The Cosmological Woes of Newtonian Gravitation Theory

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In November 1894, the astronomer Hugo Seeliger sent the journal *Astronomische Nachrichten* a short article pointing out that Newton's law of gravitation could not be applied without modification to an infinite universe with a roughly uniform matter distribution. The problem Seeliger described was exceedingly simple. As we shall see in Section 1 below, it could be developed with just a few moments thought. Thus it is no surprise that Seeliger was not the first to notice this problem. But he was the first to lay it out with sufficient vigor that the need for a solution of some sort could not be avoided. Over the decades to follow, the problem lurked quietly in the corners of gravitational and cosmological theory, with proposals for its resolution reflecting whatever were the then current trends in physical theory.

The simplest solution—possibly that of Newton himself—was just to deny that Seeliger's argument was valid. This untenable 'no-solution-needed' solution was in the minority of those views expressed in the historical record. The majority felt that the problem revealed that one or other of the commitments of Newtonian cosmology required modification. These commitments can be collected into three groups:

> **Cosmological.** Space is infinite, Euclidean and filled with a (near) uniform mass distribution.
> **Gravitational.** All bodies attract one another according to Newton's inverse square law of gravitation.
> **Kinematical.** Newton's three laws of motion.

The earliest escapes were sought in minute adjustments of Newton's inverse square law of gravitational attraction. Seeliger and Neumann proposed augmenting the

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law with a tiny correction term whose effect would only become apparent at cosmic distances. More diverse escapes were also sought. Kelvin proposed that ethereal matter may not gravitate, allowing at least this form of matter to be spread uniformly through space. August Föppl suggested that there may be negative gravitational masses. Charlier, Selety and others proposed that the distribution of matter in the universe may have a hierarchical structure that allowed a vanishing mean matter density yet without concentrating all matter in a central island. Soon virtually every supposition buried in the 'cosmological' or 'gravitational' groups had been weighed and its modification proposed. Lense even explored the possibility of an escape through an alternative geometry for space.

The problem achieved its moment of greatest glory when Einstein (1917a) thrust it into his first attempts at a relativistic cosmology. Agreeing with Seeliger, Einstein saw the problem as revealing a need for adjustment of Newton's inviolable law of gravitation. He used the adjustment as a foil to motivate the introduction of a cosmological term in the gravitational field equations of general relativity. However he also used the paradox to pose a dilemma for Newtonian cosmology: either the universe was homogeneous and gravitationally paradoxical or its matter was concentrated in a physically untenable island universe. Selety soon showed, however, that this was a false dilemma. The work on hierarchical cosmologies had already shown an escape between the horns of the dilemma.

This work in the 1920s marked the end of the first phase of the problem posed by Seeliger; my purpose in this paper is to review the course of this first phase.[1] In a sequel I will review the later phase initiated by the discovery of the expansion of the universe and the advent of dynamical cosmologies. There it is found that the most satisfying escape from the problem lies not in modification of either 'cosmological' or 'gravitational' assumptions of Newtonian cosmology. Rather it lay in a modification of its kinematical core. The resolution depends on a hitherto obscured sense of relativity of acceleration in Newtonian cosmology and finds its fullest expression in the connection between gravitation and space-time curvature in Newtonian space-time theory.

1. The non-convergence of gravitational force in Newtonian cosmology

In order to fix our topic, it will be helpful to give a brief and simple derivation of the problem that exercised Seeliger. In a Newtonian universe, the gravitational force exerted on a test body of unit mass is the resultant of the forces exerted by all the masses of the universe, which we shall assume to be distributed uniformly in space with mass density ρ. This force is computed by an integration over all these masses. This integral fails to converge. It can take on any value according to how we approach the limit of integration over all space.

To see this lack of convergence, picture the uniform matter density ρ as distributed in concentric spherical shells of very small thickness Δr all centered on

[1] I gratefully acknowledge the assistance of the many before me who have mapped out various parts of the story in greater and lesser detail, especially Jaki (1979), Jammer (1961: 127–29), North (1965: 16–23, 180–185) Oppenheim (1920: 86–87), Sklar (1976) and Zenneck (1901: 51–53).

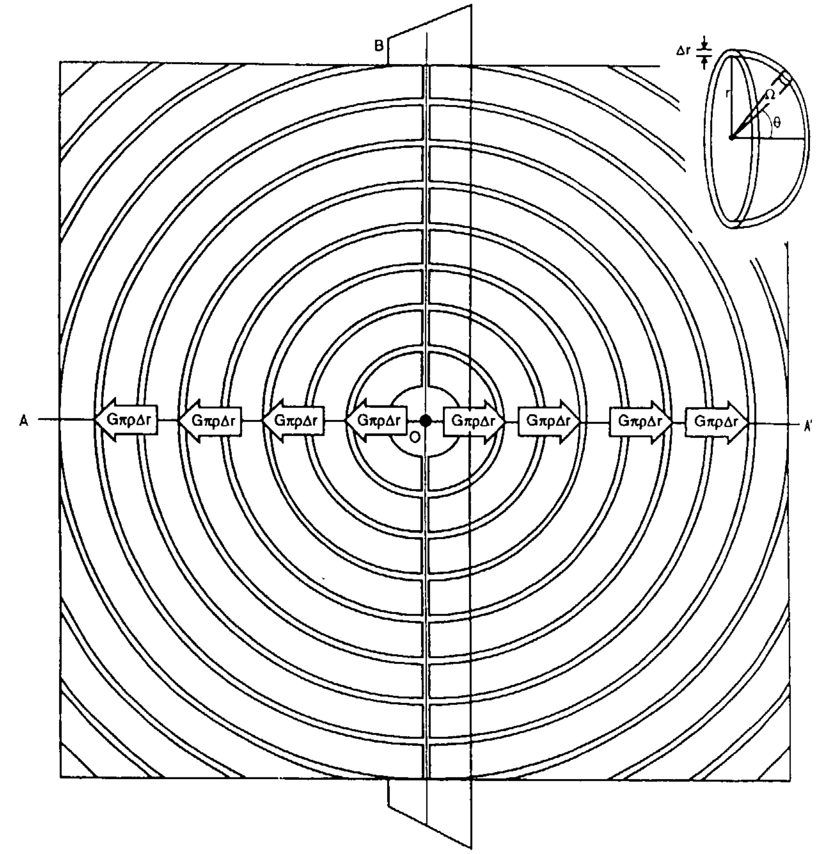

Figure 1. Non-convergence of force on a test mass in Newtonian cosmology.

the unit test mass at O, as shown in Figure 1. Choose some arbitrary axis AA' and divide all the spherical shells into hemispherical shells by passing a plane B through O and perpendicular to AA'. Each hemispherical shell exerts a force on the test mass in direction AA' and its magnitude is independent of the radius r of the shell. To see the independence from r, consider how much matter in a shell at radius r is subtended by some small solid angle Ω at O. That amount of matter increases with r^2, but the gravitational force it exerts on the test mass decreases with $1/r^2$. So, overall, this force will be independent of r. This holds for each element in the shell, so we conclude that the net force exerted by the entire shell is a constant, independent of its radius r. The direct calculation reveals that the value of the constant is $G\pi\rho\,\Delta r$.[2] Thus the net force F on the test mass along an arbitrarily chosen axis AA' is given by an infinite series, each term representing the force due to one hemispherical shell

$$F = G\pi\rho\,\Delta r - G\pi\rho\,\Delta r + G\pi\rho\,\Delta r - G\pi\rho\,\Delta r + G\pi\rho\,\Delta r - \dots$$

The series has alternating signs since shells on alternating sides exert a force in alternating direction. This alternating series is well known not to converge. According to how one groups and reduces the terms in the series, the sum can have many different values.[3] Each corresponds to a different way of approaching the limit of infinitely many masses in the associated integration.

2. Seeliger's formulations of the problem

Seeliger's papers, especially his (1895a), contain the most detailed and general development of the problem. The price is that his exposition is the most cumbersome of all expositions; he alone resorts to infinite series expansions in Legendre polynomials and includes tidal forces in the analysis. Virtually all later commentators managed to reduce the exposition of the core difficulty to one or two lines of formulae.

Seeliger initiated his discussion in his Seeliger 1895a (p. 129) by asking whether Newton's law of gravitation holds exactly for masses separated by "immeasurably great distances" [*unermesslich große Entfernung*]. While observational astronomy gives the strongest reasons to believe the law within our planetary system, we have no similar foundation in experience for the law on the larger, cosmic scale. Nonetheless, he urged, the matter can be decided by applying the law to "simple and obvious examples" [*einfachen und naheliegenden Beispielen*] on the cosmic scale. It turns out that "thoroughly possible and conceivable assumptions lead to quite impossible or unthinkable conclusions" so that

[2] The total mass in a ring with an angular position θ and an angular thickness of $d\theta$ in the shell is $\rho r\, d\theta\, 2\pi r \sin\theta\,\Delta r$. Therefore the total force exerted by the hemisphere at r is

$$2\pi G\rho\,\Delta r \int_0^{\pi/2} \sin\theta\,\cos\theta\, d\theta = G\pi\rho\,\Delta r.$$

[3] For example

$$\begin{aligned}(G\pi\rho\,\Delta r - G\pi\rho\,\Delta r) + (G\pi\rho\,\Delta r - G\pi\rho\,\Delta r) + (G\pi\rho\,\Delta r - G\pi\rho\,\Delta r) + \dots\\ = 0 + 0 + 0 + \dots\\ = 0\end{aligned}$$

and

$$\begin{aligned}G\pi\rho\,\Delta r - (G\pi\rho\,\Delta r - G\pi\rho\,\Delta r) - (G\pi\rho\,\Delta r - G\pi\rho\,\Delta r) - \dots\\ = G\pi\rho\,\Delta r - 0 - 0 - \dots\\ = G\pi\rho\,\Delta r.\end{aligned}$$

> Newton's law, applied to the immeasurably extended universe, leads to insuperable difficulties and irresolvable contradictions if one regards the matter distributed through the universe as infinitely great.[4] (Seeliger 1895a: 132)

To arrive at these difficulties, Seeliger asked after the gravitational effect of the masses of the universe at a point O in space. To compute these effects, he laid out a spherical coordinate system (r, θ, γ) centered on O. He represented the discontinuously distributed masses of the universe by an equivalent continuous distribution with density ρ. He found the gravitational effect of the masses between radial coordinate values $r = R_0$ and $r = R_1$ to be

$$\text{(1a)}\qquad \varphi = -G \iiint_{\text{space}} \frac{\rho}{r}\, dV = -G \int_0^{2\pi}\int_0^{\pi}\int_{R_0}^{R_1} \rho r \sin\gamma\, dr\, d\gamma\, d\theta,$$

$$\text{(1b)}\qquad F_x = -\frac{d\varphi}{dx} = G \int_0^{2\pi}\int_0^{\pi}\int_{R_0}^{R_1} \rho \sin\gamma\cos\gamma\, dr\, d\gamma\, d\theta,$$

$$\text{(1c)}\qquad Z_x = -\frac{d^2\varphi}{dx^2} = 2G \int_0^{2\pi}\int_0^{\pi}\int_{R_0}^{R_1} \frac{\rho}{r}\, dr \sin\gamma\, \frac{3\cos^2\gamma - 1}{2}\, d\gamma\, d\theta,$$

where φ is the gravitational potential at O and dV a volume element of space. F_x is the gravitational force in the direction of the x coordinate axis, which aligns with angular coordinate $\gamma = 0$. Z_x, called "strain" [*Zerrung*] by Seeliger, is the tidal gravitational force acting in the x direction on neighboring masses located on the x axis. It is defined as the difference in gravitational force acting between two such bodies per unit distance of separation. For further details, including a synopsis of Seeliger's derivation, see Appendix A.

The integrals (1b) and (1c), remain well defined as long as R_1 is finite. However, Seeliger observed, if they are applied to an infinite matter filled universe, they cease to be well defined:

> If ρ is a finite magnitude for infinitely large regions, then, in general, F_x and Z_x are completely undetermined, as long as one makes no definite assumption on the way in which the finite values of R_1 become infinitely great. Therefore both quantities can equally well become infinite or remain finite.[5] (Seeliger 1895a: 131)

Seeliger's claim is that integrals of (1b) and (1c) give no definite results in the limit as R_1 goes to infinity; they vary according to the path taken to the limit.

To make good on this claim, Seeliger applied the formulae to the case of a universe filled with a homogeneous matter distribution of everywhere constant

[4] "durchaus mögliche und vorstellbare Annahmen zu ganz unmöglichen oden undenkbaren Consequenzen führen ... das Newton'sche Gesetz auf das unermesslich ausgedehte Universum angewandt auf unüberwindliche Schwierigkeiten und unlösbare Widerspruche führt, wenn man die im Weltall verstreute Materie als unendlich groß ansieht."

[5] "Wenn dann [ρ] eine endliche Größe für unendlich große Bezirke ist, dann werden im Allgemein [F_x] und [Z_x] völlig unbestimmt, so lange man über die Art, wie man von endlichen Werthen R_1 zu den unendlich großen gelangt, keine bestimmte Voraussetzung macht. Beide Größen können ebensogut unendlich werden, wie endlich beliben."

density ρ. To approach the limit, he took the region of integration bounded by R_1 to be a sphere, centered on an arbitrary point *other than O*, and the sphere was then allowed to grow infinitely large, as shown in Figure 2. If r is the distance of the point O from the center of the sphere, Seeliger reported that the force[6]

$$F_x \text{ is proportional to } r\,\rho, \tag{1b'}$$

and is directed towards the center of the sphere. The tidal force

$$Z_x \text{ is proportional to } \rho. \tag{1c'}$$

Since the location of the center of the sphere is arbitrary, the gravitational force is also arbitrary, taking any desired value, according to where one locates the center of the sphere.

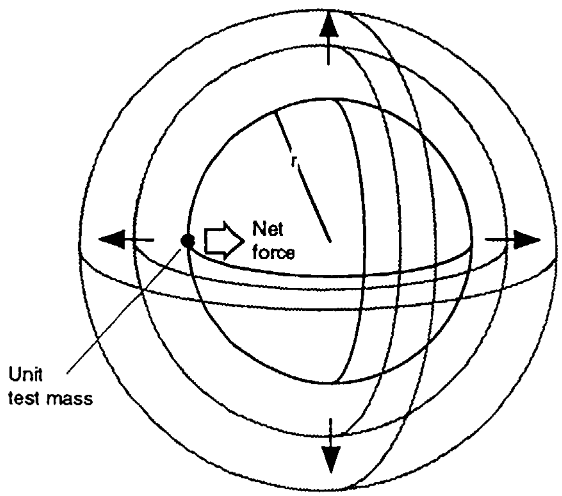

Figure 2. Seeliger's spheres.

[6] If $\mathbf{r}$ is the radius vector from the center of the sphere, one quickly sees that Seeliger's result is given more fully by the force $\mathbf{F} = -(4/3)G\pi\rho\,\mathbf{r}$ (as Seeliger pointed out in effect in his later Seeliger 1896: 379). To see this, note that once Seeliger's sphere has grown so that it touches the test mass, any extra matter added will exert no net gravitational force on the test mass. All extra matter will be added in spherical shells and a well-known theorem of Newtonian gravitation theory tells us that such shells exert no net force. Another familiar theorem helps us find the force exerted by the matter in the sphere that has just grown to touch the test mass. That force is the same force that would be exerted if all the matter in the sphere were concentrated at its center. That would be a mass $(4/3)\pi r^3\rho$ at a distance r from the test mass. It exerts a force on the unit test mass of magnitude $(4/3)G\pi\rho r$ towards the center of the sphere which gives the vector result $\mathbf{F} = -(4/3)G\pi\rho\,\mathbf{r}$. From this result, we can also derive an expression for the tidal force. If we imagine that the result defines a force field through space, then the differential force on two unit test masses separated by small distance $\Delta\mathbf{r}$ is given as $\Delta\mathbf{F} = -(4/3)G\pi\rho\Delta\mathbf{r}$. From this, we read off the tidal force as $Z_x = -(4/3)G\pi\rho$ and see that the result is the same along all axes. For a simple 'lines of force' development of this argument, see Norton 1993a.

Seeliger's analysis could well have ended there were it not for the nuisance that the tidal force (1c′) does not turn out to be indeterminate, in apparent contradiction with Seeliger's claim; its value is independent of the disposition of the spheres used in the integration. This is probably the reason Seeliger proceeded to his second example, a different infinite matter distribution. He considered the gravitational and tidal forces at the apex of a double cone of very small solid angle ω and constant mass density ρ, as shown in Figure 3.

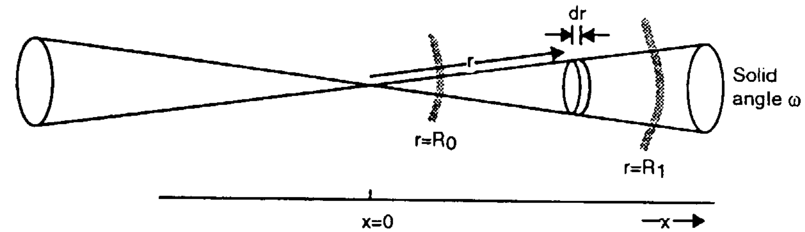

Figure 3. Seeliger's double cones.

