Curved spaces: Mathematics and empirical evidence, ca. 1830 – 1923

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In the following we survey the attempts to find empirical bounds for the curvature of physical space from astronomical data over a period of roughly a century. Our report will be organized in four sections: (1) Lobachevsky, (2) Gauss and his circle, (3) astronomers of the late 19th century, (4) outlook on the first relativistic cosmological models. In the first three passages we indicate how parallax data were used for inferences on a hypothetical curvature of astronomical space by different authors using only slightly different methodologies. In the last phase a new methodological approach to physical geometry was opened by general relativity, and two completely new data sets came into the game, mass density and cosmological redshift.

Before we enter the discussion of the astronomical data, one has to notice that astronomical observations were not the only route toward empirical data on space curvature bounds. We have reports, although only scarce direct information, that C.F.Gauss used his high precision geodetical measurements in the early 1820s for gaining a first secure empirical estimation for a bound of the "constant" K of non-Euclidean geometry (NEG). There has been an extended discussion in the history of mathematics whether or not this report can be trusted. I consider it as reliable; at other occasions I have made my arguments explicit. [1]

(1) Lobachevsky (1830 – 1840)

Already in the publication of his new geometry in the *Kazan vestnik* N.I. Lobachevsky gave a first estimation of space curvature by astronomical data (Lobatschevsky 1829-30/1898*b*). In the second part of his series (published 1830) he discussed the idealized constellation of a fixed star C with parallax angle $2p$ standing exactly orthogonal above a point A of the earth orbit (relative to the orbit plane). At the opposite point B of the orbit, with distance $\overline{AB} =: a$, the asymptotic parallel to AC would have an angle of parallelity $F(a)$, which he had calculated before. Obviously the parallax angle of the star in C was bounded by the angle of parallelity; more precisely $F(a) \geq \frac{\pi}{2} - 2p$. From his calculation of $F(a)$ Lobachevsky derived the

[1] Cf. (Miller 1972, Scholz 2004, Scholz 2006).

inequality

$$a < \tan 2p \; [\approx 2p] \, .$$

He quoted without reservation the (unreliable) parallax data of the Belgian astronomer d'Assad Montdardier published in the journal *Connaissance des tems, ou des mouvements célestes ... pour l'an 1831* (published 1828). From the smallest parallax angle given there, $1.24''$ for Sirius, Lobachevsky derived the estimation

$$a < 6.012\ 10^{-6} \quad \text{in units } K = 1 \quad .$$

We my prefer to read his result the other way round,

$$K > 3 \cdot 10^5 \, AU \approx 2.4 \, LY \tag{1}$$

($1\,AU = \frac{a}{2}$ the *astronomical unit*, *LY light year*). We see that Lobachevsky's bound for K was quantitatively identical with the distance of the star which one would have calculated from the "observational" data in Euclidean geometry.

In a second step he calculated the angle deficit δ in a solar system triangle formed by a base and a height approximately equal to the diameter of the earth orbit. He found $\delta < 3.7'' 10^{-6}$. He concluded that this deviation was small enough to consider the foundations of "ordinary geometry ... as if they had been proven rigorously" (Lobatschevsky 1829-30/1898*b*, 24). For him the *proof of the foundations* of geometry was apparently a question of a sound empirical corroboration.

In many of his other publications Lobachevsky discussed the relationship between physical and astronomical space to his new geometry. Sometimes he included quantitative estimations, although mostly weaker ones than in 1830,[2] sometimes by qualitative methodological remarks, highly interesting in themselves. In his *Novie nachalie ...* he considered force laws as origin for the metric of the physical geometry and hypothetically allowed even to consider different geometries for different kinds of forces (Lobatschevsky 1835/1898*a*, 76), (Lobachevsky 1949, 158) .

