation. Theoretical and Experimental Researches." *London, Edinburgh and Dublin Philosophical Magazine* (6) **39**: 488–504.

——— (1921). "Sur l'absorption de la gravitation." *Comptes Rendus des Séances de l'Académie des Sciences de Paris* **173**: 478–479.

——— (1921–22). "Sull'assorbimento della gravitazione." *Rendiconti della Reale Accademia dei Lincei. Classe di Scienze Fisiche, Matematiche e Naturali* (5) **30** (2° Semestre): 75–79, 289–294, 350–354, 442–446; **31** (1° Semestre): 41–45, 81–86, 141–146, 221–226, 343–346.

——— (1930). "Quelques recherches sur l'absorption de la gravitation par la matière." *Journal de Physique et le Radium* (7) **1**: 314–323.

——— (1941). "Le mie ricerche scientifiche." *Nuovo Cimento* **18**: 71–86.

——— (1954). "Su di un'ipotesi cosmogonica." *Rendiconti della Accademia Nazionale dei Lincei. Classe de scienze fisiche, matematiche e naturali* (8) **17**: 150–157.

——— (1957a). "Sull'ipotesi dell'assorbimento gravitazionale." *Rendiconti della Academia Nazionale dei Lincei. Scienze fisiche, matematiche e naturali* (8) **22**: 392–397.

——— (1957b). "Ipotetiche conseguenze dell'assorbimento gravitazionale." *Rendiconti della Academia Nazionale dei Lincei. Scienze fisiche, matematiche e naturali* (8) **22**: 397–402.

MOYER, Donald Franklin. (1978). "Revolution in Science: the 1919 Eclipse Test of General Relativity." In *On the Path of Albert Einstein*. Arnold Perlmutter and Linda F. Scott, eds. 55–102. New York: Plenum.

NEWCOMB, Simon. (1903). "On the Desirableness of a Re-investigation of the Problems Growing Out of the Mean Motion of the Moon." *Monthly Notices of the Royal Astronomical Society* **63**: 316–324.

——— (1909). "Fluctuations in the Moon's Mean Motion." *Monthly Notices of the Royal Astronomical Society* **69**: 164–169.

OPPENHEIM, S. (1920). "Kritik des Newtonschen Gravitationsgesetzes." In *Encyklopädie der mathematischen Wissenschaften*. Band VI.2.B *Astronomie*. K. Schwarzschild, S. Oppenheim and W. Dyck, eds. 80–158. Leipzig: B. G. Teubner.

PERUCCA, Eligio. (1958). "Commemorazione del socio Quirino Majorana." *Rendiconti delle Sedute della Accademia Nazionale dei Lincei* **25**: 354–362.

POINCARÉ, Jules Henri. (1906/7). "Les Limites de la Loi de Newton." *Bulletin Astronomique* **17**: 121–269 (1953).

POTTER, Harold Herbert. (1922). "Note on the Gravitational Acceleration of Bismuth." *Physical Review* **19**: 187–188.

POYNTING, John Henry. (1900). "Recent Studies in Gravitation." *Nature* **62**: 403–408.

PRESTON, Samuel Tolver. (1881). "On the Importance of Experiments in Relation to the Mechanical Theory of Gravitation." *London, Edinburgh and Dublin Philosophical Magazine* (5) **11**: 391–393.

ROSENFELD, Léon. (1969). "Newton's View on Aether and Gravitation." *Archive for History of Exact Sciences* **6**: 29–37.

ROSEVEARE, N. T. (1982). *Mercury's Perihelion from Le Verrier to Einstein*. Oxford: Clarendon Press.

ROSS, Franck Elmore. (1911). "Empirical Short-Period Terms in the Moon's Mean Longitude." *Monthly Notices of the Royal Astronomical Society* **72**: 27–28.

RUSSELL, Henry Norris. (1921). "On Majorana's Theory of Gravitation." *Astrophysical Journal* **54**: 334–346. [Reprinted in *Contributions of the Mount Wilson Observatory* No. 216: 1–13].