The gravitational force due to each individual cone is infinite. Thus, he noted, the gravitational force exerted by both cones at the apex is of the form "$\infty - \infty$" and is indeterminate. The tidal force due to one cone acting in the direction of the cone's axis follows directly from substitution into (1c) with $\gamma = 0$. It is given by[7]

$$Z_x = 2\omega G \int_{R_0}^{R_1} \frac{\rho}{r}\,dr \tag{1c''}$$

and becomes infinite in the limit of infinite R_1. Since both cones produce the same tidal force—an expansion along the axis of the cone—the effect of both cones is double that of a single cone, so that the tidal force at the apex of the double cone is infinite.

This example of the double cone completed Seeliger's first analysis. It was unsatisfactory in so far as Seeliger had only shown non-convergence of tidal force in a rather contrived example of an infinite matter distribution, the infinite double cone. He had still not shown that there was a problem with tidal forces in a universe homogeneously filled with matter. This deficiency was remedied when Seeliger (1896) returned to review his results. There he showed that there was a way of approaching the limits in the integrals (1b) and (1c) for such a universe so that the gravitational force F_x vanished, but the tidal force became infinite. In concept the method was simple. Seeliger's double cones had yielded an infinite tidal force,

[7] More directly, the volume element at distance r to $r + dr$ from the apex contains mass $\rho\,\omega\,r^2\,dr$ and it exerts a gravitational force $F_x = G\rho\,\omega\,r^2\,dr/(r - x)^2$ on masses at coordinate position x on its axis. The tidal force is $(d^2F_x/dx^2)|_{x=0} = 2G\rho\,\omega\,dr/r$. Integration over one cone yields (1c″).

but they did not correspond to a matter distribution filling all space. All Seeliger needed was a shape similar to the double cone that would fill all space as it grew to infinite size. If this shape were well chosen, the tidal force integral (1c) would diverge along this path to the infinite limit, demonstrating that tidal forces were also ill-behaved, converging or diverging according to the way the limit in the integration is taken. Seeliger had no trouble in finding such a shape. It is given by

$$\log \frac{R_1}{R_0} = am + mP_2(\cos\gamma), \tag{2}$$

where R_0 and R_1 are the limits of integration of the radial coordinate r, $P_2(\cos\gamma) = (1/2)(3\cos^2\gamma - 1)$ is the second Legendre polynomial in $\cos\gamma$, a is some constant greater[8] than $1/2$ and m is a parameter which becomes infinite as the shape grows to infinite size. This expression corresponds to the shape shown in Figure 4. For convenience I will call the shape 'Seeliger's peanut.' As the shape grows, it becomes more elongated so that the tidal force at the center of the peanut grows without limit. However the peanut also grows in width so that in the limit it fills all of space.

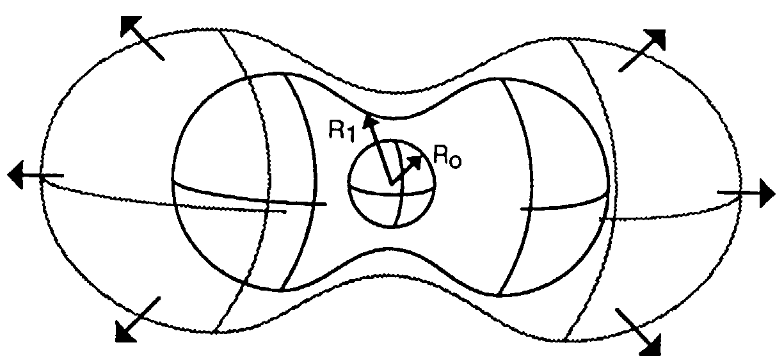

Figure 4. Seeliger's peanut.

The family of shapes specified in (2) is not the only one that has the property Seeliger sought. It does have the fortunate property, however, that the integrations of (1b) and (1c) become especially easy. The Legendre polynomial introduced in (2) combines with that already in (1c) to enable easy evaluation of the integral from known integrals. The force F_x is obviously zero from the symmetry of the shapes. For some fixed m, the tidal force is given by substitution of (2) into (1c)

[8] The need for this restriction on the value of a becomes apparent if the expression in (2) is rewritten as $am + mP_2(\cos\gamma) = m(a - 1/2 + (3/2)\cos^2\gamma)$.

yielding

$$Z_x = 2G\rho \int_0^{2\pi}\int_0^{\pi} \log\frac{R_1}{R_0}\, P_2(\cos\gamma)\sin\gamma\, d\gamma\, d\theta = \frac{8}{5}G\pi\rho\, m,$$

where the $\gamma = 0$ axis is aligned with the axis of rotational symmetry of the peanut. It diverges in the limit of infinite m.

Seeliger (1896: 380–382) completed his discussion with a qualitatively different type of demonstration of the problems faced by gravitation theory in a universe containing an infinite matter distribution. The argument will reappear often enough for it to be worth us labeling it the 'flux argument.' Seeliger considered some closed surface F in space which enclosed n masses $m_1, m_2, \ldots, m_n$. Through an application of Green's theorem, he arrived at the result for the gravitational potential φ

$$\int \frac{\partial\varphi}{\partial n}\, ds = -4\pi G(m_1 + \ldots + m_n),$$

where the integration extends over the entire surface of F and n is an inward pointing, unit, normal vector.[9] Seeliger then imagined the masses $m_1, m_2, \ldots, m_n$ distributed uniformly within the volume enclosed by F with constant density ρ. He rewrote his result in the form

$$M\left(\frac{\partial\varphi}{\partial n}\right) = -4\pi G\rho\frac{R}{S}, \tag{3}$$

where S is the surface area of F, R is its volume and the operator M returns the value of $\partial\varphi/\partial n$ averaged over the surface area of F. By selection of a large surface F enclosing sufficiently many masses, the right hand side of (3) can be made arbitrarily large. Therefore the average value of $\partial\varphi/\partial n$ can also be made arbitrarily large. Thus individual values of $\partial\varphi/\partial n$ can be made arbitrarily large as well. This already yields an undesirable result since it corresponds to arbitrarily large field strengths. Seeliger elaborated on its undesirability:

> It follows from potential theory that there must be in the *universe unlimited (infinitely) great accelerations* and indeed with every conceivable mass distribution. Therefore there are motions that begin with finite speed and lead to infinitely great speeds in finite time. This in itself already contains something absolutely inadmissible, if one does not want to call all of mechanics into question.[10] (Seeliger 1896: 382; emphasis in original)

[9] As in Appendix A1, I have replaced Seeliger's potential V by the now standard $-\varphi$ (note sign change) and restored G which Seeliger had set to one. Similarly, below, I replace Neumann's φ by $-\varphi$.

[10] "Nach der Potentialtheorie müssen demzufolge im *Universum unbegrenzt (unendlich) große Beschleunigungen* vorkommen und zwar bei jeder denkbaren Massenverteilung. Das sind also Bewegungen, die mit endlicher Geschwindigkeit beginnend in endlicher Zeit zu undendlich großen Geschwindigkeiten führen, was an sich schon eine absolute Unzulässigkeit enthält, wenn man nicht die ganze Mechanik in Frage stellen will." Here as elsewhere Seeliger slides rather too hastily from the conclusion that the field strength can be set without upper bound to the conclusion that it is actually infinite.

3. The puzzle of Neumann's elusive priority claim

Shortly after Seeliger had published his first note, he was answered with a cry from Carl Neumann that he had already seen the problem some twenty years before. Neumann's 1896 book is a lengthy treatise in electrical field theory, published after Seeliger's first communication. Its focus was the notion of electrostatic equilibrium, such as arises for electric charges in a conductor, if the forces between the charges are given by a Coulomb potential, which Neumann wrote as $\varphi(r) = -1/r$. The existence of such equilibrium was elevated (Neumann 1896: viii) to the "principle" (*Princip*) or "axiom" (*Axiom*) at the basis of the treatise and its project became the investigation of alternative forms for the potential function $\varphi(r)$ compatible with his axiom of equilibrium. To motivate his occupation with alternative forms of the potential, Neumann sought to cast doubt on what he called the "Newtonian potential function (*Newton'sche Potentialfunction*) $\varphi(r) = -1/r$" or sometimes "Newton's Law" (*Newton'sche Gesetz*). In the first chapter, he briefly reviewed objections to it, including one based in gravitation theory whose potential also conformed to "Newton's law." After mentioning the possibility that the Newton's law may require modification in the domain of very small distances, he proceeded:

> However a modification of Newton's law might also be called for not only in the very *small* but also in the very *large*, at least in case one entertains the usual representation that all of universal space is filled with stars to infinity in roughly uniform distribution. For then the universe would be looked upon in the relevant aspect as an infinitely great sphere of roughly constant density. And this infinitely great homogeneous sphere, representing the universe in all its entirety, would obviously, on the foundation of Newton's law, strive to draw in the individual heavenly bodies, such as f[or] ex[ample] the Sun, Mercury, Venus, Earth, Mars, etc., towards its center. Further, the intensity of the corresponding forces would be proportional to the displacements of the individual heavenly bodies from that center.
>
> Now, however, the surface of the universal sphere under discussion lies fully in the infinite. Therefore its center has a completely *undetermined position*. And so those forces, exerted by this universal sphere on the individual heavenly bodies, would be likewise completely *undetermined* in their direction and strength—which obviously would be absurd.[11] (Neumann 1896: 1–2; emphasis in original)

Neumann here has recapitulated one of the arguments that Seeliger gave for the in-

[11] "Aber nicht nur für sehr kleine, sondern auch für sehr größe Entfernungen dürfte eine Modification des Newton'schen Gesetzes geboten sein, wenigstens, falls man der gewöhnlichen Vorstellung sich hingiebt, daß der ganze Weltraum ins unendliche hin, in einigermassen gleichförmiger Vertheilung, von Sternen erfüllt sei. Denn als dann würde das Universum der Hauptsache nach anzusehen sein als eine unendlich große Kugel von einigermassen constanter Dichtigkeit. Und diese das Universums im Großen und Ganzen repräsentierende unendlich große homogene Kugel würde offenbar, bei Zugrundelegung des Newton'schen Gesetzes, die einzelnen Himmelskörper, wie z.B. Sonne, Mercur, Venus, Erde, Mars u.s.w., nach ihrem Centrum hinzuziehen bestrebt sein. Auch würden die Intensität der betreffenden Kräfte proportional sein mit den Abständen der einzelnen Himmelskörper von jenem Centrum.
Nun liegt aber die Oberfläche der in Rede stehenden Universalkugel überall im Unendlichen. Folglich hat ihr Centrum eine völlig unbestimmte Lage. Und es würden daher jene von dieser Universalkugel auf die einzelnen Himmelskörper ausgeübten Kräfte, ihrer Richtung und Stärke nach, ebenfalls völlig unbestimmt sein;—was offenbar absurd wäre."

determinacy of gravitational force in a universe filled homogeneously with matter. In Seeliger's hands, the result arose from using a sphere that grows to infinity to evaluate the integral (1b), recovering (1b′). Neumann's treatment was far briefer. But it really did not need to be any more elaborate. Neumann was merely calling to mind quite standard results: A test body within a homogeneous sphere is drawn to its center by a gravitational force of magnitude proportional to the distance from the center. Allowing the sphere to grow by adding layers to it does not alter the result, presumably even if the sphere is allowed to grow infinitely large.

Aside from this brief application, the cosmological problems of Newton's theory of gravitation played no role in Neumann's 292 page tome. A footnote to the last sentence of the passage quoted above contained mention of Seeliger's work and Neumann's claim of priority:

> Already a long time ago these matters were remarked on by me in the *Abhandl[ungen] der K[öniglichen] Sächs[ischen] Ges[ellschaft] d[er] Wiss[enschaften]*, 1874, page 97, 98. Otherwise something similar has also been remarked on by the astronomer Seeliger (in Munich) in *Astron[omische] Nachrichten*, 1894, Vol. 137, page 3272.[12] (Neumann 1896: 2n)

This priority claim produced an effusive and apologetic response from Seeliger:

> Since [publication of Seeliger 1895a] it has become known to me that Carl Neumann[13] had remarked already on difficulties of a similar kind that may be represented as special cases of the arguments advanced by me. With agreement with such an outstanding researcher and also the circumstance that the considerations advanced by me were indeed also expressed in another form, but in doing so without changing their essential content, it might appear superfluous to return to this subject. On the other hand ... [14] (Seeliger 1896: 373)

Neumann's priority and the citation Neumann himself gives have been routinely repeated in historical surveys.[15] But Seeliger need not have been so effusive. The work Neumann cites proves rather hard to find. It is usually taken to be Neumann 1873, whose title page is dated 1873 but which appeared in a volume assembled in 1874. The first problem is that the pages Neumann cites—pages 97–98—are not within the pagination of Neumann 1873, which occupies pages 417–524 of

[12] "Schon vor langer Zeit is von mir auf diese Dinge aufmerksam gemacht worden, in den Abhandl. der K. Sächs. Ges. d. Wiss., 1874, Seite 97, 98. Uebrigens ist Aehnliches neuerdings auch von dem Astronomen Seeliger (in München) bemerkt worden, in den Astron. Nachrichten, 1894, Bd. 137, Seite 3272."

[13] Seeliger's footnote at this point reads "Comp[are] (*Vergl.*) Carl Neumann, Allgemeine Untersuchungen über das Newton'sche Princip etc. Leipzig 1896, S. 1" which is Neumann (1896: 1).

[14] "Seitdem ist mir bekannt geworden, daß Carl Neumann schon früher auf Schwierigkeiten ählicher Art aufmerksam gemacht hat, die sich als specielle Fälle der von mir vorgebrachten Argumente darstellen dürften. Die Zustimmung eines so herrvorragenden Forschers und auch der Umstand, daß sich die von mir angestellten Ueberlegungen zwar auch in anderer Form aussprechen lassen, daß hierdurch aber ihr wesentlicher Inhalt nicht sich ändert, könnte es überflüssig erscheinen lassen, auf diesen Gegenstand zurückzukommen. Andererseits ... "

[15] For example Zenneck's (1901: 51) authoritative Teubner encyclopedia article allows that Neumann had "first hinted" (*hat wohl zuerst darauf hingewiesen*) at an undetermination of the gravitational force in his work of 1874. Mention of independent discovery is given also by Oppenheim (1920: 86) in his Teubner encyclopedia article. Jammer (1961: 127) gives Neumann credit as "the first to call attention to such difficulties [of Newton's law of gravitation applied to the universe as a whole]."

the relevant volume. (Pages 97–98 of the same volume are filled with the work of another author.) The second is that it is by no means obvious where in the paper Neumann addresses gravitational problems of cosmology. I cannot say that the discussion is not there, buried somewhere in the paper's lengthy and technical discussion of electrodynamic forces. But I can say that I could not find it and that, if it is there, it is not given any prominence whatever.

Thus Neumann's claim presents something of a puzzle. Did Neumann really lay out the problem decisively in the 1870s? Perhaps the incorrect pagination 97–98 stems from Neumann citing his own work from a separatum. The pagination of separata used not to match that of the journal printing. Each article in separatum would start at page 1. Since Neumann 1873 exceeded 100 pages in length, a separatum may well include pages 97–98. This is a kind construal, however, especially since nothing pertaining to the gravitation problem seems to appear on the pages of the article that would correspond to pages 97–98 of a separatum. More plausible is that Neumann just gave an incorrect citation. In the same footnote in which he cited his own work of 1874, he also cited Seeliger 1895a. As the reader can see from the above quote, the citation is incorrect. Neumann gives the year of publication as 1894. The article was signed "München 1894 November" but appeared in Volume 137 of 1895. Similarly his citation is to *page* (*Seite*) 3272. The correct pagination is pages 129–136. The article appears in *issue* number 3273—even the issue number is off by one! This is strong evidence that Neumann's citations are inaccurate.

If Neumann's citation is incorrect, should we look elsewhere, perhaps more widely in the 1874 volume of *Abhandlungen der mathematisch-physischen Classe der Königl. Sächsichen Gesellschaft der Wissenschaften* cited? That volume contains nine articles submitted and printed in the period 1871–1874. Only one is by Neumann and is Neumann 1873. We cannot rule out the possibility of another of Neumann's publications containing the work claimed. If that other work exists, however, later reviewers and historians have given no hint of it. Zenneck (1901: 51) cites the relevant work as "Leipz. Abh. 1874", Oppenheim (1920: 86) gives it as "Leipzig. Ber. 26 (1874), p. 97" and Tolman (1934: 322) as "*Abh. d. Kgl. Sächs. Ges. d. Wiss. zu Leipzig, math.-nat Kl.* **26**, 97 (1874)" suggesting none looked further than Neumann's own (1896: 1) citation (although Oppenheim and Tolman's "26" is a mystery). Jammer (1961: 127) cites Neumann 1873 directly. And Seeliger (1896: 1) himself does not cite any work of Neumann from the 1870s; he merely cites Neumann (1896: 1) and we may wonder what subtlety is masked by Seeliger's " . . . also expressed in another form. . . . "

While I cannot resolve the issue of Neumann's priority, we can at least be fairly confident that he deserved far less than Seeliger conceded. At best in his work of the 1870s he may have noticed that the relevant integral for gravitational force fails to converge. But this is not to say that he recognized that failure to amount to a very significant theoretical problem, one that deserved detailed and prominent investigation in its own right and one that ought to be brought with vigor to much wider notice. It was Seeliger who did that. Indeed the inclusion of the problem in the introductory pages of Neumann's later work is something of an oddity

since that work has nothing to do with gravitation and cosmology. It reads like an overreaching attempt to secure priority for a result that Neumann may never quite have published—but certainly wished that he had. As it turns out, the bulk of citation to the problem in the decades following Seeliger's work attribute the result to Seeliger, either explicitly or implicitly and without mention of priority in Neumann's work of the 1870s.[16]

One final irony remains. While Seeliger's development of the problem involved some detailed calculation, its existence, as Neumann showed, can be made apparent through quite simple reflection. Thus it would be surprising if Seeliger and Neumann were the only ones to see it. We may expect that anyone who reflects seriously on the gravitational properties of an infinite matter distribution would run into one or other form of the problem. And we know that this happened in at least two cases. As we shall see below, forms of the problem were hit upon by both Richard Bentley (who put them to Newton in their famous correspondence) and Kelvin in the context of his ether theory. For different reasons, neither saw in the problem a serious challenge for Newton's theory of gravitation. Presumably many more researchers found and dismissed the result in similar ways—but they remain unknown to us since they did not publish or, if they did, in such an obscure way that it remains hidden to us now. The Neumann of the 1870s would surely fall into this class were it not for his own efforts of 1896 to draw attention to his work. What Neumann seems not have had is a sense of the importance of his result in the 1870s—or surely he would then have drawn more attention to it. It was Seeliger who had the courage to insist on the importance of the problem. Here Seeliger seems to be alone.[17]

4. **Kelvin finds the flux argument**

Lord Kelvin (William Thomson) also hit upon the cosmological problems of Newtonian gravitation theory independently, it seems, of Seeliger and Neumann—or at least he acknowledges no debt to them. Moreover we can see in the development of his work a line of thought that would take him directly to the problem. In the fall of 1884, Kelvin delivered twenty lectures at Johns Hopkins University on wave theory and molecular dynamics. In the original lectures (recorded stenographically now reproduced in Kargon and Achinstein 1987), in Lecture XVI (p. 162), Kelvin addressed the question of whether the luminiferous ether was imponderable, that is, has no weight.