(2) Gauss and his local environment (1840 – 1855)

Gauss' correspondence shows that since his turn towards the *non-Euclidean* perspective on the foundations of geometry (in I. Toth's terminology) about 1815 he saw the necessity of empirical studies of precision bounds for the validity of Euclidean geometry. In 1823/24 he was able to conclude from his

[2] In his Crelle Journal article (Lobatchevsky 1837) he quoted the angle sum deficit in a solar system triangle (apparently erroneously with a value weakened by 2 decimals) (Bottazzini/Tazzioli 1995, 28); in his booklet (Lobatschevsky 1840, 60) he weakened the bound again by an order of magnitude. See also (Daniels 1975).

high precision geodetical measurements a first methodologically well founded bound for the "constant" K of NEG:

$$K > 5\,R\,(\approx 2\cdot 10^{-3} AU \approx 10^{-9} LY)\;, \quad R = \text{earth radius} \tag{2}$$

This bound was disillusioningly small from the astronomically point of view, but the only well founded estimation at the time.[3] Gauss knew well that parallax measurements were completely unreliable at the time. In a letter to H.C. Schumacher (29. June 1831) Gauss reported on properties of NEG which came close to a method to determine bounds of space curvature by parallax data.[4] He described a generalized version of the "angle of parallelity", with respect to a transversal straight line $h = AC$ at any angle $\alpha = \angle BAC$ to a given straight line $g := AB$. With his knowledge about angle sums in NEG (structurally comparable to the angle sum theorem of his surface theory) he could have given a similar argument as Lobachevsky in 1830, even in a slightly less idealized constellation. But apparently he never did; even not *after* Bessel's precision measurement of the parallax of 61 Cygni in 1838.[5] The reasons for his caution may have been the difficulty to find a logically well founded inequality between the parallax and the angle of parallelity. Lobachevsky had avoided even to pose the problem in his idealized estimation.

B. Listing who cooperated with Gauss in the Gauss-Weber seminar over many years reported to his own students in the 1870s that Gauss had discussed the question of determination of the nature of physical space by astronomical observations in his seminars in the 1840s and/or early 1850s, but apparently never claimed to have a solution to it.[6]

While Gauss remained very cautious, his student B. Riemann dared a more definite claim. At the end of his famous *Habilitationsvortrag* of 1854 Riemann discussed the application of the concept of manifold to the determination of the structure of "the space", i.e., physical space (sect. III.3). Just in passing he made a remark which sheds light on how he thought on the question:

> If one assumes existence of the bodies independent on the place, then the curvature measure is constant everywhere, and it follows from astronomical measurements that it cannot be different from zero; at least its reciprocal value had to be an area against which the area accessible to our telescopes had to vanish. (Riemann 1867, 285)

In slightly later terminology the first subphrase means: Assuming free mobility of rigid bodies, the (sectional) curvature is constant, $\kappa = c$. The rest

[3] Cf. footnote (1).

[4] (Gauss Werke VIII)

[5] Bessel's value for the parallax was $p \approx 0.3''$, distance $d \approx 10^6 AU \approx 10\,LY$.

[6] (Hoppe 1925)

says, generally spoken, by astronomical reasons $c << 1$. Notice Riemann's interesting form of double negation:[7] $\neg(\kappa \neq 0)$ meant for him ("at least") $\kappa << \epsilon$ for any reasonably small ϵ constrained by the empirical constellation, here $\epsilon = A(\triangle)^{-1}$ (see below). The last subphrase formalizes as

$$|\kappa^{-1}| >> A(\triangle), \quad A \text{ area of triangles } \triangle \text{ accessible to telescopes.}$$ [8]

This was nothing but a reformulation of

$$|\kappa| A(\triangle) << 1 \,, \quad \text{or} \quad |\kappa| << A(\triangle)^{-1}. \tag{3}$$

By his remark Riemann expressed the experience that the Gaussian correction term of angle sums could be considered as negligible in all practical applications of astronomy of the time. In this regard Gauss and his environment apparently agreed with Lobachevsky, independent of how much was known in detail about his arguments and whether one agreed with his derivation of this result. In any case Riemann's formulation expressed the state of art of theoretical evaluation of the results of high precision astronomy at the time of Bessel and Gauss. His statement was empirically and theoretically well founded, but phrased in a rather complicated way.

