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The Conversion of St. John: A Case Study on the Interplay of Theory and Experiment

The Argument

Gravitational redshift of spectral lines as one of the three early-known experimental implications of Einstein's general theory of relativity and gravitation was intensively searched for by researchers all over the world, but around 1920 most of the contemporary evidence in the sun's Fraunhofer-spectrum conflicted with the predictions of relativity theory.

In 1923 the American astrophysicist Charles Edward St. John announced that his own solar spectroscopic data would force him to retreat from his former skepticism concerning the existence of gravitational redshift. This statement was at the time widely interpreted by scientists and journalists alike as the open confession of a rapid conversion of one of the few remaining serious scientific opponents of Einstein's theory.

This paper demonstrates that this illusion of a sudden "Gestalt switch" in St. John's evaluation of data can be dissolved by a careful step-by-step account of St. John's research practice between 1917 and 1923. After a fine-grained diachronic report of the development of St. John's interpretation of his and others' data, the second part of the paper consists in a systematic analysis of the heuristics and arguments used by St. John pro and contra gravitational redshift.

Introduction

This paper is a case study of an episode in the history of experiments on the redshift of spectral lines and the evolution of their theoretical interpretation. My plan is to present a diachronically organized analysis of publications and as yet unpublished letters by and concerning Charles Edward St. John, an important American astrophysicist, who was a major influence in the change of opinion as to whether the gravitational redshift predicted by Einstein's general theory of relativity (in what follows often abbreviated GRS and GTR, respectively) can be detected in spectroscopic data of the sun's Fraunhofer spectrum.[1] During the 1920s, more and

[1] For reviews on this issue, see in particular Earman and Glymour 1980 (theory) and Forbes 1961 (experiment); see also Hentschel 1990a–b, 1992, 1993.

more experts were convinced that the relativistic effect could be found in data that had previously been considered quite inconclusive. Why did specifically this explanation of the minute shifts of spectral lines, known since Rowland's and Jewell's high resolution studies of the sun's spectrum in the 1890s, convince the contemporary experts, while alternative explanations offered by various scientists were either rejected (W. H. Julius' theory of anomalous dispersion or Humphrey's and Duffield's pressure effects) or merged into the relativistic explanation (J. Evershed's hypothesis of nonradial convection currents)? And which arguments convinced those members of the astrophysical community in particular who had held antirelativistic prejudices? In what sense did these arguments differ from those which only five years earlier had supported the view that the relativistic line shift could *not* be found in the data? Can any subcutaneous continuities be traced beyond the obvious differences in argumentation? How did the public accounts of redshift from 1920 to 1925 change, and how did they relate to the scientific reports of the time? Extensive documentation in answer to all of these questions is given in the major part of this paper, while a systematic analysis is attempted in the final section.

Although I doubt that generalizations can be made about the complex relations between theory and experiment without prior specification of the context in which both fields of scientific endeavor are situated, this paper should nevertheless be understood as a case study aiming at the explication and clarification of steps experimental scientists typically take to harmonize theory and experiment *in a situation similar to the one St. John faced around 1917*: Well-established experimental practice was threatened by a new theoretical prediction claiming that all the former data in fact included a new, hitherto unrecognized effect. How does the practitioner, interested in the "cleanness of his data" — necessary to test the high-precision predictions of other theories — react? How does he stabilize the intricate network of hypotheses and assumptions stemming from different theories, of practical routines (in turn presupposing certain background theories about the instruments and the processes taking place in them), and of empirical data (always selected with a specific aim in mind)? In this paper I try to show that we can very well reveal the heuristics pursued by St. John in this situation, thereby removing the aura of irrational "conversion" and showing that we can understand his 1923 decision in terms of the rationality of his experimental practice.

Charles Edward St. John (1857–1935),[2] the long-time collaborator of G. E. Hale at the Mount Wilson Solar Observatory of the Carnegie Foundation of

[2] For biographical information on St. John, see Abbot 1935, Joy 1935, Adams 1937, Marsden 1981, and further literature cited there. An extensive but not complete bibliography of St. John's papers is given in Adams 1937, 298–304. See also the obituaries in the *New York Times*, 27 April 1935; St. John 1935, and the two obituaries in newspapers of Oberlin, Ohio: *The Oberlin Review*, 30 April 1935, p. 3, and *The Oberlin News-Tribune*, 30 April 1935, p. 1. (St. John had been professor of physics and astronomy at Oberlin College between 1897 and 1908, and for some time also dean of the college.) Copies of both articles were kindly sent to me by the librarians of Oberlin College, for whose help I am very grateful.