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[16] Charlier (1908: 3) wrote "It is Seeliger (Astr. Nachr. No. 3273, 1895) who first drew attention [to the cosmological difficulties of Newton's law]." ("Es ist Seeliger, der zuerst hierauf aufmerksam gemacht hat (Astr. Nachr. No. 3273, 1895) . . . ") Einstein (1954: chap. XXX) wrote of a "fundamental difficulty . . . , which, to the best of my knowledge, was first discussed in detail by the astronomer Seeliger." ("eine . . . prinzipielle Schwierigkeit . . . , welche meines Wissens zuerst von dem Astronomen Seeliger ausführlich diskutiert wurde.") See also Föppl 1897: 93, Arrhenius 1909: 223, Charlier 1922: 3, Selety 1922: 282, Findlay-Freundlich 1951: 5.

[17] This is one of the outcomes of Jaki (1979) who surveyed the awareness of the problem from the time of Newton to Seeliger. Many stood on the problem's threshold in one form or another. None gave it the uncompromising formulation of Seeliger.

> We are accustomed to call [the ether] imponderable. How do we know it is imponderable? If we had never deal with air except by our senses, air would be imponderable to us. But we can show that the weight of a column of air is sufficient to cause a difference of pressure on the two sides of a glass receiver. We have not the slightest reason to believe the luminiferous aether to be imponderable; it is just as likely to be attracted to the sun as air is. I do not like to make too many statements of that kind. At all events, the onus of proof rests with those who assert that it is imponderable. I think we shall have to modify our ideas of what gravitation is if we have a mass spreading through space with mutual gravitations between its parts without being attracted by other bodies.

This image of the ether as a mass with gravitational attraction between its parts and spreading through infinite space takes Kelvin directly to the cosmological problem. For he need only ask how strong might these gravitational forces be to arrive directly at the problem. At that was just the sort of question Kelvin would ask. His ether was no mysterious medium beyond normal physical considerations. The paragraph immediately following his pronouncement that the ether has weight contains a computation of the pressure exerted by a column of ether of infinite height on the surface of the sun.

Kelvin edited his Baltimore Lectures for subsequent publication. The edited Lecture XVI appeared in *Philosophical Magazine* as Kelvin 1901 and then in the final edition, Kelvin 1904. From parenthetical inserts we know that Kelvin had completely changed his mind by 1899 on the question of whether the ether is ponderable. One insert after the sentence "We have not the slightest reason ... " read (1901: 166; 1904: 266)

> Nov. 17, 1899. I now see that we have the strongest possible reason to believe that the aether is imponderable.

An insert with the same date at the end of the paragraph gave Kelvin's reasoning. Gravitational attraction between parts of the infinite ether would lead to infinitely great pressures in the ether, so that it would collapse unless capable of infinite resistance. His argument for these infinite pressures ran as follows:

> Suppose that aether is given uniformly spread through space to infinite distances in all directions. Any large enough spherical portion of it, if held with its surface absolutely fixed, would by the mutual gravitation of its parts become heterogeneous; and this tendency could certainly not be counteracted by doing away with the supposed rigidity of its boundary and by the attraction of aether extending to infinity outside it. The pressure at the center of a spherical portion of homogeneous gravitational matter is proportional to the square of the radius, and therefore, by taking the globe large enough, may be made as large as we please, whatever be the density. In fact, if there were mutual gravitation between its parts, homogeneous aether extending through all space would be essentially unstable, unless infinitely resistant against compressing or dilating forces.

Kelvin's argument depended on a result—that the gravitationally induced pressure at the center of a sphere of matter grew in proportion to the radius. This result in turn depended on Kelvin's version of Seeliger's flux argument, which Kelvin laid

out several paragraphs later (1901: 168–169; 1904: 267–268).[18]

> Let V be any volume of space bounded by a closed surface, S, outside of which and within which there are ponderable bodies; M the sum of the masses of all these bodies within S; and ρ the mean density of the whole matter in the volume V. We have
>
> $$M = \rho V.$$
>
> Let Q denote the mean value of the normal component of the gravitational force at all points of S. We have
>
> $$QS = 4\pi M = 4\rho V \qquad [(4)]$$
>
> by a general theorem discovered by Green seventy-three years ago regarding force at a surface of any shape, due to matter (gravitational, or ideal electric, or ideal magnetic) acting according to the Newtonian law of the inverse square law of the distance. ... If the surface is wholely convex, the normal component force must be everywhere inwards.
>
> Let now the surface be spherical of radius r. We have
>
> $$S = 4\pi r^2; \quad V = (4/3)\pi r^3; \quad V = (1/3)rS$$
>
> Hence, for a spherical surface, [(4)] gives
>
> $$Q = (4/3)\pi r\rho = M/r^2 \qquad [(5)]$$
>
> This shows that the average normal component force over the surface S is infinitely great, if ρ is finite and r infinitely great, which suffices to prove [the earlier claim of infinite force on bodies in a universe filled with a non-zero density of ponderable matter].

5. Einstein's assault on Newtonian cosmology

The flaws of the old regime are never clearer than after it has fallen and a new power has taken its place. These flaws become all the more incontestable when that new power undertakes to explain just how debased was its predecessor. And the explanation is often so oversimplified that it fails closer scrutiny. Such was the fate of Newtonian cosmology at the hands of Einstein in 1917 when he reported his efforts to apply general relativity, his new theory of gravitation, to cosmology. And we shall see below how Einstein dangerously oversimplified in his explanation. Where Seeliger had seen an arcane technical complication that required a modest technical solution, Einstein saw a symptom of a deeper and fatal ailment. In introducing Newtonian cosmology in his celebrated 1917 "Cosmological Considerations on the General Theory of Relativity," he observed that a uniform matter distribution extending to spatial infinity is incompatible with Newtonian gravitation theory as it is usually applied:

[18] The result Kelvin needs is that the gravitational force per unit mass at radial position r in a sphere of matter density ρ is $(4/3)G\pi\rho r$, Kelvin's result (5). If we now assume that this force is counterbalanced by an isotropic pressure $P(r)$, we derive its dependence on r as follows. Consider a small volume of radial thickness Δr and unit area normal to the radius. It exerts a gravitational force of $(4/3)G\pi\rho r \cdot \rho\Delta r$ on the matter below it. Therefore the pressure $P(r)$ diminishes according to $dP(r)/dr = -(4/3)G\rho^2 r$, with increasing r. Integration, assuming that the pressure P drops to zero at $r = R$, gives the pressure $P(r) = (2/3)G\pi\rho^2(R^2 - r^2)$. At the center $r = 0$, the pressure is $(2/3)G\pi\rho^2 R^2$ which exhibits the dependence on the square of radius R invoked by Kelvin.

> It is well known that Newton's limiting condition of the constant limit for [gravitational potential] φ at spatial infinity leads to the view that the density of matter becomes zero at infinity. For we imagine that there may be a place in universal space round about which the gravitational field of matter, viewed on a large scale, posesses spherical symmetry. It then follows from Poisson's equation [$\nabla^2\varphi = 4\pi G\rho$] that, in order that φ may tend to a limit at infinity, the mean density ρ must decrease towards zero more rapidly that $1/r^2$ as the distance r from the center increases.[19] In this sense, therefore, the universe according to Newton is finite, although it may possess an infinitely great total mass.[20] (Einstein 1917a: 177–178)

This was the same problem Seeliger had identified, as Einstein made clear in his parallel treatment of the same issue in his popularization of relativity theory. He wrote of a "fundamental difficulty attending classical celestial mechanics, which, to the best of my knowledge, was first discussed in detail by the astronomer Seeliger." He considered the possibility of a roughly uniform matter density throughout infinite space. However:

> This view is not in harmony with the theory of Newton. The latter theory rather requires that the universe should have a kind of center in which the density of the stars is a maximum, and that as we proceed outwards from this center the group-density of the stars should diminish, until finally, at great distances, it is succeeded by an infinite region of emptiness. The stellar universe ought to be a finite island in the infinite ocean of space.[21] (Einstein 1917b: 105)

A footnote to the last sentence described the result Einstein had in mind:

> *Proof.* According to the theory of Newton, the number of 'lines of force' which come from infinity and terminate in a mass m is proportional to the mass m. If, on the average, the mass density ρ_0 is constant throughout the universe, then a sphere of volume V will enclose the average mass $\rho_0 V$. Thus the number of lines of force passing through the surface F of the sphere into its interior is proportional to $\rho_0 V$. For unit area of the surface of the sphere the number of lines of force which enters the sphere is thus proportional to $\rho_0 V/F$ or to $\rho_0 R$. Hence the intensity of the field at the surface would ultimately become infinite with increasing radius R of the sphere, which is impossible.[22]

Einstein here gives his version of Seeliger's and Kelvin's flux argument. Seeliger's equation (3) and Kelvin's (4) captures the same result as Einstein. But, where Seeliger and Kelvin used the technical machinery of Green's theorem, Einstein used the equivalent but more vivid image of lines of force.

Einstein now administered the coup de grâce. Newtonian theory allows an island of stars only for our cosmology. But not even this is satisfactory. Such an island loses radiation to infinite space. Likewise, as energy of motion is distributed statistically among the stars of the island, some, we may suspect, will acquire enough velocity to escape the island's gravitational pull. Over time, would not all the stars eventually use this mechanism of escape, so that an island universe provides no stable system of stars? To answer, Einstein, master of statistical physics, could simply call to mind Boltzmann's analysis of the statistical physics of gas molecules in a gravitational field. Its results, Einstein could see, would hold equally for a gas of molecules or a cluster of stars. The equilibrium distribution required a finite ratio of densities at the gravitational center and at infinity, so that a vanishing density at infinity entailed a vanishing density at the center. In short, Einstein could conclude that an island universe of stars would evaporate, in apparent contradiction with the static character he presumed (notoriously in error) for the stars on the largest scale.

How was Einstein's analysis oversimplified? We shall soon see that the choice he sought to force between an evaporating island universe and an ill-behaved infinite matter distribution was a false dilemma.

Seeliger, Neumann and Einstein posed a problem that had to be solved. In the remaining Sections of the paper, I will review the various escapes entertained in the decades following, prior to 1930. We shall see that, at one time or another, virtually every facet of every assumption in the cosmological and gravitational commitments listed above were held up for scrutiny and it was urged that the rejection of each provided the escape from the problem.

6. The no-solution solution

The simplest of all responses to the problem was just to deny that there was a problem. Somehow the bad behavior of gravitational force in Newtonian cosmology was an illusion that would be dispelled by closer thought. Symmetry considerations, it would seem, must override all else: the only force distribution that is compatible with the homogeneity and isotropy of the Newtonian cosmology is a vanishing force. The problem Seeliger identified is at best a mathematical oddity that deserves little attention in the world of physics. This symmetry based

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[19] Einstein's footnote here reads: "ρ is the density of matter, calculated for a region which is large as compared with the distance between neighboring fixed stars, but small in comparison with the dimensions of the whole stellar system." ("ρ ist die mittlere Dichte der Materie, gebildet für einen Raum, der groß ist gegenüber der Distanz benachbarter Fixsterne, aber klein gegenüber den Abmessungen des ganzen Sternsystems.")

[20] "Es ist wohlbekannt, daß die Newtonsche Grenzbedingung des konstanten Limes für φ im räumlich Unendlichen zu der Auffassung hinführt, daß die Dichte der Materie im Unendlichen zu null wird. Wir denken uns nämlich, es lasse sich ein Ort im Weltraum finden, um den herum das Gravitationsfeld der Materie, im großen betrachtet, Kugelsymmetrie besitzt (Mittelpunkt). Dann folgt aus der Poissonschen Gleichung, daß die mittlere Dichte ρ rascher als $1/r^2$ mit wachsender Entfernung r vom Mittelpunkt zu null herabsinken muß, damit φ im Unendlichen einem Limes zustrebe*. In diesem Sinne ist also die Welt nach Newton endlich, wenn sie auch unendlich große Gesamtmaße besitzen kann."

[21] "haftet der klassischen Himmelsmechanik ... eine ... prinzipielle Schwierigkeit an, welche meines Wissens zuerst von dem Astronomen Seeliger ausführlich diskutiert wurde. ... Diese Auffassung ist mit der Newtonschen Theorie unvereinbar. Letztere verlangt vielmehr, daß die Welt eine Art Mitte habe, in welcher die Dichte der Sterne eine maximale ist, und daß die Sterndichte von dieser Mitte nach außen abnehme, um weit außen einer unendlichen Leere Platz zu machen. Die Sternwelt müsste eine endliche Insel im unendlichen Ozean des Raumes bilden."

[22] "Begründung. Nach der Newtonschen Theorie enden in einer Masse m eine Anzahl 'Kraftlinien,' welche aus den Unendlichen kommen, und deren Zahl der Masse m proportional sind. Ist die Dichte ρ_0 der Masse in der Welt im Mittel konstant, so umschließt eine Kugel vom Volumen V im Durchschnitt die Masse $\rho_0 V$. Die Zahl der durch die Oberfläche F ins Innere der Kugel eintretenden Kraftlinien ist also proportional $\rho_0 V$. Durch die Oberflächeneinheit der Kugel treten also Kraftlinien ein, deren Zahl $\rho_0 V/F$ oder $\rho_0 R$ proportional ist. Die Feldstärke an der Oberfläche würde also mit wachsendem Kugelradius R ins Unendliche wachsen, was unmöglich ist."

'no-solution solution' appears very infrequently in published discussions. This infrequency of publication, however, cannot be used to establish that the view is unpopular. For if one holds to this escape, one is less likely to seek publication; infrequency of publication is compatible with both a popularity and lack of popularity of the no-solution solution.

At worst, this no-solution solution is simply a blunder. As long as one holds that the gravitational force on a test body is the sum of forces exerted by all other masses, then the indeterminacy of the sum is a serious problem. It does not evaporate just because one happens to like one of the possible values over all others; one cannot wish the others away. At best, the no-solution solution is an unfulfilled promise. Since symmetry considerations do dictate one particular value for the sum, it would seem that there must be some fallacy that allows the sum to take other values. The no-solution solution, in effect, directs one to find the fallacy. However the solution can hardly be satisfactory without clearer indication of where the fallacy lies.

With the infallible wisdom of hindsight, this symmetry driven solution looks even less satisfactory. For in 1934, in the hands of Milne (1934) and Milne and McCrea (1934), Newtonian cosmology was reborn as a cosmology that mimics the expanding universe cosmologies of general relativity. This neo-Newtonian cosmology is predicated on the assumption that the gravitational force field in a homogeneous, infinite matter distribution is *not* homogeneous after all. Its inhomogeneity turns out to be what gives the cosmology is interesting properties.

6.1. Newton stumbles

Isaac Newton himself is the best known proponent of the no-solution solution. In 1692, Newton entered into a correspondence with the theologian, Richard Bentley. The latter had undertaken to inaugurate the Boyle Lectures. These would be a series of eight lecture-sermons, defending Christian religion and refuting atheism. Bentley wrote to Newton for assistance in determining how much comfort he might find in Newton's work.[23] Bentley's queries turned to the infinitude of the universe. In response, Newton gave us a portrait of how he envisaged the accommodation of an infinite distribution of masses in his system. His second letter began by considering the gravitational collapse of matter initially scattered through a finite portion of space. In such collapse, he agreed with Bentley, that it is enormously unlikely to suppose that there would be one central mass so perfectly placed that it maintained equal forces of attraction on all sides and remain at rest "a supposition fully as hard as to make the sharpest needle stand upright on its point upon a looking-glass."[24] Newton then turned to describe a cosmology with an infinite, homogeneous matter distribution. Such a distribution is possible, he asserted, but it would be (in modern language) pseudostable:

> And much harder it is to suppose all the particles in an infinite space should be so accurately poised one among another, as to stand still in perfect

[23] See Koyré 1965: chap. 4; 1957: chap. 7.