If, in addition, one assumed at least as *plausible* that the parallax p and the angle of parallelity (or more precisely its difference δ to $\frac{\pi}{2}$ etc.) were at the same order of magnitude $\delta \sim 10^{-1}" \approx 10^{-8}$ and took into account that $A(\triangle) \approx 10^{-4}\, LY^2$ in sufficient approximation even in a slightly curved space, one could easily derive the estimation $|\kappa| A(\triangle) < \delta$ from a consideration close to Riemann's.[9] Then

$$|\kappa| < 10^{-4}\, LY^{-2} \,, \quad K = \sqrt{|\kappa|}^{\,-1} > 10^2\, LY \,. \tag{4}$$

Riemann did not claim so, maybe because here the mentioned simplifying plausibility assumption had to be used. Inequality (4) shows, however, that even with Bessel's improved parallax data the Gauss/Riemann approach would lead to only a minor improvement of the order of magnitude for space curvature in comparison to Lobachevsky's from 1830.

(3) Astronomers at the end of the century, Ball and Schwarzschild (ca. 1880 – 1900)

Only a few theoretically inclined astronomers of the late 19th century dealt with the question of how to determine curvature bounds of astronomical

[7] Brouwer and Weyl should have appreciated such a kind of *tertium datur*.

[8] For AB diameter of earth orbit, C, C' two close fixed stars used for the observation of the yearly change of angle difference in Bessel's e.a. parallax observations, two kinds of triangles had to be considered as "accessible to our telescopes": $\triangle ABC$ resp. $\triangle ABC'$ or $\triangle CAC'$ resp. $\triangle CBC'$.

[9] Compare with Schwarzschild's argument below.

space, among them Rober S. Ball (1840 – 1913), Astronomer Royal of Ireland at the observatory Dunsink, and Karl Schwarzschild (1873 – 1916). Most of the astronomers including theoretically inclined ones, like H. Bruhns, did not care about the question, or frankly rejected a potential importance of NEG for astronomy, like H. Seeliger.[10] Ironically K. Schwarzschild was set on the trail of considering this question in the defense of his habilitation, when Seeliger assigned him the task to defend the thesis that NEG useless for astronomy. This incited Schwarzschild to look at the question a little later the other way round.[11]

R.S. Ball caught fire for the question in discussions with W.K. Clifford during the 1873 meeeting of the British Association for the Advancement of Science (the same at which F. Klein and W.K. Clifford got into contact).[12] Henceforth he considered it as a serious problem of empirical science. He himself was active in systematic studies of the parallax of fixed stars in large observational programs. In a paper presented to the Royal Institution London in February 1881 he discussed the present status of parallax measurements (Ball 1881). Among others he discussed the problems arising from the proper motions of the "fixed" stars. For Bessel's target star, 61 Cygni e.g., it was already $5''$ per year and resulted in specific problems for follow up observations of the same target by other astronomers. F. G. Struve made new observations in 1853 and Ball's own predecessor at the Irish observatory at Dunsink again in 1878. Already in 1853 Cygni 61 had "moved considerably" with respect to the comparison stars. Therefore different comparison stars for the parallax measurements were used. Ball explained the arising differences of parallax determinations as a consequence of different distances of the comparison stars. Another project at Dunsink which Ball reported upon was a systematic check of 300 stars for easily detectable parallaxes, i.e., above $0.3''$. Only one star of the whole collection was found positive, and Ball concluded that most of the observed stars were too far away to find parallaxes with the present techniques, which were still essentially the same as Bessel's and Struve's. So Ball was perfectly well acquainted with the details of practical parallax measurement.

His discussion of the "nature of space" at the end of his paper showed that Ball had carefully reflected upon the question. He remarked that in hyperbolic space the "observed parallax" is smaller than the "true parallax", while in elliptic space the converse holds. By *true* parallax ψ he meant the angle under which the radius $r = \overline{AB}$ of the earth orbit appears for an observer

[10] Others, like S. Newcomb, explored non-Euclidean geometry in a more speculative way as a playground for modified ether theories (hint due to M. Epple during the OW conference).