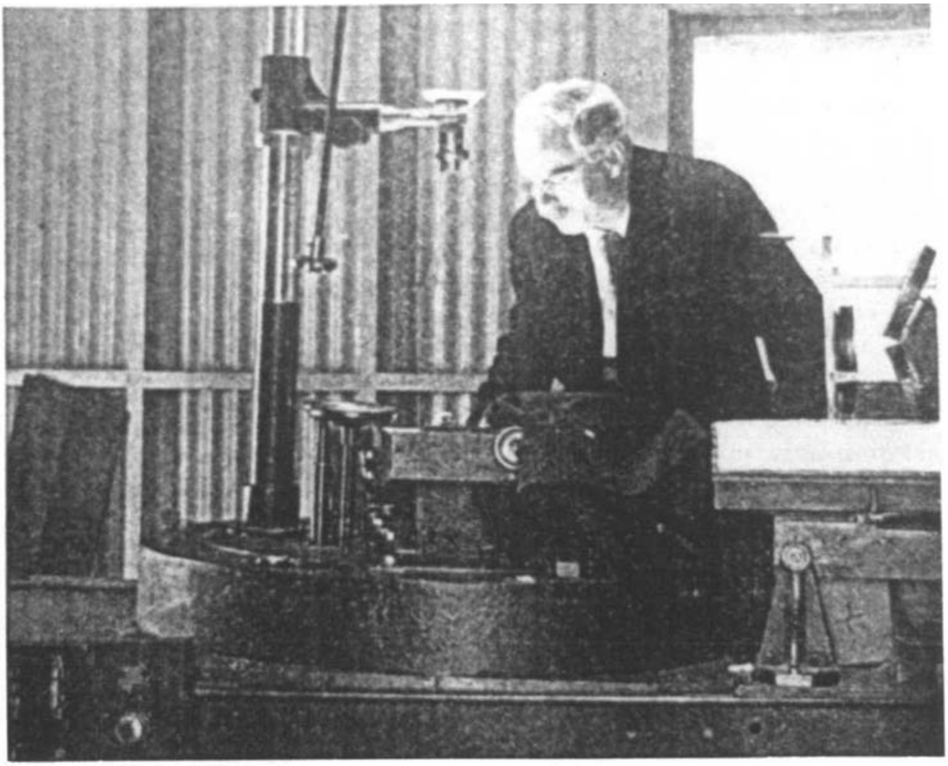

Figure 1. St. John at work; from Joy 1935.

Washington near Pasadena, California, was called to Mount Wilson by Hale in 1908 after the observatory had been erected in 1904[3] (see figure 1). This was an unusual chance for St. John who had previously taught for a number of years and was already nearly fifty when he joined the staff at Mount Wilson.

The case of St. John is ideally suited to the pursuit of answers to the questions raised above, because he did *not* belong to those enthusiastic Einstein fans who were convinced of his GTR from the very beginning (see Miller's letter, quoted on p. 152). Incidentally, St. John shared this skepticism of the viability of GTR not only with the overwhelming majority of experimentalists but also with important theoretical physicists such as Max Planck and Max von Laue, who were belatedly — and then only reluctantly — convinced of Einstein's general theory of relativity and gravitation. First of all, the mathematical apparatus involved (tensor calculus and differential geometry) had not been used before in the realm of physics, and only a few mathematically well versed theoretical physicists were able to work with it. (Even Einstein needed the assistance of his mathematician friend Marcel

[3] For the early history of the Mount Wilson Observatory and its staff, see, e.g., Adams 1947, 1954, 1955.

Grossmann in the first years of the construction of the new theory, 1913–15.) Not only was the formal apparatus radically new, but there was an even more abrupt break with the traditional principles of fundamental physics. The GTR involved a new relationship between geometry and physics, an interaction between matter and space-time structure, which formerly had been conceived of as being as distinct as container and content, and a dynamic conception of the universe as a whole — which formerly had mostly been considered a static entity — among other revolutionary changes.[4] All of these breaks with traditional principles were deemed acceptable to the scientific community only if they were really necessary — that meant: if it could be demonstrated that certain experimental data could be satisfactorily explained only by the GTR and not by any contemporary alternatives that were theoretically comparably convincing.

So the announcement of a hypothetical effect of gradients of a gravitational field on spectra, made by Einstein from 1907 on (see Einstein 1907, 1911, 1916, 1917; cf. Earman and Glymour 1980, Hentschel 1990b), was not quite welcomed by St. John — who was in this respect representative of most other astronomers and astrophysicists.[5] Therefore, when in 1923 he finally overcame his initial skepticism and his antirelativistic preconceptions (cf. p. 152), this can appropriately be called his "conversion."