[24] These letters are reprinted as Bentley 1756.

> equilibrium. For I reckon this as hard as to make, not one needle only, but an infinite number of them (so many as there are particles in an infinite space) stand accurately poised upon their points. Yet I grant it possible, at least by a divine power; and if they were once to be placed, I agree with you that they would continue in that posture without motion for ever, unless put into new motion by the same power. When, therefore, I said that matter evenly spread through all space would convene by its gravity into one or more great masses, I understood it of matter not resting in an accurate poise. (Bentley 1756: 208)

While denial of such a static, pseudostable cosmology would become the basis of the neo-Newtonian cosmology of Milne and McCrea, Newton here described the cosmology expected by everyone to arise from Newtonian theory through to the 1920s—including Seeliger and Einstein.[25] Newton then turned directly to Bentley's formulation of the problem Seeliger later identified.

> But you argue, in the next paragraph of your letter, that every particle of matter in an infinite space has an infinite quantity of matter on all sides, and, by consequence, an infinite attraction every way, and therefore must rest in equilibrio, because all infinites are equal. Yet you suspect a paralogism in this argument; and I conceive the paralogism lies in the position, that all infinites are equal. The generality of mankind consider infinites no other ways than indefinitely; and in this sense they say all infinites are equal; though they would speak more truly if they should say, they are neither equal nor unequal, nor have any certain difference or proportion one to another. In this sense, therefore, no conclusions can be drawn from them about the equality, proportions, or differences of things; and they that attempt to do it usually fall into paralogisms. So, when men argue against the infinite divisibility of magnitude, by saying, that if an inch may be divided into an infinite number of parts, the sum of those parts will be an inch; and if a foot may be divided into an infinite number of parts, the sum of those parts must be a foot; and therefore, since all infinites are equal, those sums must be equal, that is, an inch equal to a foot.
>
> The falseness of the conclusion shews an error in the premises; and the error lies in the position, that all infinities are equal. (Bentley 1756: 208–209)

The difficulty Bentley imagined is instantiated by the argument for the non-convergence of gravitational force given in Section 1 above. The shells to the left and right of the mass at O exert an infinite force to the left and again to the

[25] However we have Newton's admission later, in the fourth letter, that he had not thought much about this cosmology:

> The hypothesis of deriving the frame of the world by mechanical principles from matter evenly spread through the heavens, being inconsistent with my system, I had considered it very little before your letters put me upon it ... (Bentley 1756: 215)

Here he informs Bentley that he had not devoted much attention to the possibility of our planetary system arising through gravitational collapse from a homogeneous matter distribution. In his *Principia*, he had already given a slightly more robust explanation of the stability of the fixed stars than pseudostability:

> And lest the system of the fixed stars should, by their gravity, fall on each other, he [God] hath placed those systems at immense distances from one another. (Newton 1687: 544)

right. Presumably Bentley wished to conclude that these two forces are equal so that the mass at O remains in equilibrium—but he was loath to do so, fearing that the comparison of competing infinite forces cannot be made without fallacy ("paralogism"). Newton affirmed Bentley's worry. The "generality of mankind" are unable to compare infinites without disastrous consequences. To make his point, he recalled a classic paradox of measure, which, Newton urges, depends on the incorrect assumption that all infinites are equal.

While Newton could not agree with the argument that the two infinite forces balance, he did wish to retain the conclusion that the mass they act on remains in equilibrium. To assure Bentley of this conclusion, he indicated that there are consistent ways of comparing infinites:

> There is, therefore, another way of considering infinites used by mathematicians, and that is, under certain definite restrictions and limitations, whereby infinites are determined to have certain differences or proportions to one another. Thus Dr. Wallis considers them in his *Arithmetica Infinitorum*, where, by the various proportions of infinite sums, he gathers the various proportions of infinite magnitudes: which way of arguing is generally allowed by mathematicians, and yet would not be good were all infinities equal. According to the same way of considering infinites, a mathematician would tell you, that though there be an infinite number of infinite little parts in an inch, yet there is twelve times that number of such parts in a foot; that is, the infinite number of those parts in a foot is not equal to, but twelve times bigger than the infinite number of them in an inch. (Bentley 1756: 209)

Newton recalled the work of Wallis' *Arithmetica Infinitorum* of 1655. In order to solve problems of quadrature, Wallis needed to sum infinite series. For example, to find the area under a cubic, Wallis needed to employ the infinite sum $0^3 + 1^3 + 2^3 + 3^3 + \ldots$. While this sum is infinite, Wallis noticed that the ratio of this sum to other infinite sums was well behaved and finite.[26] Thus he found

$$\frac{0^3+1^3}{1^3+1^3} = \frac{1}{4} + \frac{1}{4} \qquad \frac{0^3+1^3+2^3+3^3}{3^3+3^3+3^3+3^3} = \frac{1}{4} + \frac{1}{12}$$

$$\frac{0^3+1^3+2^3}{2^3+2^3+2^3} = \frac{1}{4} + \frac{1}{8} \qquad \frac{0^3+1^3+2^3+3^3+4^3}{4^3+4^3+4^3+4^3+4^3} = \frac{1}{4} + \frac{1}{16}$$

etc. As the number of terms grew without limit, the ratio of the two series approaches 1/4. This example illustrates Newton's claim that infinites can be compared (by mathematicians!) and were compared by Wallis and that they can come out to be unequal. The infinite sums of the numerator and denominator prove to have the ratio of 1 : 4—the infinite numerator is only one fourth the size of the infinite denominator. And this is the whole result, for, in Wallis' hands, it becomes (in later notation) $\int_0^1 x^3\,dx = 1/4$.

So far Newton's analysis of Bentley's paralogism is impeccable. He then fell into error. Having recalled for us that there are perfectly good methods of comparing

[26] This example is given in the fragment from Wallis' *Arithmetica Infinitorum* in Struik 1986: 244–245.

infinites by means of limits, Newton seemed not to have applied them himself to the problem at hand. For, had he done so, he would surely have noticed that there was no determinate way of balancing the infinites. In the example of Section 1, he would need to find a value for the non-convergent series. Instead, apparently, he *presumed* the result: the net force on a body in an infinite, homogeneous matter distribution is zero. He then proceeded to consider how the equilibrium of the body might be disturbed by the addition of more masses:

> And so a mathematician will tell you, that if a body stood in equilibrio between any two equal and contrary attracting infinite forces, and if to either of these forces you add any new finite attractive force, that new force, how little soever, will destroy their equilibrium, and put the body into the same motion into which it would put it were those two contrary equal forces but finite, or even none at all: so that in this case the two equal infinities, by the addition of a finite to either of them, become unequal in our ways of reckoning; and after these ways we must reckon, if from the considerations of infinites we would always draw true conclusions. (Bentley 1756: 209)

It is hard to understand how Newton could make such a mistake. His mathematical and geometric powers are legendary. Perhaps Newton was so sure of his incorrect result from the symmetry considerations that he did not deem it worthwhile the few moment's reflection needed to see through to a final result.[27]

6.2 ... Arrhenius too

Arrhenius 1909 is a gentle survey of the problem of the infinity of the universe with some effort to shield the reader from technicalities. In addition to extensive discussion of Charlier's hierarchic universe (see Section 9 below), Arrhenius directly addressed the problem for an infinite universe which had been pointed out by Seeliger and which played a role in Charlier's postulation of the hierarchical universe. Arrhenius was unable to see that Seeliger had identified a real problem. He could not see any difficulty in the infinite gravitational potential Seeliger foresaw:

> Why may the potential [φ] not become infinite? The answer is: since then the speed of a star coming in 'from the outside' would be infinite at the point in question, and we observe no excessively high speeds among the stars. ... But if one assumes with the ancient philosophers a roughly infinite distribution of stars, then there is no 'outside' in relation to the world of stars and there exists no danger of infinite speeds.[28] (Arrhenius 1909: 224–225)

He then gave a brief synopsis of Seeliger's discussion of the indeterminacy of gravitational force in Newtonian cosmology, including the derivation of (1b′) by

[27] Newton could expect no saving correction from Bentley, who, presumably, falls into the mathematically ignorant "generality of mankind." Certainly, in his Boyle Lectures, Bentley (1756: 171) was quite happy to affirm for an infinite matter distribution that "An infinite attraction on all sides of all matter is just equal to no attraction at all. ... "

[28] "Warum darf das Potential [φ] nicht unendlich werden? Die Antwort ist: weil dann die Geschwindigkeit eines 'von Außen' gekommenen Sterns an dem betreffenden Punkt unendlich wäre, und wir beobachten keine übermäßig hohe geschindigkeiten bei den Sternen. ... Wenn man aber mit den alten Philosophen eine ungefähr gleichmäßige Verteilung der Sterne im unendlichen Raum annimt, so giebt es kein 'Außen' in Bezug auf die Sternenwelt und die Gefahr der unendlichen Geschwindigkeiten existiert nicht."

means of a sphere allowed to grow infinitely large. Arrhenius then laid out a clear statement of the 'no-solution solution,' with its foundation in a symmetry argument:

> Accordingly, it is very much understandable that Seeliger's argumentation is frequently construed as conflicting with the infinity of the world. This, however, is not true. The difficulty lies in that the attraction of a body surrounded by infinitely many bodies is undetermined according to Seeliger's way of calculation and can take on all possible values. *This, however, only proves that one cannot carry out the calculation by this method.* Also, how can one think of an infinitely great sphere, containing the stars, as surrounded by an infinitely great empty space? If a body is in an infinite space, in which other bodies are roughly uniformly distributed, then, if one ignores nearby bodies, its attraction is equally great in all directions, as is easy to see for reasons of symmetry. Consequently, these attractions cancel one another; and the body behaves just as if it were under the influence of nearby bodies or collections of bodies and the distant bodies were not present at all; therefore [it would be] exactly as if an absorption of gravitational force took place.
>
> So there really is no valid reason for why the world would not be sown roughly uniformly with stars.[29] (Arrhenius 1909: 226, my emphasis)

6.3 Bach's no-solution non-solution

Bach's (1918) response to the problem can be conveniently mentioned here although it is not properly a no-solution solution. Bach believed that the problem could be escaped by allowing for random non-uniformities in the cosmic matter distribution that was, in some sense, uniform overall. He computed the gravitational force on a test body due to stars surrounding it, all stars presumed to be of the same mass. The nearest star, the second nearest star, ... the nth nearest star, ... were assumed to lie at unit distance, distance $\sqrt[3]{2}$, ... distance $\sqrt[3]{n}$, ... respectively from the test body. This prescription ensured that there would be exactly n stars enclosed in a sphere of radius n centered on the test body, for all n. Now each star lies on the surface of a sphere of the radius specified, centered on the test mass. Bach introduced a random element by assuming that each star might be found with equal probability on any location of its sphere's surface. He then proceeded to compute the sequence of scalar forces on the test body due to the nearest star; the nearest and the second nearest; the nearest, second and third nearest; etc. This calculation was carried to the limit of infinitely many stars. Because of the random

[29] "Es is demnach sehr wohl verständlich, daß die Seeliger'sche Beweisfuhrung häufig als gegen die Unendlichkeit der Welt streitend angeführt wird. Dies trifft aber nicht zu. Die Schwierigkeit sollte darin bestehen, daß die Anziehung eines Körpers in einer Umgebung von unendlich vielen Körpern nach der Seeliger'schen Berechnungweise unbestimmt wird und alle mögliche Werte annehmen kann. Dies beweist aber nur, daß man nach dieser Methode nicht die Berechnung ausführen kann. Wie kann man sich auch eine unendliche große Kugel, die Sterne enthält, von einem unendlich großen leeren Raum umgeben denken? Wenn ein Körper in einem unendlichen Raum sich befindet, wo andere Körper ungefahr gleichmäßig verteilt sind, so ist seine Anziehung, wenn man von den nächstliegenden Körpern absieht, nach allen Richtungen gleich groß, wie aus Symmetriegründen leicht ersichtlich. Diese Anziehungen heben einander infolgedessen auf, und der Körper verhält sich ganz so, als ob er nur unter dem Einfluß der nächstliegenden Körper oder Körpersammlungen stände und die entfernteren Körper gar nicht vorhanden wären, also genau wie wenn eine Absorption der Schwerkraft stattfände.
Es giebt also tatsächlich kein triftiger Grund, weshalb das Weltall nicht ungefähr gleichförmig mit Sternen besäet wäre."

element introduced, Bach could compute only a probability distribution $p(x)$ for the scalar force x and, because of the complication of the calculation, his result was arrived at by numerical methods. Bach found the limit probability distribution p massed near $x = 2$, where unit force is set as the force due to the nearest star, with the probability of much larger forces very small. This dominance of finite forces, Bach believed, resolved the problem.

Bach's analysis, however, fails to resolve the problem. It is based on the simple misunderstanding that the problem resides in the supposedly infinite values of gravitational forces in a uniform Newtonian cosmology. However the difficulty is not the infinity of force but its lack of convergence. Any force—finite or infinite—can be recovered by choosing a suitable approach to the limit in the integral that represents the total force. All that Bach has shown is that some approaches in the cases he considers can yield finite results. This does not preclude others yielding quite different results for the same mass distribution, in which case the problem remains.[30]

7. Escape by modifying Newton's inverse square law of gravitation

While Newton and Arrhenius thought Newtonian cosmology in no need of repair, Seeliger was convinced otherwise. Arrhenius' (1909) discussion came to his notice during the printing of his Seeliger 1909, to which he added a dismissive appraisal of Arrhenius' paper in a footnote (p. 7). "I content myself with the affirmation of the fact that Herr Arrhenius has totally misunderstood my exposition."[31] Seeliger, like many others, thought the cosmological problem he had laid out called for a modification of Newton's inverse square law of gravitation.

7.1. Seeliger's modification

Seeliger (1895a: 132–133) saw the problem posed by his work as a dichotomy: either Newton's inverse square law is not the exact expression for gravitational force; or "the total matter of the universe must be finite or, more exactly expressed, infinitely great parts of space may not be filled with masses of finite density."[32] Seeliger was inclined to accept the first horn of the dilemma.[33] He saw nothing sacred in Newton's law. It was merely "a purely empirical formula and assuming its exactness would be a new hypothesis supported by nothing."[34] Thus he pro-

[30] I am grateful to Michel Janssen for drawing this paper to my attention and for the information that "Rudolf Bach" is actually a pseudonym of Rudolf Förster, according to Förster's claim in a letter of February 16, 1918, to Einstein, in which the paper Bach 1918 is foreshadowed.

[31] "Ich benüge mich mit der Konstatierung der Thatsache, daß Herr Arrhenius meine Darlegungen total mißverstanden hat."

[32] "muß die Gesamtmaterie des Weltalls endlich sein oder genauer ausgedrückt, es dürfen nicht endlich große Theile des Raumes mit Masse von endlicher Dichtigkeit erfüllt sein."

[33] Seeliger (1896: 384) explained his preference for the first since "one is saved from awkward metaphysical considerations on the finitude or infinitude of matter." ("man hierdurch mißlichen metaphysischen Betrachtungen über die Endlichkeit oder Unendlichkeit der Materie entrückt ist.")

[34] "Nun ist weiter das Newton'sche Gesetz eine rein empirische formel, deren Genauigkeit als eine absolute anzunehmen eine neue und durch nichts gestützte Hypothese wäre."

posed the addition of extra terms to Newton's law that would solve the difficulty while remaining compatible with observations of planetary motions. The difficulty was that one could find infinitely many such admissible modifications. They were merely constrained by the requirement that they lead to converging integrals corresponding to (1). The indeterminacy of these three integrals depended on the fact that the three integrals

$$\int_{R_0}^{R_1} \rho\, r\, dr, \qquad \int_{R_0}^{R_1} \rho\, dr, \qquad \int_{R_0}^{R_1} \rho \frac{1}{r}\, dr \tag{6}$$

all diverged to infinity in the limit of infinitely large R_1 (with ρ a constant). A satisfactory modification of Newton's law would be one that led to finite values for these three integrals. Seeliger proposed a particular modification—but, he admitted, more as an example of what such modification may be than as an attempt for a deeper physical viewpoint. Since the notion that gravitation is an unmediated action at a distance had fallen from favor, he raised the possibility of a very slight absorption of gravitational force in space. On the analogy with agents such as light, this led to an attenuation factor of the form $\exp(-\lambda r)$ for gravitation passing through a distance r, for λ some positive constant.[35]

Seeliger chose to apply this attenuation factor to the expression for gravitational force F between two bodies of mass m and m', which became

$$F = -Gmm' \frac{e^{-\lambda r}}{r^2}. \tag{7}$$

This modification immediately solved the problem since the three integrals corresponding to (6) became

$$\int_{R_0}^{R_1} \rho\, e^{-\lambda r} r\, dr, \qquad \int_{R_0}^{R_1} \rho\, e^{-\lambda r}\, dr, \qquad \int_{R_0}^{R_1} \rho \frac{e^{-\lambda r}}{r}\, dr,$$

and they were finite in the limit of infinite R_1.