[11] (Schemmel 2005)

[12] According to Beichler, James, "Twist til' we tear the house down", *Journal of Paraphysics* 1(1999). [http://members.aol.com/jebco1st/Paraphysics/twist1.htm, visited Oct. 3., 2005].

at a star C, $\psi = \angle ACB$, while the *observed* parallax p was half the angle difference of the yearly change of the angle distance between C and a close by comparison star C' (which ideally should have less annual parallax motion than C, i.e. should be of much larger distance), $p = \frac{1}{2}|\angle CAC' - \angle CBC'|$. Clearly the strongly idealizing assumptions of Lobachevsky were a far cry from what Ball as an experienced astronomer (40 years later) considered as empirical data. He therefore refrained from a definite answer of how to determine a hypothetical space curvature from them. He indicated only quite generally that "it would seem" that it can only be "elicited by observations of the same kind as those whch are made use of in parallax measurements" (Ball 1881, 519).

A more definite answer was given two decades later by K. Schwarzschild in a talk given during the 1900 meeting of the *Astronomische Gesellschaft* at Heidelberg. It has been discussed at different places how Schwarzschild determined bounds for the curvature of astronomical space from parallax and other data. [13] Schwarzschild knew very well the problem of the complicated interrelationship of "true" parallax, observed parallax, and angle of parallelity (for hyperbolic geometry) and wanted to include the case of positive curvature explicitly. He started his consideration with a simplification similar to the one of Lobachevsky and assumed that one of the two stars, C', needed for a parallax observation as comparison starwas in the plane of the earth orbit, the other one, C, approximately orthogonal to it. Then the geometrical description of the "observed" parallax was greatly simplified and allowed to derive a relationship between the radius of the earth orbit a, the distance d to the star C, the radius R of space curvature R (Schwarzschild's terminology, identical with Gauss characteristic "constant" K for the hyperbolic case), and the observed parallax ψ:

$$\begin{aligned} \sinh\frac{d}{R} &= \frac{a}{\sqrt{\psi^2 R^2 - a^2}} \quad \text{for} \quad \kappa < 0, \\ \sin\frac{d}{R} &= \frac{a}{\sqrt{\psi^2 R^2 + a^2}} \quad \text{for} \quad \kappa > 0 \end{aligned}$$

In the hyperbolic case, the reality condition $\psi^2 R^2 - a^2 > 0$ led to a constraint for the radius of curvature $R > \frac{a}{\psi}$. Schwarzschild now argued (differently to Lobachevsky) with a kind of isotropy argument for parallaxes, which made it unnecessary to refer to direct observations in the idealized geometrical constellations of the stars C and C':

> Because it is sure that many stars do not have a parallax of $0.05''$ the minimal value [for ψ, E.S.] is below $0.05''$, and a lower bound for the curvature radius of the hyperbolic space follows ... (Schwarzschild 1900, 341).

[13] Cf. (Schemmel 2005)

He thus arrived for the hyperbolic case at

$$R > 4 \cdot 10^6 \, AU \approx 60 \, LY, \tag{5}$$

an order of magnitude above Lobachevsky's value (due to his sharper parallax values) and with a convincing derivation without *logical* dependence on a precarious simplifying assumption.

For the case of positive curvature Schwarzschild gave an ingenious estimation of a lower bound for the curvature radius. He assumed his teacher Seeliger's view of an "island of stars" with an estimated number of visible stars $N \leq 4 \cdot 10^7$ contained in a ball of radius of $10^8 \, AU$ (order of magnitude), which lie in a "wider relatively empty space" (in later terms "our" galaxy, which was considerably underestimated by Seeliger in cardinal number and geometric extension). In 1900 about 100 stars were known with a parallax $> 0.1''$. For small radius of curvature R these nearest stars with measurable parallax would fill most of the space of the 3-sphere, while most of the other 10^7 ones had to concentrate closely to the antipodal point of the earth. In order to make an equal distribution of the 10^7 stars possible, Schwarzschild calculated that the radius of curvature should at least be

$$R \approx 10^8 AU \approx 10^3 LY. \tag{6}$$

He hoped that more knowledge might be obtained from studying star magnitudes in greater detail.