The period in which St. John's change of opinion took place (1921–24) can be fairly well documented by a close analysis of all the papers he wrote in this period,[6] in conjunction with his letters to colleagues preserved in other astronomers' estates. St. John's oeuvre has not yet been closely studied by historians of science, and the resources in various archives containing material by and about him seem to have remained untouched up to now.

Because of the rather extensive documentation from this period of St. John's work, we must confine our analysis of his conversion to the theory of relativity to the second half of the year 1923, as will be shown in the next section. St. John's other activities in the period 1917–24 cannot be commented on here at length, although there are interesting interdependencies between his work on the GRS and, say, his refutation of Julius (around 1915), his studies on the Evershed effect and the pole effect just after their discoveries (in 1909 and 1913, respectively), and his work on the revision of Rowland's tables for the solar spectrum (lasting until 1928). Here I cannot give more than a summary of his and his colleagues' activities in the relevant period (see table 1).

[4] For more about this topos of "revolution" see Hentschel 1990a, sec. 2.4.

[5] In Germany, Erwin F. Freundlich was the first to take Einstein's predictions seriously; in the Netherlands it was Willem de Sitter; and in England, Arthur Stanley Eddington.

[6] Although up to now I have not been able to find St. John's private estate.

Table 1. Activities of St. John and his colleagues at Mount Wilson, 1916–26

Year	#	St. John's activities concerning spectral shifts	St. John's other activities [and collaborators]	His colleagues' activities
1913	12	Study of possible radial motion in sunspots as suggested by Evershed, causing Doppler shifts at the sun's limb.	Study of the distribution of elements in the sun's atmosphere and their level-dependence as a precondition to understanding their velocity distribution in solar vortices. [With Miss Ware]: Study of desirable data for standards of wavelengths.	Hale et al.: Study of the magnetic field in the sun, esp. in sunspots. King: Temperature variation of electric furnace spectra.
1914	13	Further study of Doppler shifts due to radial movement in sunspots. [With Babcock]: Start of work on the pole effect, a systematic displacement of arc lines.	Continuation of study of distribution of elements, using flash spectra. Start of systematic enquiry of the sun's rotation, leading to mean linear velocities f 1.93 km/s [1914–21] with small and irregular variations each year and level dependence.	Continuation of above work by Hale et al. King and Koch: microphotometric studies of laboratory spectra.
1915	14	[With Babcock]: Continuation of his studies of *the variability of spectrum* lines in the iron arc.	Continuation of his work on standards of solar wavelengths.	Continuation of work on the sun's magnetic field.
1916	15	Check whether the relative positions of neighboring Fraunhofer lines are systematically affected by anomalous dispersion. Negative results. Refutation of Julius' theory.		Ellerman: Map of sunspot spectra. King: Experiments on anomalous dispersion with electric furnace.
1917	16	Determination of the fundamental wavelengths of solar and standard lines with special reference to a possible relativity effect, based on lines in the nitrogen (cyanogen) band at 3883Å, because of its pressure-independence. Freedom from Doppler effect by comparison with	(i) [With Miss Ware]: Further investigation of the errors of measurements in pairs of closely adjacent lines (overseparation ~0.013Å) for fixing the limits of reliability of data and determining the systematic error. (ii) [With Misses Ware and Miller]: Study of	King: Photographs of electric furnace spectra, esp. of Fe, Cr, and Ti, also of the 3883Å band, and test of their temp.-dependence. Babcock: Redetermination of pressure effect for iron. Adams: Stellar spectroscopy.

Table 1. (continued)