Seeliger was also interested in estimating the value of λ. The simplest choice was to make λ so small that no observation of gravitational phenomena in astronomy could distinguish it from zero—any non-zero value at all, no matter how small, resolved the cosmological problem. But Seeliger also raised the possibility that one could place a value on λ by assuming that it was responsible for known anomalies in the motion of the planets. The anomalous (and soon to be famous) advance of the perihelion of Mercury of about 40″ per century could be accounted for, it turned out, by setting λ to the modest value 0.000 000 38. However he was concerned that this value of λ led to slight but definite perihelion motion in the remaining planets. In the first of his 1895 papers, he could proceed no further in comparing these motions with known motions. He had to await the results of Newcomb's

[35] Seeliger clearly intended that the agent lose intensity λ per unit distance by absorption, so that intensity $I(r)$ at distance r satisfies $dI/dr = -\lambda I$, which solves to yield $I(r) = I(0)\exp(-\lambda r)$.

work on planetary motion then in progress. In any case, Seeliger did not take very seriously the possibility that he had found an explanation for the anomalous motion of Mercury. The astronomical anomalies were only superficially connected with his cosmological considerations. If it works, he remarked, it does so by chance, only formally and without deeper foundation.

Seeliger must have been relieved that he took this modest view. Shortly, in Seeliger 1896: 387–389, with Newcomb's (1895) work in hand, he reported the failure of his attempt to read off a value of λ from astronomical anomalies. If its value were set from the motion of Mercury, then it predicted perihelion motions for Venus, Earth and Mars well outside the actual range Newcomb found compatible with observation. A similar fate befell the attempt to relate Neumann's proposed modification of Newton's law (see Section 7.2 below). However a proposal by Hall fared much better. He had proposed that Newton's inverse square law be replaced by a law with a distance dependence of

$$\frac{1}{r^{2+\alpha}} \tag{8}$$

with $\alpha = .00000016$, and this modification turned out to give perihelion motions for these same planets within Newcomb's error bounds. Seeliger was unmoved by Hall's success. He felt it resulted from chance. In any case, in his (1895a: 136), he had already pointed out that Hall's modification fails to resolve the cosmological problem. The three integrals corresponding to (6) are

$$\int_{R_0}^{R_1} \rho\, r^{1-\alpha}\, dr, \qquad \int_{R_0}^{R_1} \rho \frac{dr}{r^{\alpha}}, \qquad \int_{R_0}^{R_1} \rho \frac{dr}{r^{1+\alpha}},$$

and the first two still diverge for infinite R_1. This result, incidentally, shows that the cosmological problems are somewhat robust. They do not depend entirely on taking exactly Newton's law for gravitational attraction. Some obvious modifications still yield the problem. The second integral still diverges for any $\alpha \leq 1$ and the first for any $\alpha \leq 2$.

7.2. Neumann's modification

Seeliger's modification (7) was offered as one of many. It was intended to show that there exists at least *some* modification of Newton's law that allows an escape from the cosmological problem, without necessarily finding the one correct such modification. Finding this one correct modification was not possible solely on the basis of the requirement that it escape the cosmological problem, for very many modifications succeed. Some further condition would be needed to pick among them.

Neumann's 1896 modification of Newton's law was introduced in quite another spirit. The purpose of the work was to find alternatives to the Newtonian potential function $\varphi(r) = -1/r$ of electrostatics, with gravitation only an incidental interest. Neumann had found a quite precise condition to restrict his search for alternative forms. The Newtonian form was distinctive in so far as it allowed a system of charges to come to equilibrium when confined to electrical conductors. It turned out that most other forms for $\varphi(r)$ were incompatible with such equilibrium. Neumann showed (p. 16), for example, that charges mediated by the potential $\varphi(r) = -(1 - \exp(-\alpha r))/r$, for α some constant, if placed in a metal sphere "can never come to rest, but must remain in ceaseless motion."[36] Chapters 3 and 4 contained his principal theorem: if a system of electric charges can come to equilibrium in a conductor, then the charges must interact via a potential of the form

$$\varphi(r) = -\frac{Ae^{-\alpha r}}{r} - \frac{Be^{-\beta r}}{r} - \frac{Ce^{-\gamma r}}{r} - \ldots, \tag{9}$$

with the Newtonian $\varphi(r) = -1/r$ a special case, and (conversely), if the constants $\alpha, \beta, \gamma, \ldots$ are all positive and the constants $A, B, C, \ldots$ all have the same sign, then any system of charges in a conductor mediated by a potential (9) will admit equilibrium.

All these considerations and Neumann's extensive calculations proceeded essentially independently of the cosmological problem of gravitation. It made a brief reappearance on p. 122, where Neumann showed that the problem was resolved by a single term special case of his general potential

$$\varphi(r) = -\frac{Ae^{-\alpha r}}{r}, \tag{9$'$}$$

for any positive α, no matter how small, so that the escape still succeeds if the difference of the law from Newton's original is, as he put it, "vanishingly small" [*verschwindend kleine*]. This followed from a result on the previous page (p. 121). There he showed that the potential φ due to form (9′) inside a homogeneous sphere of charge density ϵ and radius R at position $\rho_1 < R$ was given by

$$\begin{aligned} \varphi &= -4\pi R^2\epsilon \cdot A\left(\frac{1}{\alpha^2R^2} - \frac{(1+\alpha R)e^{-\alpha R}}{\alpha^2R^2}\,\frac{e^{\alpha\rho_1} - e^{-\alpha\rho_1}}{2\alpha\rho_1}\right) \\ &\longrightarrow -\frac{4\pi\epsilon \cdot A}{\alpha^2} \quad \text{as} \quad R \longrightarrow \infty. \end{aligned} \tag{10}$$

Thus, if the matter filling a homogenous, infinite universe were conceived as an infinite sphere, then the exponential potential law (9′) predicted a constant gravitational potential and no gravitational force on a test body.

Our impression that this gravitational problem was of the least concern to Neumann is reinforced by the footnote he embedded in its discussion:

> Incidentally, that objection is only a *hypothetical* one. For it is based on the hypothetical representation that all of universal space, into the infinite, is filled with stars in roughly *uniform* distribution.[37] (Neumann 1896: 122; emphasis in original)

[36] "niemals zur Ruhe kommen können, sondern in unaufhörlicher Bewegung sich befinden"

[37] "Uebrigens ist jener Einwand nur eine *hypothetischer*. Denn er basirt auf der hypothetischen Vorstellung, daß der ganze Weltraum ins Unendliche hin, in einigermaßen *gleichförmiger* Verteilung, von Sternen erfüllte sei."

He had noted earlier (p. 114) that his exponential potential (9′) led to a gravitational force between masses m_1 and m_2 at distance r of

$$F = -Gm_1m_2\,\frac{1+\alpha r}{r^2}\,e^{-\alpha r}.$$

He *conjectured*, but apparently did not think it worth the effort of calculating, that this force formula would also be able to explain the anomalous advance of Mercury's perihelion if Seeliger's force (7) could. He also introduced Seeliger's force expression (7) as to due to Laplace, mentioning Seeliger only briefly at the end of his discussion.

That Laplace had used the formula was allowed by Seeliger (1896: 386) (without mention of Neumann). In spite of his effusive welcome of Neumann's attention, Seeliger must have grown weary of Neumann's attempts to minimize Seeliger's priority.[38] Such weariness may explain Seeliger's (1896: 385–386) strained observation that Neumann's general exponential potential (9) need not resolve the gravitational problem. For in the limit of infinitely many terms, the sum becomes an integral, which can yield a potential $\varphi(r) = -A/r^{1+\alpha}$. This potential corresponds to Green's formula (8) and does not solve the gravitational problem.[39]

7.3. Einstein and the cosmological constant

Neumann had his own purpose in calling attention to the cosmological problems of Newtonian theory. He wanted to motivate consideration of quite specific, alternative forms for the potential for his work on electrostatics. Einstein (1917a), too, had his own very similar purpose in recalling the cosmological difficulties of Newtonian theory. He hinted at it when he indicated how a modification of Newton's law of gravitation (written in the field form of Poisson's equation $\nabla^2\varphi = 4\pi G\rho$) could resolve the problems.[40]

> We may ask ourselves the question whether [these difficulties] can be removed by a modification of the Newtonian theory. First of all we will indicate a method which does not in itself claim to be taken seriously; it merely serves as a foil for what is to follow. In place of Poisson's equation we write
>
> $$\nabla^2\varphi - \lambda\varphi = 4\pi G\rho \tag*{[(11)]}$$

[38] Neumann's obsession with priority includes his claim (1896: 225) that he introduced the much used word *ponderomotorisch*.

[39] The observation is, in fact, strained since, in replacing the sum (9) by an integral,

$$-\int_{\alpha_1}^{\alpha_2} g(\alpha)\exp(-\alpha r)/r\,d\alpha,$$

one has to choose a quite specific density function $g(\alpha)$ and limits α_1, α_2 to recover Green's formula—for example, $g(\alpha) = -A\ln r\, r^{-\alpha}e^{\alpha r}$ with $\alpha_2 = \alpha$ and $\alpha_1 = \infty$.

[40] For uniformity of notation I have represented Einstein's gravitational constant κ by G and his Δ by ∇^2.

> where λ denotes a universal constant. If ρ_0 be the uniform density of a distribution of mass, then
>
> $$\varphi = -\frac{4\pi G}{\lambda}\rho_0 \qquad [(12)]$$
>
> is a solution of equation [(11)]. This solution would correspond to the case in which the matter of the fixed stars was distributed uniformly through space, if the density ρ_0 is equal to the actual mean density of the matter in the universe.[41] (Einstein 1917a: 179)

Einstein's escape is delivered with his customary flair for simplicity. Seeliger and Neumann offered modifications of Newton's law, but one could only see after tedious calculation that they resolved the cosmological difficulty. In Einstein's case, one sees merely by effortless inspection that the modified law (11) admits a cosmological solution (12) with uniform matter distribution. Einstein does not relate his proposal (11) to Seeliger's or Neumann's proposals. In his popular treatment (Einstein 1954: 106), he does mention Seeliger's slight adjustment of Newton's law. However a reader would be mistaken in equating Seeliger's adjustment (7) with Einstein's proposal (11). Rather Einstein's proposal coincided with Neumann's single term proposal (9′), as comparison of (12) with Neumann's limiting case (10) suggests.[42] Neumann (1896: 81–85) had calculated the differential field equations that corresponded with his potential (9) for the cases of forms with one, two, three and higher terms. They were of successively greater complication as the number of terms increased. The equation for the one term case (12′) was

$$\nabla^2\varphi - \alpha^2\varphi = 4\pi A\epsilon,$$

which corresponds to Einstein's (11) if we set $\lambda = \alpha^2$, $\rho = \epsilon$ and $G = A$.

Einstein's deeper purpose in introducing (11) became clear as the paper unfolded. In this paper, Einstein sought to develop a relativistic model of space-time that is spatially closed and with a static matter distribution that we now know as the Einstein universe. The problem is that this model is not a solution of his gravitational field equations of general relativity, unless they are augmented by the notorious 'cosmological' term $\lambda g_{\mu\nu}$; that is, unless his original field equations

$$G_{\mu\nu} = \kappa T_{\mu\nu}$$

become

$$G_{\mu\nu} - \lambda g_{\mu\nu} = \kappa T_{\mu\nu},$$

[41] "Man kann sich die Frage vorlegen, ob sich dieselben durch ein Modifikation der Newtonschen Theorie beseitigen lassen. Wir geben hierfür zunächst einen Weg an, der an sich nicht beansprucht, ernst genommen zu werden; er dient nur dazu, das Folgende besser hervorzutreten zu lassen. An die Stelle des Poissonschen Gleichungen setzen wir ... wobei λ eine universelle Konstante bedeutet. Ist ρ_0 die (gleichmäßige) Dichte einer Massenverteilung, so ist ... eine Lösung der Gleichung [(11)]. Diese Lösung entspräche dem Falle, daß die Materie der Fixsterne gleichmäßig über den Raum verteilt wäre, wobei die Dichte ρ_0 gleich der tätsachlichen mittleren Dichte der Materie des Weltraumes sein möge."

[42] To see the equivalence, set Einstein's and Neumann's quantities into correspondence according to $G = A$, $\rho_0 = \epsilon$ and $\lambda = \alpha^2$.

where $G_{\mu\nu}$ is the Einstein tensor, $T_{\mu\nu}$ is the stress-energy tensor, $g_{\mu\nu}$ the metric tensor, κ and λ constants. But how is Einstein to give independent motivation for the addition of the cosmological term? The way had been prepared by his analysis of the problems of Newtonian cosmology. At the moment of its introduction (p. 186) Einstein could announce that the modified field equation of general relativity "is perfectly analogous to the extension of Poisson's equation given by equation [(11)]."[43] (The analogy is less than perfect—something Einstein may have found it expedient to overlook in order not to compromise his introduction of the cosmological term. As Trautman (1965: 230) pointed out, Einstein's augmented gravitational field equation reduces in Newtonian limit to a field equation other than (11): $\nabla^2\varphi + \lambda c^2 = 4\pi G\rho$.)

A small puzzle remains. Why did Einstein characterize his modification of Poisson's equation in Newtonian theory as something that " ... does not in itself claim to be taken seriously; it merely serves as a foil for what is to follow. ... " Is it not a perfectly proper resolution of a problem in Newtonian cosmology? We may entertain several explanations. Perhaps Einstein, like Seeliger, was worried that the modification is by no means uniquely determined by the condition that it resolve the cosmological problem; it is just a haphazard selection among many viable choices. Or perhaps he was not able to take such modification of Newtonian theory seriously since it still left the theory without relativistic character and therefore essentially flawed. I favor another possibility. Einstein saw the role of the modified Poisson equation as suggesting a structurally analogous modification of his gravitational field equations of general relativity by a cosmological term. But the consideration that really motivated the introduction of the cosmological term was Einstein's need for the Einstein universe to be a solution of his field equations. And this need was in turn a result of his program of ensuring Machian character for general relativity. Thus, although analogous, the deeper needs for the two modifications were different. One allowed convergence of an integral; the other brought compliance with Einstein's Machian program. So the two differed at this deeper level and Einstein's hesitation was intended to prevent confusion of his Machian interests with Seeliger's problem.

8. Escape by modifying the universality of gravitation

Seeliger, Neumann and Einstein showed that a slight adjustment of the inverse square distance dependence of gravitation force is sufficient to allow Newtonian cosmology. It was also immediately apparent that this is not the only way that Newtonian gravitation may be adjusted to readmit cosmology.

8.1. FÖPPL'S NEGATIVE MASSES

At the February 6, 1897 sitting of the Bavarian Academy of Sciences, Seeliger presented a paper by August Föppl (1897). The latter sought a way of escaping

[43] "welche der durch Gleichung [(11)] gegebenen Erweiterung der Poissonschen Gleichung vollkommen analog ist."

Seeliger's cosmological problem that, as he put it, did not involve giving up Newton's law of gravitation. His inspiration was the strong analogy between electric fields and gravitational fields. Since there were both positive and negative electric charges, there could be either attraction or repulsion between charges according to their signs. Thus, as long as the amount of positive and negative charge in the universe were equal, the "flux of force" through the surface of a sphere would converge to zero as the sphere grew infinitely large. (This result is seen most vividly using the lines of force model that Föppl did not invoke. Lines of force originating in charges of one type end in charges of the other. So the total number of lines of force emanating from a spherical portion of space is a measure of the difference between the quantities of positive and negative charge enclosed. The number of lines of force would converge to zero if the quantities of positive and negative electric charge suitably approached equality in the limit of large volumes of space.)

One way we saw above that Seeliger found of expressing the cosmological problem was as the infinite build up of field strengths on the surface of a piece of the universe as the piece grows without limit. Einstein's later version of this same formulation involved in infinite accumulation of lines of force on the surface of a spherical piece. Föppl pointed out that the existence of gravitationally negative masses would immediately resolve the cosmological problem, as long as the amounts of masses of both signs were equal in the universe. For then, just as an infinite build up of the flux of electric force did not occur, so also would there be no infinite build up of the flux of gravitational force.[44] Föppl saw this supposition of negative masses as an extension of Newton's law, not a compromise of it. However there is a sense in which it is a violation of Newton's law. Newton proposed a *universality* for gravitation: all bodies attract all others. That, Föppl proposed, should now be given up.

The bulk of Föppl's discussion is given over to more detailed consideration of the proposal. He sought to establish, for example, that gravitational masses of like sign attract and of opposite sign repel. This he did by invoking the analogy to electromagnetism. On the basis of the analogy, he proposed an expression for the energy density of the gravitational field and from it the desired result followed. He did try to address the awkward fact that we have no observational record of bodies that gravitationally repel. That, he explained, was easy to understand. Masses of opposite sign must have been expelled by repulsive forces from our vicinity.

[44] Föppl (1897: 95) made clear that he proposed just the gravitational mass—the measure of the mass as gravitational field source—to take either sign. The inertial mass—the measure of its resistance to acceleration—must remain positive, a result "Mach has already proved in detail in his *Mechanik*." ("hat schon Mach in seiner Mechanik ausführlich nachgewiesen.") We may wonder here precisely what Föppl intended. If *Mechanik* refers to Mach 1883, *Die Mechanik in ihrer Entwicklung Historisch-kritisch dargestellt*, then we would look to Mach's celebrated definition of mass in terms of acceleration induced in interacting bodies. However, far from demonstrating that negative inertial masses are impossible, he assures us (pp. 266–267) :"that there are, by our definition, only positive masses, is a point that experience teaches, and experience alone can teach." ("nach unserer Definition bloß positive Massen gibt, lehrt die Erfahrung und kann nur die Erfahrung lehren.") But Mach allows that the constancy of *vis viva* is an experience and this experience shows through a simple thought experiment the transitivity of equality of mass (p. 269). Perhaps this experience can teach us that there are no negative inertial masses through a similar thought experiment.

Perhaps because of the doubts one must have over positing entities one never expects to encounter, Föppl's concluding paragraph hedged carefully. He was far from asserting that there really are such negative gravitational masses; he merely noted that we could not now deny their possibility and, since their existence could be confirmed observationally, the possibility ought to be made known.