Schwarzschild was not only the first one among astronomers to derive convincing quantitative bounds for the curvature of astronomical space, he was, moreover, quite open his a personal preference for the hypothesis of positive space curvature. Like Clifford he remarked that the finite extension of space in the case of $\kappa > 0$ gives "some comfort" to the mind. In this case Seeliger's star island would no longer stand isolated in an infinite, otherwise empty space, but space would be homogoneously filled with stars and would be geometrically adapted to perceptible matter. Space itself would be "finite and completely or nearly filled by this star system" (Schwarzschild 1900, 342)

(4) Outlook on the first relativistic cosmological models: Einstein, de Sitter, and Weyl (1916 – 1923, short and sketchy)

The next, by far most radical, turn for modern cosmology came with Albert Einstein's general theory of relativity (GRT) in 1915 and with the first cosmological models constructed on its base during the next few years. GRT changed the role of curvature completely. It was now no longer nearly exclusively accessible by triangle data in the sense of Lobachevsky or Gauss and linked to mass distribution only by heuristic arguments like in Schwarzschild's inventive discussion of 1900. From now on curvature was insolubly linked to the mass-energy-stress tensor T on the right hand side of the Einstein equation:

$$Ric - \frac{1}{2}\bar{R} = 8\pi\,[\frac{G}{c^4}]\;T \qquad (7)$$

(g Lorentz type metric on the space-time manifold, Ric its Ricci tensor, $\bar{R}$ scalar curvature, G Newton's gravitational constant, c velocity of light).

In the new physical setting of GRT, mass and energy density became the most important determinative element for space-time curvature. But already six years after the first presentation of a relativistic cosmological model by Einstein, Weyl came along with the surprising observation that a constant characterizing cosmological redshift (at that time still quite hypothetical, at the end of the 1920s clarified as the "Hubble constant") entered the determination of space curvature just as well. Here I can give only a very short and sketchy first glance at this turn crucial for empirical studies of space curvature in the 20th century.

The first step into general relativistic cosmological models seems to have been made by Karl Schwarzschild. In one of his last letters before his premature death, written on Feb. 6, 1916, he informed Albert Einstein about steps towards transferring his older astronomical-cosmological considerations of positively curved spaces to the new general relativistic frame. Schwarzschild proposed to consider positively curved space sections, putting the "entire universe under uniform pressure".[14] Here Schwarzschild indicated a first step towards what a year later became Einstein's first general relativistic cosmological model, the *Einstein cylinder world* (Einstein 1917)

$$\mathbb{R} \times S^3 \quad \text{with metric} \;\; ds^2 = -dt^2 + d\sigma_\kappa^2\,,$$

$d\sigma_{\kappa^2}$ the metric of constant sectional curvature κ on the 3-sphere.[15]

From now on mass energy density ρ was linked to the curvature of space-time models of relativistic cosmology, in Einstein's model by

$$\kappa = \frac{1}{R^2} = \frac{4\pi G}{c^4}\rho \qquad (8)$$

(G Newton's gravitational constant, c velocity of light). Before he arrived at this relation, Einstein had introduced his famous ad-hoc "cosmological term" Λg with a new miraculous constant Λ on the left hand side of his equation (7) to get rid of the negative pressure already observed by Schwarzschild.

At the time it was close to impossible to make a reasonable guess of cosmic mass energy density. By an evaluation of available counts aof stars and nebulae and very rough estimations of their mean distances, Einstein

[14] Cf. (Schemmel 2005)

[15] Another contribution by Schwarzschild to this solution derived from the interior solutioin to his sperical symmetric solution of the Einstein equation (hint due to H. Goenner during the OW conference).

came to a provisional estimation of $\rho \sim 10^{-22} g\, cm^{-3}$. Because of its great unsecurity, Einstein did not publish the estimation but wrote about it only in a letter to M. Besso (Dec 1916) or mentioned it in conversations.[16] Evaluated by (8) the estimation led to

$$R \sim 10^7\, LY\ . \tag{9}$$

Even though the mass density estimations were so precarious that Einstein even did not want to publish them, they contained an impressive result. In spite of the insecurity of several orders of magnitude, it indicated that the curvature bounds gained by optical measurements in the 19th century (or even Schwarzschild's first estimation of curvature on the basis of equal distribution of stars) were much lower than those one had to expect in relativistic cosmological models.