SCHLOMKA, Teodor. (1927–30). "Über die Abhängigkeit der Schwerkraft von Zwischenmedium." *Zeitschrift für Geophysik* **3**: 397–400; **6**: 392–396.

SCHNELLER, H. (1934). "Nekrolog. Kurt Felix Bottlinger." *Vierteljahresschrift der astronomischen Gesellschaft zu Leipzig* **69**: 350–353.

SCHUSTER, Arthur. (1903). "Cosmical Radio-activity." *Chemical News* **88**: 166–167.

SEE, Thomas Jefferson Jackson. (1917). *Electrodynamic Wave-Theory of Physical Forces*. Vol. I. Bulletins 1 to 6 inclusive. ["Announcing the discovery of the physical cause of magnetism, of electrodynamic action, and of universal gravitation. With proof that these fundamental forces of nature are due to the interpenetration of waves propagated with the velocity of light through the free aether, but more slowly through the solid masses, whence arises also the refraction, dispersion and perhaps absorption of part of the wave energy, and thus the hitherto unexplained fluctuations of the Moon's mean motion established by Newcomb in 1909, and justly pronounced the most enigmatical phenomenon presented by the celestial motions."] Lynn (MA): Thos. P. Nichols & Sons.

VON SEELIGER, Hugo. (1895). "Über das Newton'sche Gravitationsgesetz." *Astronomische Nachrichten* **137**: 129–136.

——— (1896). "Über das Newton'sche Gravitationsgesetz." *Sitzungsberichte der Bayerischen Akademie der Wissenschaften zu München* **26**: 373–400.

——— (1909). "Über die Anwendung der Naturgesetze auf das Universum." *Sitzungsberichte der Königlichen Bayerischen Akademie der Wissenschaften zu München. Mathematisch-physikalische Klasse* **39** (4. Abhandlung): 1–25.

SPENCER JONES, Harold. (1926). "The Rotation of the Earth." *Monthly Notices of the Royal Astronomical Society* **87**: 4–31.

——— (1939). "The Rotation of the Earth, and the Secular Acceleration of the Sun, Moon and Planets." *Monthly Notices of the Royal Astronomical Society* **99**: 541–558.

——— (1955). "Definition of the Second of Time." *Nature* **176**: 669–670.

——— (1956). "The Rotation of the Earth." In *Encyclopedia of Physics [Handbuch der Physik]*. Vol. 47: 1–23. S. Flügge, ed. Berlin: Springer.

STEENBECK, Max & TREDER, Hans-Jürgen. (1984). "Möglichkeiten der experimentellen Schwerkraftforschung." *Veröffentlichungen des Forschungbereichs Kosmische Physik* (Heft 11): 1–46.

TAYLOR, W. B. (1876). "Kinetic Theories of Gravitation." *Annual Report of the Board of Regents of the Smithsonian Institution*: 205–282.

THOMSON, William (Lord KELVIN). (1903). [Discussion on the nature of the emanations from radium]. *Report of the Meeting of the British Association for the Advancement of Science* **73**: 535–537.

WHITTAKER, Edmund. (1951–53). *A History of the Theories of Aether and Electricity*. 2 vols. London: T. Nelson and Son.

WILSON, H. A. (1922). "Note on the Ratio of Mass to Weight for Bismuth and Aluminium." *Physical Review* **20**: 75–77.

WOODWARD, James F. (1972). "The Search for a Mechanism: Action-at-a-Distance in Gravitational Theory." Ph.D. dissertation, University of Denver, Denver (CO) [UMI 72-33,077].

ZEEMAN, Pieter. (1918). "Some Experiments on Gravitation. The Ratio of Mass to Weight for Crystals and Radioactive Substances." *Proceedings of the Royal Academy of Amsterdam* **20**: 542–553.

ZENNECK, J. (1901). "Gravitation." In *Encyklopädie der mathematischen Wissenschaften, mit Einschluß an ihrer Anwendung*. Band V.1. *Physik*. Arnold Sommerfeld, ed. 25–67. Leipzig: Teubner.