8.2. Kelvin's imponderable ether

Föppl's proposal of negative masses was just one way that a modification of the universality of gravitation could allow escape from the problem. Kelvin invoked a more direct modification that could be applied only to a special form of universe filling matter. He had concluded that his ether would need to sustain infinite forces if it gravitated. So he posited that it did not. The universality of Newtonian gravitation did not extend to the matter of ether:

> If we admit that ether is to some degree condensible and extensible, and believe that it extends through all space, then we must conclude that there is no mutual gravitation between its parts, and cannot believe that it is gravitationally attracted by the sun or the earth or any ponderable matter; that is to say, we must believe ether to be a substance outside the law of universal gravitation. (Kelvin 1901: 167; 1904: 266)

9. Escape by modification of the cosmological assumptions

The escapes considered so far depend on modifying one or other aspect of Newton's law of gravitation. These modifications then allow a cosmology with a uniform matter distribution. A quite different response to the problem was possible. One could leave Newton's law intact and ask just how much the assumptions of cosmological character needed to be modified in order that one may retain an interesting cosmology compatible with Newton's law of gravitation. Escapes of this character emerged rapidly.

9.1. Seeliger's dispute with Wilsing

One of the earliest responses to Seeliger's 1895 original paper was a critique by Wilsing (1895a) in a later issue of *Astronomische Nachrichten*. Seeliger (1895b) responded impatiently to Wilsing's critical remarks and they descended into a cycle of exchanges, Wilsing (1895b), Seeliger (1895c), with the journal's editor calling a halt at the last reply. One significant notion did emerge in Wilsing's opening salvo. He urged that a contradiction between a cosmological model and gravitation theory ought not to be resolved by modifying gravitation theory. Because of their highly speculative character, one ought to give up the cosmological assumptions:

> Herr Seeliger sees in the fact that, according to Newton's law, an infinitely long matter-filled cone would also exert an infinitely great attraction on a point on its axis, an objection against the general validity of the law itself; the author of a cosmogonic hypothesis can only draw the converse conclusion, that that mass distribution, purely imagined by him and inaccessible to observation, could have been possible at no time, for it cannot be explained

> by the forces abstracted from the world of experience accessible to us.[45] (Wilsing 1895a: 388)

It is hard to fault the good sense of this viewpoint. It does presuppose, however, that we retain Newtonian gravitation theory in its unmodified form, which is incompatible with a homogeneous matter distribution. Many then found this incompatibility hard to accept. Wilsing then proceeded to skirt a possibility that, after 1930, would prove to be the key to the modern resolution. Seeliger and most who considered the problem in the decades to follow, including Einstein, did not contemplate seriously how the homogeneous cosmology might develop in time, presuming that it must be static.

> But even if one now wanted to pursue the point of view of Herr Seeliger in relation to the possibility of the mass distribution imagined, then the consideration of the *temporal* development of the system itself, ignored by Herr Seeliger, would not permit a conclusion in relation to the exactness of Newton's law. For the action of the latter would only have the result of the destruction of the mass distribution thought of and its replacement by such a distribution that was compatible with it. Rather, an objection against Newton's law would arise from a mechanical stand point, if one converts the problem, with Herr Seeliger, from a dynamic to a static one by neglecting time, and adds in yet a further demand that the corresponding, imagined mass distribution should represent a state of equilibrium. This augmentation of the conditions satisfied by the imagined distribution appears especially adapted to demonstrate the inadequacy of a conclusion from merely thought up mechanical processes against the laws objectively valid in the world of experience.[46] (Wilsing 1895a: 388–389; emphasis in original)

[45] "Herr Seeliger erblickt in der Thatsache, daß nach dem Newton'schen Gesetze die einen unendlich langen Kegel erfüllende Materie auf einen Punkt in seiner Axe auch eine unendlich große Anziehung ausüben würde, einen Einwurf gegen die allgemeine Gültigkeit dieses Gesetzes selbst; der Urheber einer kosmogonischen Hypotheses kann nur den umgekehrten Schluß ziehen, daß jene bloß von ihm vorgestellte, der Beobachtung unzugängliche Massenverteilung zu keiner Zeit möglich gewesen sein könnte, da sie durch die bekannten, aus der uns zugänglichen Erscheinungswelt abstrahirten Kräfte nicht erklärt werden kann."

[46] "Aber selbst, wenn man der Anschauungsweise des Herrn Seeliger bezüglich der Möglichkeit der vorgestellten Massenvertheilung zunächst folgen wollte, so würde selbst dann die Berücksichtigung der von Herrn Seeliger außer Acht gelassenen zeitlichen Entwicklung des Systems einen Schluß bezüglich der Exactheit des Newton'schen Gesetztes nicht zulassen. Denn die Wirkung des letzteren würde dann nur die Zerstörung der gedachten Massenvertheilung zur Folge haben und ihre Ersetzung durch solche Vertheilungen, welche eben mit ihm verträglich sind. Ein Einwurf gegen das Newton'sche Gesetz würde sich dagegen vom mechanischen Standpunkt aus nur ergeben, wenn man mit Herrn Seeliger durch Vernachlässigung der Zeit das Problem aus einem dynamischen zu einem statischen machte, und noch die weitere Forderung hinzufügte, daß die betreffenden vorgestellten Massenvertheilungen Gleichgewichtslagen darstellen sollen. Dieses Vermehrung der von den vorgestellten Massenvertheilungen zu erfüllenden Bedingungen erscheint besonders geeinget, um das Unzulängliche einer Schlußfolgerung von nur gedachten mechanischen Vorgängen auf die in der Erscheinungswelt objectiv gültigen Gesetze darzuthun." This is followed immediately by remarks similar to those of Arrhenius who thought Seeliger had merely proved that one cannot use Seeliger's methods to compute gravitational force in a Newtonian cosmology:

> If Herr Seeliger further assumes, as an example of a mass distribution mentioned, the infinitely extended universe as spherically bounded and filled with mass of uniform density; and he stipulates a midpoint; and then calculates the forces at definite points to have arbitrary values; then this result proves only that the form of the mathematical investigation fails as soon as one leaves the grounding of the imaginable and applies the same to a transcendental problem. ("Wenn Herr Seeliger weiter, als Beispiel einer Massenvertheilung der erwähnten Art, das unendlich ausgedehnte Weltall kugelförmig begrenzt und mit Masse gleicher Dichtigkeit erfüllt annimmt, einen Mittelpunkt festsetzt und dann bei Berechnung der Kräfte in bestimmten Punkten zu beliebigen Werthen gelangt, so beweist dieses Ergebnis nur, daß die Formen mathematischer Untersuchung versagen, sobald man den Boden des Vorstellbaren verlässt und die selben auf ein transcendentales Problem anwendet.")

But Wilsing, unlike Arrhenius, clearly thought a homogeneous matter distribution untenable.

Had Wilsing followed his own suggestion and calculated how a homogeneous matter distribution develops in time, he would have been in a position to discover the neo-Newtonian dynamic cosmologies of Milne (1934) and Milne and McCrea (1934) out of which the modern resolution of the problem would come. However he did not and we may well wonder what sense he would have made of their dynamics prior to the discovery of the expansion of the universe. He gives no hint of the form of the distribution that dynamical evolution would provide after the initial instant and we are left to wonder how he would apply Newton's theory to a homogeneous distribution of matter at its initial instant, given the problems Seeliger had identified. Seeliger's (1895b) response set the tone by referring to Wilsing's "irrelevancies" (*Nebensächliche*) and "errors" (*Irrthümern*) and the resulting tension effectively precluded profit from the ensuing exchange.

9.2. Einstein's and Seeliger's false dilemma

Einstein's (1917a) argument was based on a dilemma for Newtonian cosmology: either matter was concentrated in an island or it was homogeneously distributed. Either case brought disaster: the island would evaporate and the homogeneous distribution would suffer Seeliger's divergent forces. Seeliger had explicitly posed essentially this same dilemma without the evaporation argument.

> Therefore it is necessary to choose between two assumptions:
> 1) the total mass of the universe is immeasurably great, then Newton's law cannot hold as a mathematically strict expression of the prevailing forces of attraction,
> 2) Newton's law is absolutely exact, then the total matter of the universe must be finite or, expressed more exactly, there cannot be infinitely great parts of space filled with mass of finite density.[47] (Seeliger 1895a: 132–133)

Seeliger's dilemma depends on two erroneous presuppositions: that an everywhere non-vanishing mass density entails that the total mass of the universe is infinite; and that Newton's law is incompatible with a universe that has any everywhere non-vanishing mass density. The latter, in particular, was not a consequence of his divergent integrals.

Kelvin seemed to have a more acute sense of what these divergences entailed. While he inferred from them that his ether did not gravitate, he could not draw a similar conclusion for ordinary matter, such as constitutes the stars, for the latter clearly does gravitate. For such matter, his gloss of the consequences of the divergences was more cumbersome than Seeliger's, but it avoided the pitfalls. The flux argument provided

[47] "Es wird deshalb nothwendiger Weise zwischen den beiden Annahmen eine Wahl zu treffen sein: 1) die Gesammtmasse des Weltalls ist unermesslich groß, dann kann das Newton'sche Gesetz nicht als mathematisch strenger Ausdruck für die herrschenden Anziehungskräfte gelten, 2) das Newton'sche Gesetz ist absolut genau, dann muß die Gesammtmaterie des Weltalls endlich sein oder genauer ausgredrückt, es dürften nicht unendlich große Theile des Raumes mit Masse von endlicher Dichtigkeit erfüllt sein." A similar statement of this dilemma is in the introduction to Seeliger 1896 (p. 373).

> decisive proof that the mean density of ponderable matter through any very large spherical volume of space is smaller, the greater the radius; and is infinitely small for an infinitely great radius. (Kelvin 1901: § 11; 1904: § 11)

We shall see below in Section 9.3 that this formulation is still not quite strong enough.[48] To avoid the divergences, one must make a further stipulation on how rapidly the mean density at a given radius diminishes as the radius becomes infinite. We do not know if Kelvin was aware of the need for this further restriction. Rather than pursuing such theoretical issues, the practical Kelvin proceeded to a practical issue: estimating just how much matter might reside in the stars in our observable neighborhood. Taking that neighborhood to be spherical, Kelvin used the forces computed by the flux argument to determine the accelerations on stars in our neighborhood. He delimited the matter density on the assumption that it must be sufficiently small so that the accumulated velocity imparted to the stars over millions of years remains within the small bounds delivered by observation and arrived at a densities he found plausible.

9.3. The Hierarchic Universe: Between the Horns of the Dilemma

Kelvin did not ask what sorts of cosmologies might be compatible with avoidance of the gravitational problem. That question was asked by Fournier d'Albe (1907), who noticed that Kelvin's condition on the matter density, when suitably strengthened, was compatible with a matter distribution that was homogeneous, in a significant sense. Charlier (1908) developed the idea in the following year. In brief, what they found was a scheme for distributing matter through infinite space that required no preferred center, but led to a vanishing matter density when averaged over all space. They imagined matter grouped locally in clusters; then these clusters were in turn grouped into clusters; and this next level of clusters into a higher level of clusters; and so on indefinitely. The spacing between the clusters could be so contrived that the mean matter density vanished over infinite space and in a way that escaped the gravitational problems. This hierarchic universe[49] passed between the horns of Einstein's and Seeliger's dilemma, for it was a universe with no preferred center, with matter distributed throughout, but free from Seeliger's gravitational divergences.

The work of Fournier d'Albe and Charlier and those who later developed the hierarchic cosmology depended on a small number of results that can be summarized as follows. Assume that the matter distribution is so contrived that the density of matter ρ at a distance r from some point in space diminishes according to[50]

$$\rho(r) = \frac{K}{r^n} \tag{13}$$

for $K > 0$ a constant and $n \geq 0$. Different values for n lead to more or less rapid approaches to zero density as r becomes infinite. Table 1 shows how suitable selection of n can ensure convergence of virtually every quantity involved in the cosmology as the volume of space under consideration grows indefinitely. (See Appendix B for supporting calculations):

<table>
<tr><th></th><th>$n=0$</th><th>$n=1$</th><th>$n=2$</th><th>$n=3$</th><th>$n=4$</th></tr>
<tr><td>Total Mass</td><td colspan="4">Diverges for $n \leq 3$</td><td>Converges for $n > 3$</td></tr>
<tr><td>Mean Density</td><td>K for $n = 0$</td><td colspan="4">Vanishes for $n > 0$</td></tr>
<tr><td>Gravitational Potential</td><td colspan="3">Diverges for $n \leq 2$</td><td colspan="2">Converges for $n > 2$</td></tr>
<tr><td>Gravitational Force</td><td colspan="2">Diverges for $n \leq 1$</td><td colspan="3">Converges for $n > 1$</td></tr>
<tr><td>Tidal Force Z_x</td><td>Diverges for $n = 0$</td><td colspan="4">Converges for $n > 0$</td></tr>
</table>

Table 1. Convergence properties of quantities in hierarchic cosmology

In particular, if we set a value of n for which $2 < n \leq 3$, then we have a cosmology with

- total matter of infinite mass
- vanishing mean density
- convergent gravitational potential, force and tidal force.

Such a cosmology passes between the horns of Seeliger's dilemma in so far as it has infinite total mass but there is no need to forgo Newton's law since the relevant gravitational quantities are well defined. We see also that Kelvin's condition of vanishing mean density is not sufficiently restrictive, since this vanishing obtains when $0 < n < 2$ for which not all the gravitational quantities are well defined.

[48] Nonetheless Seeliger carefully corrected his earlier misstatements of the consequences of his calculations with a similar formulation: "If Newton's law is to hold absolutely exactly, then infinitely great spaces of the universe cannot be filled with mass of finite density on average." (Seeliger 1909: 9). ("Wenn das Newton'sche Gesetz absolut genau sein soll, dann dürfen nicht unendlich große Räume des Weltalls mit Masse von durchschnittlich endlicher Dichtigkeit erfüllt sein.") Oddly Seeliger makes no mention of Charlier's 1908 hierarchic cosmology, which made manifest the need for such reformulation, although he must have been aware of it. For Seeliger (1909: 7) cites Arrhenius 1909 and the latter has an extensive discussion of Charlier's work. And we shall see below in Section 9.4 that Charlier later thanks Seeliger for a letter of 28 April 1909 in which Seeliger corrects a technical result in Charlier 1908. Seeliger 1909 was presented to the Bavarian Academy just three days later on 1 May 1909.

[49] The term 'hierarchic' seems to have been introduced as late as Selety (1922) who spoke of a "Molekularhierarchie" (p. 298) and "die molekularhierarchische Welt" (p. 304).

[50] More precisely, $\rho(r)$ is the average matter density on the *surface* of a sphere of radius r. It is important not to confuse this density with the mean density of matter over the entire *volume* of the sphere. The latter mean density only cannot diminish faster that $1/r^3$—the case of single centrally located mass.

9.4. How an infinite world may be built up

While it is easy to assume that cosmic matter dilutes with distance according to a rule such as (13), it is another matter to show that such dilution can be achieved within a matter distribution that is, in some significant sense, homogeneous. Modern work on this problem was initiated by an unlikely source. Fournier d'Albe 1907 is a somewhat inflated, semi-popular work intended to develop the notion that the world of the scale of our solar system is but one of an infinite hierarchy of worlds extending into the small and large. Proceeding into the small, the next level down is the atomic level; the atoms are the sun and the electrons the planets (pp. 84–85). Proceeding into the large, the units of the next level are stars clustered into galaxies. Fournier d'Albe's essential purpose was to elaborate this grandiose vision through a seamless fusion of simple technical results and wild speculation. However, the hierarchic structure of his world just happened to allow him to escape two problems of an infinite cosmology. The first was Olbers' paradox of the darkness of the night sky (see below). The second was Kelvin's concern that stellar matter be not so densely distributed that stars falling into our system acquire too great a velocity. To escape both difficulties, Fournier d'Albe assumed his systems so distributed that the matter in a sphere of cosmic size increases only in direct proportion to the radius of the sphere (Part II, chap. II). This rate of dilution corresponds with the setting of $n = 2$ in (13). It escapes Olbers' paradox, but Fournier d'Albe apparently did not notice that it only just fails to ensure the convergent potentials needed to escape high stellar velocities. The latter requires any $n > 2$.

The real value of Fournier d'Albe's work lay in the fact that Carl Charlier read the work and, as he tells us in the opening paragraph of Charlier 1908, saw in the hierarchic proposal a way to escape the problems of an infinite cosmology. Charlier gave the escape a mathematically precise form in his (1908) "Wie ein unendliche Welt aufgebaut sein kann." The essential content of (1908) was repeated, corrected and extended in a different article, Charlier 1922, but with the same title translated into English, " How an Infinite World May Be Built Up." His model proposed a hierarchy of larger galaxies and galaxy clusters, systems $S_1, S_2, \ldots$ such that

> N_1 stars form a galaxy S_1,
> N_2 galaxies of type S_1 form a second order galaxy S_2,
> N_3 galaxies of type S_2 form a third order galaxy S_3,
> etc.

The systems $S_1, S_2, \ldots$ are presumed spherical with radii $R_1, R_2, \ldots$ and have masses $M_1, M_2, \ldots$. As galaxies of the $(i-1)$th order are packed to form a galaxy of the ith order, empty space must be introduced in sufficient amount to ensure the dilution of the mean matter density. This is ensured by locating each galaxy of $(i-1)$th order and radius R_{i-1} in a sphere of otherwise empty space of radius ρ_i within a galaxy of ith order, as shown in Figure 5. Thus the mean density of matter will decrease as we proceed up the hierarchy if $R_{i-1}/\rho_i < 1$, for each i, since the ratio of mean density in systems of order $(i-1)$ and i is $(R_{i-1}/\rho_i)^3$.