Things became even more interesting, although also more involved, with W. de Sitter's invention of the second model of general relativistic cosmology. To describe it shortly in terms given slightly later by F. Klein and H. Weyl, de Sitter's model operated on a Lorentzian manifold H, the *de Sitter hyperboloid*, defined in 5-dimensional Minkowski space $\mathbb{M}^{(4,1)}$ by the equation

$$\sum_{i=1}^{4} x_i^2 - x_0^2 = a^2.$$

Here a, the *de Sitter radius*, determined the scalar curvature $\bar{R}_H$ of the whole manifold, which turned out to be constant,

$$\bar{R}_H = \frac{12}{a^2}\,.$$

It was possible to choose a family of time-like curves and orthogonal to it a family of spacelike (locally defined) hypersurfaces in order to specify a cosmological model with well defined geometry inside the de Sitter hyperboloid. Here quite different choices were possible and led to strictly different cosmologies. We cannot discuss these problems here, but rather proceed to one of the possibilities studied by H. Weyl. [17] After he had freed himself from following Einstein's cosmological considerations quite closely, Weyl studied a model of diverging time-like flow lines on H which were very naturally given by a geometrical construction on the hyperboloid. The orthogonal spacelike sections to the flow lines had constant positive curvature (Weyl 1923, §39),

$$\kappa = \frac{1}{a^2}\,,$$

and constituted segments of spherical spaces of the de Sitter radius a.

[16] Published in (Moszkowski 1921) (hint due to H. Goenner during the conference).

[17] For the broader context see (Goenner 2001, Bergia 1999)

Moreover, an intriguing new feature came into sight. The flow lines could naturally be interpreted as idealized trajectories of the mean motion of stars or nebulae (the latter were identified as galaxies only during the later 1920s). Their divergence led to the model phenomenon of a systematic redshift expected for light emitted from very distant sources. If one described the change of wavelenghts λ_0 and λ_1 from source to observer by the parameter z with

$$\lambda_1 = (1+z)\lambda_0 \ ,$$

z was governed by the de Sitter radius a. Weyl calculated the dependence of the redshift z on the distance d between source and observer. For the sake of simplicity we give here only the infinitesimal linearization of Weyl's formula. It was

$$z = \frac{1}{a}\, d \quad \text{(Weyl 1923, 323)} \quad . \tag{10}$$

By purely mathematical reasons (linear term of Taylor development) the redshift increases linearly for "small" cosmic distances. After the empirical establishment of such a linear relationship in the late 1920s by E. Hubble, the linear factor was called the *Hubble constant* H. In Weyl's approach it was directly linked to the de Sitter radius of the model, $H = a^{-1} = R^{-1}$. By Weyl's reading of the de Sitter model, there arose another *empirical determinant* for the curvature radius R of cosmic geometry in addition to mass energy density (which did not play any role in the de Sitter model). If his model approach was of any use, the radius could be determined by measuring the constant H of the infinitesimal linearization and by taking its reciprocal (notation H of course not yet used by Weyl).

At the time when Weyl made these remarks, the first results for the redshift of nebulae had just been obtained. Some nebulae, like the one in Andromeda, M 31 had blueshift, $z < 0$, others were redshifted, $z > 0$. While about 1920 de Sitter expected a predominantly blueward shift, the US astronomers V. Slipher, H. (Harlow) and M. (Martha) Shapley drew the conclusion from their observations that most nebulae were shifted to the red. A systematic plot of the distance redshift relation was not yet possible, as distances of nebulae were still very difficult to estimate. But when Weyl wrote the appendix to his 5th edition of *Raum - Zeit - Materie* in late 1922, he could refer to an article by the two Sliphers which listed the result of redshift observations of the large US observatories (mostly Slipher's). In a collection of 25 nebulae, 22 were redshifted and only 3 were shifted blueward (Shapley 1919, table V). The redshifts were given in terms of "flight" velocities and had the orders of magnitude 10^2 or 10^3 $km\, s^{-1}$.