Charlier identified three problems facing an infinite cosmology. Each could be

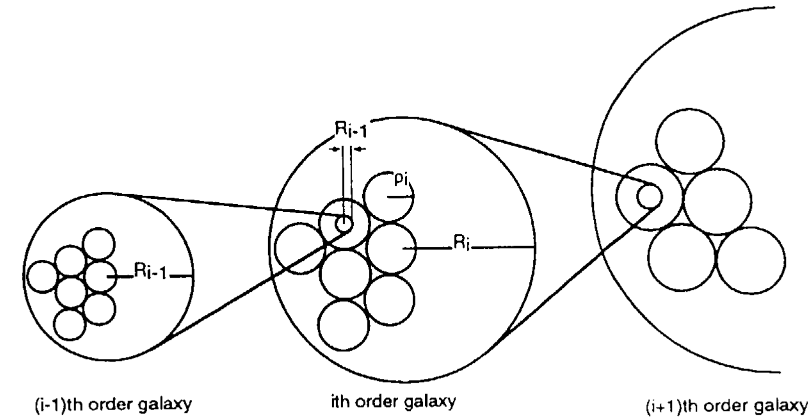

Figure 5. Charlier's hierarchic universe.

solved by requiring that the mean matter density dilute with increasing volumes of space at a suitable rate. Thus each problem could be restated as what he called a criterion that could be translated into a specific rate of dilution. The three criteria are:

> **Olbers' Criterion.** In 1826, Olbers had remarked that, if the universe were filled uniformly with stars, then the sky should glow as brightly as the face of the sun.[51] To see how this arises, in a form relevant to Charlier's work, note that the intensity of light from distant stars diminishes with the inverse square of distance. But if stars are distributed uniformly in space, then the number of stars increases with the square of distance. Thus the total light incident from such stars on the earth is represented by a diverging integral. The intensity of starlight in the sky of such a universe would be infinite.[52] This intensity should be finite.
>
> **Seeliger's Criterion.** The gravitational force in a universe with a uniform matter distribution diverges, as Seeliger pointed out. This force should be finite.
>
> **Velocity Criterion.** As celestial objects fall into the cosmic gravitational field, the depth of the potential well determines how great these velocities are. In an infinite universe with a uniform matter distribution, this well is infinitely deep. Since Charlier knew only

[51] The history of the paradox is a great deal richer than this simple remark suggests. See Jaki 1969.

[52] If the intensity of light due to a star at distance r is A/r^2 and the uniform density of stars is ρ, then the intensity of light due to stars at distance $r > R_0$ is $\int_{R_0}^{\infty} (A/r^2)\rho 4\pi r^2\, dr = \infty$. The occulting of more distant stars by nearer stars reduces the intensity expected to that of the surface of the sun.

of small velocities observed among celestial objects, he required that this well not be so deep as to yield stellar velocities beyond the modest ranges then known. In effect this criterion calls for the convergence of the gravitational potential.

We need not recapitulate Charlier's fairly complicated analysis since the essential results are already implicit in Table 1. Instead we can recover his results rapidly once we have connected the quantities of Charlier's model with the dilution rates of (13). To do this, when $r = R_i$, we will approximate the density $\rho(r)$ of (13) by the mean density $\overline{\rho}_i$ of the ith order galaxy. If the mass of a star is M_0, then the total mass of an ith order galaxy is $M_i = M_0 N_1 N_2 \cdots N_i$ and its mean density is

$$\overline{\rho}_i = \frac{M_0 N_1 N_2 \cdots N_i}{(4/3)\pi R_i^{\,3}}.$$

It follows that the mean densities of $(i+1)$th and ith order galaxies are related by

$$\frac{\overline{\rho}_{i+1}}{\overline{\rho}_i} = N_{i+1}\left(\frac{R_i}{R_{i+1}}\right)^3.$$

But we require that $\overline{\rho}_i$ dilute with distance as $1/r^n$. That is, that

$$\frac{\overline{\rho}_{i+1}}{\overline{\rho}_i} = \left(\frac{R_i}{R_{i+1}}\right)^n.$$

Comparing the last two equations, it follows that the requirement that the mean density dilute as $1/r^n$ is equivalent to requiring that the number of ith order galaxies in an $(i+1)$th order galaxy be set by[53]

$$N_{i+1} = \left(\frac{R_{i+1}}{R_i}\right)^{3-n}. \qquad (13')$$

Without calculation, we can see that both Olbers' and Seeliger's criterion amount to the same constraint. For Seeliger's criterion amounts to requiring that matter dilute sufficiently rapidly so that the gravitational lines of force emitted by cosmic matter not build to infinite density. These lines of force dilute with the inverse square of distance. Light from distant stars also dilutes with the inverse square of distance. Therefore any star distribution that satisfies Seeliger's criterion will satisfy Olbers' criterion and vice versa. We know from Table 1 that gravitational force converges whenever we set $n > 1$. Thus the two criteria are satisfied by

[53] It follows from (13′) that Charlier's model admits a maximum n of 3. This limit arises since the density $\rho(r)$ of (13), the density at the surface at radial distance r, is being approximated by a mean density over the whole volume of the various orders of galaxies. This mean density dilutes at its fastest when the entire universe has just one star, which is the matter of every order galaxy. It is the case of $n = 3$.

requiring that (13′) with $n = 1$ set an upper limit for N_{i+1}. Written after the form of Charlier 1922: 4, 6, the condition is

$$\frac{R_{i+1}}{R_i} > \sqrt{N_{i+1}}.$$

Unfortunately Charlier did not see the equivalence of the two conditions in 1908. He gave the correct result for Seeliger's criterion (1908: 9). But he gave a result for the Olbers' criterion (1908: 8–9) which he later described as "inexact" (1922: 5) and then published a corrected analysis supplied to him by Seeliger in a letter of 28 April 1909. He also reported (1922: 5) that Franz Selety had informed him through a letter of the correct results, apparently at the time of publication of Charlier 1922. Selety (1922: 299–300) recounts Selety's corrected analysis.

The velocity criterion can be treated similarly. From Table 1 we see that it amounts to the stronger requirement that $n > 2$. Thus, within Charlier's model, the criterion amounts to setting (13′) with $n = 1$ as an upper limit for N_{i+1}:

$$\frac{R_{i+1}}{R_i} > N_{i+1}.$$

Charlier's (1922: 14–15) version of this result replaces the strict inequality $>$ by an inequality $\geq$, apparently in error. Selety (1922: 300–301) again points out the error and in an addendum to proofs (1922: 322–324) criticizes Charlier's revised 1922 treatment.

9.5. The Einstein-Selety debate

The hierarchic cosmologies had already passed squarely between the horns of the dilemma Einstein had presented in his famous cosmology paper (1917a). Someone had to respond. So Franz Selety (1922) took on the task of dismantling Einstein's dichotomy and a great deal more. The paper is fairly long-winded and covers more material than can be reviewed here. However it does lay out quite clearly the options provided by Charlier's hierarchic model. As Selety summarized in his Introduction, this model allowed a Newtonian cosmology to satisfy all of

> 1. Spatial infinity of the universe,
> 2. Infinity of the total quantity of mass,
> 3. Complete filling of space with matter of overall finite local density.
> 4. Vanishing of the average density of the universe.
> 5. Non-existence of a singular center point or central region of the universe.[54] (Selety 1922: 281)

In addition, of course, he recalled how a suitable rate of dilution of matter with distance would escape Olbers' paradox and the divergence of gravitational force

[54] "1. Räumliche Unendlichkeit der Welt,
2. Unendlichkeit der Massengröße,
3. Vollkommene Erfüllung des Raumes mit Materie von überall endlicher lokaler Dichte.
4. Verschwinden der mittleren Dichte der Welt.
5. Nichtexistenz eines singulären Mittelpunktes oder Mittelgebietes der Welt."

noted by Seeliger. Finally Selety expended considerable effort to urge that the hierarchic universe provides a way of satisfying the Machian requirements that had influenced Einstein so profoundly in his work on general relativity.

However Selety differed from the standard view of which divergences must be avoided in Newtonian cosmology. He recognized that diverging gravitational potentials could be avoided by allowing the matter density of (13) to dilute with distance faster than $1/r^2$ (p. 287). However he was willing to entertain the divergence of the gravitational potential because he found the case of the $1/r^2$ dilution to have especially interesting properties. In particular, in this case, the mass M_i of an ith order galaxy would be proportional to its radius R_i. The $(i-1)$th order galaxies within it would be in gravitational free fall and the order of magnitude of their velocities would be determined by the depth of the gravitational potential well of the ith order galaxy. The depth of that well was fixed by M_i/R_i. So if M_i was proportional to R_i that depth would be same at all levels. Thus Selety could conclude something very pretty about the velocity of any galaxy with respect to the next order galaxy that contains it: at all levels of the hierarchy, they have the same order of magnitude. Selety had arrived at a similarity between the structures of each order in the hierarchy. As one moved up the hierarchy the lengths, times and masses of processes would preserve their ratios. Selety was sufficiently impressed with this result to want to suggest a "relativity of magnitude" (*Relativität der Größe*) in the hierarchic universe in which matter density diminishes as $1/r^2$ (p. 304).[55]

This relativity of magnitude allowed Selety to address a thorny problem of the hierarchic universe. Einstein had observed that a finite cluster of stars in empty space would scatter. Might not the same fate befall a hierarchic universe? Each galaxy is a region of greater density within a region of lesser density.[56] Selety had to concede that it is of infinitely small probability that normal dynamical processes could bring about a hierarchic structure. But, once it was in existence and if it diluted its matter density as $1/r^2$, then this relativity of magnitude would ensure that it could not lose its hierarchic structure in any finite time. The reasoning was simple. Imagine some process through which a galaxy of some order is destroyed. That same process could also befall higher order galaxies. However the time required for the process to be completed would grow in direct proportion to the size of the galaxies. There is no upper limit to the orders of the galaxies and no upper limit to their size. Therefore, for any such process of destruction and any nominated finite time, there is some order in the hierarchy beyond which it has not had an effect by that time.[57]

[55] Selety did not give details of the derivation of this necessarily vague result. Presumably we must imagine that the velocity of an $(i-1)$th order galaxy is given as the ratio of some characteristic length and time. If the velocity is to remain roughly constant as we proceed up the hierarchy, these lengths and times must preserve their ratio. The masses of systems would also grow in direct proportion to the lengths and times since the total mass of a galaxy grows, by supposition, in proportion to its radius, as we move up the hierarchy.

[56] Arrhenius (1909: 227) worried about exactly such instabilities, including also the possibility of eventual collapse of the galaxies under their own gravity.

[57] I cannot resist observing the unhappy corollary. Since Newton's theory of gravitation is time reversible, in so far as Selety's result obtains, it also supplies a proof that a hierarchic universe of the type he favors cannot arise through normal processes from a uniform matter distribution in any finite time.

The case of matter diluting as $1/r^2$ had further special importance. For it enabled Selety to mount a challenge to a major part of Einstein's 1917 argument. There, as we have seen in Section 5 above, Einstein (1917a: § 1) had argued against the possibility of the stars clustering in an island in an otherwise empty universe. If we assumed that the gravitational potential at spatial infinity is finite, then by statistical mechanical arguments, that island would evaporate. Each star would, over time, through random processes acquire sufficient energy to escape the island. Boltzmann's analysis of a kinetic gas in a gravitational field also yielded a relationship between the equilibrium densities ρ and ρ_0 of an isothermal gas in regions, where the potentials are φ and φ_0 respectively:

$$\rho = \rho_0 \exp\left(-\frac{\varphi - \varphi_0}{RT}\right). \tag{14}$$

The gas temperature is T and R is the ideal gas constant.[58] We can now estimate the equilibrium density ρ at spatial infinity that must be supported by a density ρ_0 at the island. The potential difference $(\varphi - \varphi_0)$ between spatial infinity and the stellar island is assumed to be finite. Thus the ratio ρ/ρ_0 would be greater than zero and the density at infinity not zero after all. A stable island is possible only if it is not an island!

The weak point in Einstein's argument was the presumption that the potential at infinity had to be not just finite but quite small. This followed, Einstein urged, because infinite potentials would be reflected in huge stellar velocities and since "the value of the gravitational potential [is] itself necessarily conditioned by heavenly bodies." (1917a: § 1)[59] If the latter remark was intended to indicate that no island matter distribution could generate an infinite potential difference, then Einstein was wrong, as Selety now demonstrated. He assumed (pp. 288–295) a central massing of matter whose density diluted indefinitely with distance. He also assumed that this matter was an ideal gas with pressure p and temperature T obeying the ideal gas law

$$p = \rho RT.$$

He asked how the density ρ might dilute with distance if the gas was assumed to be held in static equilibrium in its central massing by its own gravitational field. He modeled this equilibrium by assuming the gas to be in an isothermal state and that the outward forces of its pressure p exactly balanced the inward gravitational forces. In a brief analysis, he showed that this latter condition of mechanical equilibrium was satisfied by an isothermal, ideal gas whose density diluted with

[58] In this discussion, the density ρ is a molar density and the potential φ has units of energy per mole.

[59] "der Verlauf des Gravitationspotentials durch die Himmelskörper selbst bedingt sein mußte."

distance r from the center as[60]

$$\rho = \frac{RT}{2\pi G r^2}. \tag{15}$$

That is, the density ρ dilutes as $1/r^2$. The gravitational potential $\varphi(r)$ grew logarithmically with r as

$$\varphi(r) - \varphi(r_0) = G \int_{r_0}^{r} \frac{\rho}{r'} 4\pi r'^2 \, dr' = 2RT \ln \frac{r}{r_0}. \tag{16}$$

It now followed that the density dilution of (15) was fully compatible with the Boltzmann's result (14). Indeed, as Selety noted (p. 293), (15) and (16) entailed the Boltzmann formula (14)! And the potential $\varphi(r)$ would grow infinitely large with r while ρ dropped to zero. Thus, kinetic matter, be it an ideal gas or a cluster of stars, could be held in a stable island by its own gravitational field if its density diluted as $1/r^2$.

Einstein's evaporation argument was almost completely defeated. His last refuge was the remark that large potential differences happen not to obtain in our universe for we do not observe the large velocities they would engender. Here Selety faltered with a weak response. Such high velocities are possible, he urged, but they are just improbable and that is why we do not see them. One argument (p. 293) for this low probability was that stellar velocities in such a universe would be distributed according to the Maxwell velocity distribution. In the case of lower temperatures this accords low probability to high velocities.[61]

Within a few months, Einstein (1922) had published his reply. Selety's assault on Einstein's cosmological work clearly found its mark through Selety's assertion that the hierarchic cosmology would have the Machian character Einstein sought in general relativistic universes. Einstein spent the bulk of his response justifying his Machian requirements and arguing that the hierarchic universe fails to meet them. Einstein seemed rather unmoved by Selety's dismantling of his 1917 dilemma for Newtonian cosmology. He wrote only a paragraph on the matter, essentially conceding his error. It read:

> It is to be admitted that the hypothesis of the "molecular-hierarchic" character of the construction of the universe of stars has much going for it from the standpoint of Newtonian theory, even though we may consider the hypothesis of the equivalence of the spiral nebulae and the Milky Way as refuted by the latest observations. This hypothesis explains naturally the darkness of the heavens and resolves Seeliger's conflict with Newton's law, without conceiving of matter as an island in empty space.
>
> Also from the standpoint of the general theory of relativity, the hypothesis of the molecular-hierarchic construction of the universe is *possible*. But from the standpoint of this theory the hypothesis is nevertheless to be seen as unsatisfactory.[62] (Einstein 1922: 436; emphasis in original)

Selety pursued the debate vigorously with two rejoinders (1923a, 1924) and also (1923c). Einstein did not reply again.

9.6. The brief celebrity of the hierarchic cosmology

Within the renewed interest in gravitation and cosmology at this time, the hierarchic cosmology enjoyed its moment of greatest celebrity. The mathematician, Émile Borel (1922) reported in April 1922 that Einstein's recent visit to the Collège de France had inspired new attention on the problem of the finitude or infinitude of space. Borel's contribution was not directly to the physics. Rather it was to give an appealing plausibility argument for the possibility of matter distributions such as are proposed by the hierarchic cosmology: Their average matter density is zero. But this does not mean that all their matter is concentrated in a central island. They still exhibit a kind of homogeneity that Borel called "quasi-periodic" [*quasi-périodique*].

Borel's contribution was to display an arithmetic model with these same properties. He considered the sequence 'α' of integers which use only the digits 0 and 1:[63]

$$\begin{cases} \ldots, & -111, & -110, & -101, & -100, & -11, & -10, \\ -1, & 10, & 11, & 100, & 110, & 111, & 1000, & \ldots. \end{cases}$$

The density of integers in α amongst all integers $\ldots, -5, -4, -3, -2, -1, 0, 1, 2, 3, 4, 5, \ldots$ has properties analogous to those of the density of matter in a hierarchic universe. Consider that subset of the integers with fewer than n digits. There are $(2^n - 1)$ integers of α in that subset of $(2/9)(10^n - 1)$ integers overall. So the density of integers of α is $9(2^n - 1)/2(10^n - 1)$ which has an upper bound of $(1/5)^{n-1}$. Thus, as the size of the subset of integers considered grows infinitely large, the density of members of a drops to zero. However, at the same time, the distribution of α exhibits a kind of homogeneity expressible as a quasi periodicity. Consider, for example, the pattern of members of α in the vicinity of 100: ... , 100, 101, 110, 111, ... This same pattern is reproduced arbitrarily often through the integers. We find it in the neighborhood of 1100, of 10100, of 11100, of 10000100, etc. In the last case it arises as ... ,10000100, 10000101,

[60] Selety's analysis is given in a footnote on p. 292. While I can reproduce the result, I cannot reproduce his second equation of the footnote and suspect it is a typographical error.