For a first estimation of distances Weyl could rely on another source. The young Swedish astronomer Knut Lundmark had done research at the large US telescopes and found in 1919 that M 31, the Andromeda nebula, was another distant star "island" (galaxy). He used supernovae observations to estimate the distance of it. In the *Astronomische Nachrichten*, easily accessible to H. Weyl, Lundmark and other authors (Haag, Doig e.a.) gave the first estimations of distances of some of the extragalactic nebulae (Lundmark 1919). This made it possible for Weyl to assume for the nebulae of Shapley's table distances "between 1 to 10 lightyears". He thus arrived at an estimation of the de Sitter radius

$$R = H^{-1} \sim 10^9 \ LY \quad \text{(Weyl 1923, app. III)} \ . \tag{11}$$

This was a splendid estimation. After E. Hubble had made his improved and systematic measurements of distances and redshifts of galaxies, he arrived at a redshift constant $H \approx 5 \cdot 10^{-10} \ Y^{-1}$ ($\approx 500 \ km \, s^{-1} \, Mpc^{-1}$), $c \, H^{-1} \approx 2 \cdot 10^9 \ LY$. Thus Hubble's new precision measurements agreed surprisingly well with Weyl's rough estimation six years earlier.[18]

Final remarks

In 1923 there were now two methods for determining the curvature of the spatial sections of cosmological models, which relied on rather different principles, the more direct one from mass energy estimations and the Einstein equation, and an indirect one from cosmological redshift data and specific model assumptions for the redshift. The two methods led to first estimations which were both 5 – 7 orders of magnitude above the highest bounds derived by the methods of the 19th century and differed among each other "only" by two orders. As mass density estimations were highly precarious at the time, and remained so for decades to come, the difference was not considered to be a defect. In fact, the redshift measurements became much more precise (up to a systematic error mentioned in footnote (18)) already at the end of the decade and remained the most important and best measured empirical parameter of relativistic cosmology up to the 1960s. The type of models, on the other hand, changed. They were soon enriched by models with expanding space sections (Friedmann - Lemaître models as a physically preferred class inside the highly symmetric Robertson-Walker cosmologies). They started to play the main role from the turn to the 1930s onward.

It may be useful, however, to remember that not all of the protagonists of the first relativistic cosmological models were convinced that expansion ought to be considered the "real cause" of cosmological redshift. Most markedly, Hermann Weyl whose diverging flow line model in the de Sitter manifold played a crucial role for the spread and rise of the later expanding world approaches, insisted on the essentially mathematical model character of the "space kinematic" description of cosmological redshift, which "must on any account be examined as a possibility" (Weyl 1930, 301). Confronted with

[18] Because of a systematic error in distant measurements of Cepheids which were instrumental for the whole procedure, Hubble's value for H later turned out to be still an order of magnitude too large. It was corrected by W. Baaade and A. Sandage in the 1950s. Today's values are $H \approx 70 \ km \, s^{-1} \, Mpc^{-1}$, i.e., $c \, H^{-1} \approx 1,4 \cdot 10^{10} \ LY$ (±10%.)

the turn of many of his colleagues toward the expanding world models he warned, however:

> ...It is not my opinion that we can vouch for the correctness of the "geometrical" explanation which relativistic cosmology offers for this strange phenomeneon [cosmological redshift, E.S.] with any amount of certainty at this time. Perhaps it will have to be interpreted in a more physical manner, in correspondence with the ideas of F. Zwicky. (Weyl 1930, 300f.)

Zwicky had just proposed to consider a field theoretic explanation of the energy loss of photons on long range passages over cosmic distances. His attempts to calculate such effects were surely premature. But is remains to be seen whether the paradigm of a "real expansion" of space sections, which turned into the completely dominant one in the later part of the 20th century will stay so in the next one.

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