[61] The weakness of the argument lies in its vagueness. How low is low? How high is high? How do they compare with observed velocities? It is interesting to note that with the advent of neo-Newtonian cosmologies of Milne and McCrea in the 1930s, the presumption against gravitational potentials that diverge at spatial infinity disappeared. In those cosmologies, the gravitational potential increases with the square of distance from the origin of coordinates, thus diverging at infinity.

[62] "Es ist zuzugeben, daß die Hypothese vom 'molekularhierarchischen' Charakter des Aufbaues der Sternwelt vom Standpunkt der Newtonschen Theorie manches für sich hat, wenn auch die Hypothese von der Gleichwertigkeit der Spiralnebel mit der Milchstraße durch die letzten Beobachtungen als widerlegt zu betrachten sein dürfte. Diese Hypothese erklärt ungezwungen das Nichtbuchten des Himmelsgrundes und vermiedet den Seeligerschen Konflikt mit dem Newtonschen Gesetz, ohne die Materie als Insel im leeren Raum aufzufassen.

Auch vom Standpunkte der allgemeinen Relativitätstheorie ist die Hypothese vom molekularhierarchischen Bau des Weltalls möglich. Aber vom Standpunkt dieser Theorie ist die Hypothese dennoch als unbefriedigend anzusehen."

[63] This is the series as given by Borel. Presumably the omission of 0 and 1 from the series is an error.

10000110, 10000111, ... This homogeneity, as Borel pointed out, exhibits one defect. The pattern at 0 of three successive members of α, $(\ldots, -1, 0, 1, \ldots)$ is reproduced nowhere else in the integers. Also the distribution of members of a exhibits reflection symmetry only when reflected about 0.

Borel's work attracted immediate response. Costa (1922) pointed out that Borel's pattern of distribution of masses would not allow one to escape infinite gravitational potentials. Selety (1923b) complained that Borel's construction contained a center of gravity and provided a multidimensional alternative free from the problem, based on a construction proposed by Fournier d'Albe.

This activity surrounding Borel's work reflected the growth of interest in the hierarchic cosmology. In a last minute addition (27 June 1922) to his original paper, Selety (1922) had reported Borel's efforts. In his reply to Einstein, Selety (1923a: 58) was pleased to note that he had become aware of even more work on the hierarchic cosmology, listing four recent papers, including Costa 1922. The cosmology also started to enter the relativity textbooks. Silberstein (1924: 540–543) included a short appendix reviewing the cosmology. However the hierarchic cosmology shone only briefly. In later decades it enjoyed no serious presence in relativity or cosmology texts. A major exception is the cosmology article written for the *International Encyclopedia of Unified Science* by Erwin Findlay-Freundlich, an astronomer and former associate of Einstein. Freundlich (1951: 5, 23–27) considered the hierarchic hypothesis within the context of the Milne-McCrea neo-Newtonian cosmologies and argued that it provides an escape from the diverging gravitational potential at spatial infinity of these cosmologies. Freundlich seemed to prefer, however, that Newton's law be modified so that a uniform matter distribution becomes admissible and a big bang singularity avoidable.

10. Escape through the finitude of space

We have seen how one can escape the gravitational problem for Newtonian cosmology by reducing the amount of matter the cosmology presumes. The attraction of the hierarchical universe is that this reduction is effected with minimal compromise to the homogeneity of the matter distribution. There is a more direct way of achieving this end. We need only presume that the geometry of space is such that it is closed so that a uniform matter distribution corresponds with a finite total mass. Such was the supposition explored by Lense (1917: 1050–1054), as part of a broader analysis of Newton's law of gravitation in non-Euclidean spaces. Lense assumed a spherical-elliptical space and asked after the gravitational potential, force and tidal force given by Seeliger's formulae (1).[64]

It was not so clear which was the appropriate form of Newton's law to employ in such a space. The simplest choice was to continue to allow gravitational force

[64] The volume element of these formula, $r^2 \sin\gamma \, dr \, d\gamma \, d\theta$, needed to be altered to $\sin^2 r \, \sin\gamma \, dr \, d\gamma \, d\theta$ to accommodate the non-Euclidean geometry.

to vary with the source mass μ and distance r as

$$\frac{\mu}{r^2}.$$

However this dependence is better adapted to a Euclidean space where it entails a conservation of lines of force. Since the areas of spherical shells containing the mass μ grow in proportion to r^2 in a Euclidean space, this μ/r^2 force law ensures that the total flux of lines of force penetrating a sphere remains constant, proportional to μ, on each sphere. In a spherical space, the areas of the corresponding spheres grow with radius r as $4\pi R^2 \sin^2(r/R)$, where R is the radius of curvature of the space. So an analogous gravitation law that conserves lines of force would have a dependence

$$\frac{\mu}{R^2 \sin^2(r/R)}.$$

Lense's results were convergent integrals. The first dependence gives constant values for the potential and tidal force and a vanishing gravitational force. The second law gives a constant value for the potential and vanishing force and tidal force.

11. Conclusion

With the discovery of the recession of the galaxies, cosmology changed and with it Newtonian cosmology. Milne (1934) and Milne and McCrea(1934) proposed neo-Newtonian cosmologies that mimicked the dynamics of the relativistic Friedman universes. They still presumed a uniform matter distribution and retained Newton's inverse square law of gravitation. So Seeliger's original problem still held and had to be addressed. In the decades that followed a new resolution of the problems took the place of the many escapes that had been tried prior to 1930. That new resolution left untouched the 'cosmological' and 'gravitational' commitments of Newtonian cosmology, introduced at the beginning of this paper. Instead it sought to modify the 'kinematical' commitments of Newtonian cosmology. The idea was perhaps implicit in Milne and McCrea's original work; it is stated more explicitly in Milne (1942); and it is developed through the work of a number of researchers over the decades that follow, finding its most complete, modern statement in Malament (1995). That escape amounts to the recognition that there is a relativity of acceleration in Newtonian cosmology, so that the specification of which are the inertial frames becomes as much a matter of convention as does the specification of a rest frame in special relativity. Since gravitational force in Newtonian theory is only defined once the inertial frames are specified, the indeterminacy of gravitational force that Seeliger revealed reflects only this indeterminacy of inertial frames (see Norton 1995).

Perhaps the most astonishing part of our story is that both of the greatest figures of cosmology and gravitation, Newton and Einstein, stumbled on the same problem. When presented with the problem, Newton seemed so sure that his cosmology would be well behaved that he saw no need to think the problem through. Einstein also was overly hasty, seeing in the problem a dilemma for Newtonian cosmology that others showed to be a false dilemma. There is a final irony in Einstein's association with the problem. He thought it a useful foil for his 1917 work in cosmology. The deeper purpose of that work was to show how his new general theory of relativity could be made compatible with the demands of a generalized principle of relativity—something that he believed impossible in Newtonian theory. Yet we now see in Einstein's foil the strongest evidence of a relativity of acceleration in Newtonian cosmology. However debate still rages over whether Einstein has had any success in his efforts to find a relativity of acceleration through his general theory of relativity.[65]

[65] For a survey of this long standing debate see Norton 1993b.

Appendix A: **Seeliger's derivation of his expressions for gravitational potential, force and tidal force**

Seeliger's (1895a) derivation of his formulae (1) is recapitulated in Seeliger 1896, 1909. It proceeded as follows. He considered a point A in space surrounded by the masses of the universe and sought the gravitational effect of these masses at A. He assumed that the matter in each mass is arranged in a sphere. Within each sphere, the mass is laid out in concentric spherical shells of constant density. (This is a close approximation of how the matter of the stars and planets are actually arranged.) This matter distribution will be discontinuous. Seeliger proposed to replace it by a piecewise continuous distribution with the same gravitational effects. To do this, he expanded each sphere into a much large sphere of the same mass. As long as the matter in the sphere remains in concentric shells of constant density and as long as the spheres do not grow so large as to engulf A, a well-known theorem of Newtonian gravitation theory tells us that the gravitational effect at A of the spheres will remain the same. Choosing any suitable one of the many ways of expanding the spheres that remain within these limits, Seeliger replaced the original matter distribution with a continuous distribution with an everywhere finite mass density ρ.

Seeliger set up a spherical coordinate system (r, θ, γ) centered on a point O *other* than A, with A lying at a distance x from O on the $\gamma = 0$ axis, as shown in Figure A1. Consider a volume element at (r, θ, γ) bounded by coordinate differentials $dr, d\theta, d\gamma$. Its mass dm is given by $\rho r^2 \sin\gamma\, dr\, d\theta\, d\gamma$. Consider all such mass elements in the volume V between a surface $r = R_0$ and $r = R_1$, where R_0 and R_1 will vary with the angular coordinates θ and γ. The gravitational potential due to all these elements at A is

$$\varphi(x) = -G\int_V \frac{dm}{s} = -G\int_{\theta=0}^{2\pi}\int_{\gamma=0}^{\pi}\int_{r=R_0}^{r=R_1} \frac{\rho\, r^2 \sin\gamma\, dr\, d\gamma\, d\theta}{s}, \tag{A1}$$

where s is the distance from A to the mass element dm. The function $\varphi(x)$ shows how the gravitational potential φ varies along the line OA as the distance from O to A changes. This functional dependence encodes both the gravitational and tidal force at O, as we see if $\varphi(x)$ is expanded as a Taylor series at O

$$\varphi(x) = \varphi(0) + x\left.\frac{d\varphi}{dx}\right|_{x=0} + \frac{1}{2}x^2\left.\frac{d^2\varphi}{dx^2}\right|_{x=0} + \ldots. \tag{A2}$$

The term in x contains the gravitational force at O, and the term in x^2, the tidal force at O, $Z_x = -d^2\varphi/dx^2$. To recover F_x and Z_x, Seeliger needed to write (A1) as a power series expansion. This he was able to do by means of a standard result in function theory. In the spherical coordinate system given, it turns out that $1/s$ can be written as an infinite series of Legendre polynomials

$$\frac{1}{s} = \frac{1}{r}\left(P_0(\cos\gamma) + \frac{x}{r}P_1(\cos\gamma) + \frac{x^2}{r^2}P_2(\cos\gamma) + \ldots\right),$$

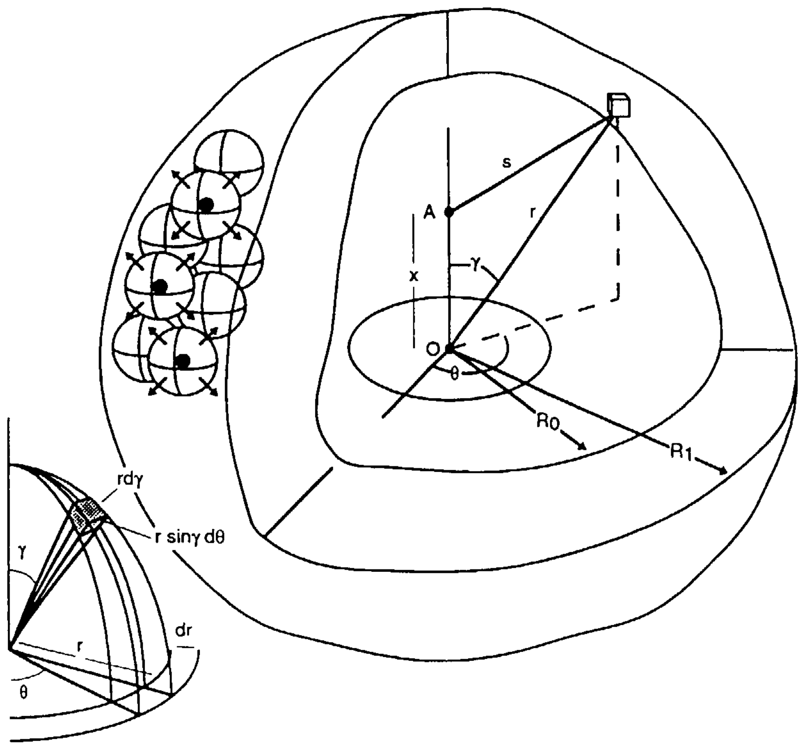

Figure A1. Seeliger's derivation.

where the Legendre polynomials are

$$P_0(\cos\gamma) = 1, \quad P_1(\cos\gamma) = \cos\gamma, \quad P_2(\cos\gamma) = \frac{1}{2}(3\cos^2\gamma - 1), \ \ldots$$

Substituting for $1/s$ in (A1) we recover

$$\begin{aligned}\varphi(x) = &- G\int_{\theta=0}^{2\pi}\int_{\gamma=0}^{\pi}\int_{r=R_0}^{r=R_1} \rho\, r\, P_0(\cos\gamma)\sin\gamma\, dr\, d\gamma\, d\theta \\ &- xG\int_{\theta=0}^{2\pi}\int_{\gamma=0}^{\pi}\int_{r=R_0}^{r=R_1} \rho\, P_1(\cos\gamma)\sin\gamma\, dr\, d\gamma\, d\theta \\ &- x^2G\int_{\theta=0}^{2\pi}\int_{\gamma=0}^{\pi}\int_{r=R_0}^{r=R_1} \frac{\rho}{r}\, P_1(\cos\gamma)\sin\gamma\, dr\, d\gamma\, d\theta \\ &- \ldots\end{aligned} \tag{A3}$$

Comparing the terms of (A2) with (A3), we can read off directly Seeliger's expressions (1) for φ, F_x and Z_x at the origin of coordinates O.

For uniformity of exposition I have altered Seeliger's notation. I have restored G which Seeliger set to unity by a choice of units. His density δ, potential V, force X, tidal force Z, displacements a and ρ and his coordinate system (r, φ, γ) have been replaced by my density ρ, potential $-\varphi$, force F_x, tidal force Z_x, displacements x and s and my coordinate system (r, θ, γ). (Note the sign change in V.) Seeliger also employs the convention of writing the integral $\int f(x)\,dx$ as $\int dx\, f(x)$ so that it is easy to mistake what lies within the scope of an integration operation in a multiple integral.

Appendix B: **Convergence of quantities in hierarchic cosmologies**

We assume that the density ρ of matter dilutes with distance r as K/r^n. Hence we compute the entries in Table 1 as follows. The total mass M enclosed between a sphere of radius R and a smaller sphere of radius R_0 is given as

$$M = \int_{R_0}^{R} \rho\, dV = 4\pi K \int_{R_0}^{R} r^{2-n}\, dr$$

$$= \begin{cases} \dfrac{4\pi K}{3-n}\left[R^{3-n} - {R_0}^{3-n}\right] & \text{for } n \neq 3, \\ 4\pi K \log \dfrac{R}{R_0} & \text{for } n = 3. \end{cases}$$

(The volume element dV is $4\pi r^2\, dr$.) Hence M converges as $R \longrightarrow \infty$ just in case $n > 3$. The mean density $\overline{\rho}$ for this shell is likewise

$$\overline{\rho} = \frac{M}{(4\pi/3)\left(R^3 - {R_0}^3\right)} = \begin{cases} \dfrac{3K\left(R^{3-n} - {R_0}^{3-n}\right)}{(3-n)\left(R^3 - {R_0}^3\right)} & \text{for } n \neq 3, \\ \dfrac{3K \log(R/R_0)}{\left(R^3 - {R_0}^3\right)} & \text{for } n = 3. \end{cases}$$

Hence $\overline{\rho} \longrightarrow 0$ just in case $n > 0$. The convergence of the potential φ, gravitational force F_x and tidal force Z_x due to masses in this shell is decided by Seeliger's three integrals (6). For the case of the gravitational potential, we have

$$\int_{R_0}^{R} \rho r\, dr = \int_{R_0}^{R} K r^{1-n}\, dr = \begin{cases} \dfrac{K}{2-n}\left(R^{2-n} - {R_0}^{2-n}\right) & \text{for } n \neq 2, \\ K \log \dfrac{R}{R_0} & \text{for } n = 2. \end{cases}$$

Hence the potential φ converges as $R \longrightarrow \infty$ just in case $n > 2$. For the case of the force F_x we have

$$\int_{R_0}^{R} \rho\, dr = \int_{R_0}^{R} K r^{-n}\, dr = \begin{cases} \dfrac{K}{1-n}\left(R^{1-n} - {R_0}^{1-n}\right) & \text{for } n \neq 1, \\ K \log \dfrac{R}{R_0} & \text{for } n = 1. \end{cases}$$

Hence the force F_x converges as $R \longrightarrow \infty$ just in case $n > 1$. For the case of the tidal force Z_x we have

$$\int_{R_0}^{R} \rho r^{-1}\, dr = \int_{R_0}^{R} K r^{-n-1}\, dr = \begin{cases} -\dfrac{K}{n}\left(R^{-n} - {R_0}^{-n}\right) & \text{for } n \neq 0, \\ K \log \dfrac{R}{R_0} & \text{for } n = 0. \end{cases}$$

Hence the tidal force Z_x converges as $R \longrightarrow \infty$ just in case $n > 0$.

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