Year	#	St. John's activities concerning spectral shifts	St. John's other activities [and collaborators]	His colleagues' activities
		results at solar limb. Means: at center zero; at the limb 0.0018Å toward the red, i.e., within the limit of error. No evidence for GRS of the order of 0.008Å as required by GTR.	solar rotation, esp. the influence of haze and scattered light. (iii) [With Babcock]: Development of the Pfund arc for standard purposes, free from the pole effect. Establishment of secondary standards by precision measurement of 506 iron lines.	Anderson: Stark effect. Nicholson, Joy, Ellerman, et al.: Solar observations and photography. Seares and Van Maanen: Sun's magnetic field.
1918	17	Publication of his results of 1917.	(i) [With Miss Ware]: Spectrographic study of solar rotation, esp. comparison of rotational velocities of high-level H and K lines of Ca with low-level lines of the cyanogen band. (ii) Determination of solar and laboratory wavelengths of iron lines.	War research for army and navy. Expedition to solar eclipse in Wyoming. Adams: Stellar spectroscopy. King: Work with electric furnace. Nicholson and Joy: Solar observations. Van Maanen et al.: Sun's magnetic field.
1919	18	(i) Study of displacements in the spectrum of Venus to test a hypothesis by Evershed that the wavelengths in light reflected by Venus vary with the relative positions of Venus, the sun, and the earth: minor residuals of the order 0.004Å observed. (ii) Check of the variation of wavelengths with the altitude of the sun, as suggested by Perot: no change observed.	Continuation of his determinations of solar wavelengths [with Babcock] in the iron-arc spectrum over the region 3370–6750Å. Continuation of his spectrographic investigations on the solar rotation [with Miss Ware]: Now, the center of the sun's disk is observed simultaneously with the two limbs. → Evidence for local disturbances in the reversing layer.	Continuation of above work; King: Study of the Zeeman effect in a small furnace between the poles of a magnet. Anderson and Takamine: Study of the effect of an electric field on radiation with a new electromagnet. Babcock: Check of the constancy of wavelength in mixed arcs, of the pressure and pole effects with the interferometer.
1920	19	Negative evidence for Einstein's GRS in data of the magnesium triple in the green yielding practi-	[With Miss Ware]: Continuation of studies on solar rotation with minor technical improvements.	Continuation of above work. Hubble, who joined the staff in Sept., focuses on investigations of nebu-

Table 1. (continued)

Year	#	St. John's activities concerning spectral shifts	St. John's other activities [and collaborators]	His colleagues' activities
		cally zero shifts as compared to the relativity deduction of 0.011Å for these lines. Announcement of an extensive investigation about lines, whose behavior in the solar spectrum is exceptional, depending upon level, intensity, wavelength or molecular weight, also including the cyanogen band.	Development of hydrodynamics of sunspot vortices with complicated inward and outward flows of the spot vapors as inferred from observed Doppler shifts. [With Babcock]: Preparation of a table of solar wavelengths based on the international system through comparison of simultaneous spectrograms of the sun and the iron arc.	lae. Michelson, who was appointed research associate in 1919, conducts investigations on the application of interference methods in astronomy (e.g., double stars previously unresolved) and on the velocity of light. Ellerman: Photographic map of the sunspot spectrum, covering 3900–6600Å on a scale of 1 cm/Å.
1921	20	Review of current knowledge about redshifts. Beginning of an extensive program on sun-arc displacements including observations at center and limb and covering the widest possible range in wavelength and line intensity, as a reliable body for statistical discussions. Continuation of above work, including the spectrum of Venus, solar rotation, and the wavelengths of solar and terrestrial lines.	[With Babcock]: Publication of 1026 iron-arc lines, determined by comparing photographs taken with 5 different gratings and with four pairs of interferometer plates. Claimed accuracy: <0.001 Å. [With Nicholson]: Prove that oxygen and water vapor are absent from the spectrum of Venus; discrepancies between Venus and sky light due to atmospheric refraction.	Continuation of above work. Babcock & Anderson: Interferometer study of pressure displacement of selected iron lines, free from effects of thermal or mechanical disturbances. Anderson: Constancy of the sun's wavelengths from the zenith to 30°. Michelson: New determination of the velocity of light between Mt. Wilson and Mt. Antonio, and search for evidence of relative motion of the ether. Miller: Repeats the Michelson-Morley experiment on Mt. Wilson.
1922	21	Continuation of former work [with Babcock] on the accurate determination of wavelengths of solar and terrestrial lines, together with an extensive study of the causes giving	[With Babcock et al.]: Selection of 300 iron-lines as tertiary standards after an elimination of those recognized as unstable and after cross-checking with 6 other	Continuation of above work, esp. on the Zeeman effect in sunspot spectra by Hale, Nicholson, & Ellerman. Pease: Measurement of stellar diameters with the

Table 1. (continued)

Year	#	St. John's activities concerning spectral shifts	St. John's other activities [and collaborators]	His colleagues' activities
		rise to displacements of lines in the sun, such as general and local convection, lateral drifts, pressure and possible effects from density distribution, and irregular refraction and dispersion.	observers of the IAU in May 1922. [With Babcock]: Investigation of center-limb shifts, using both grating spectrographs and an interferometer, covering 4900–6600Å. Check that amount of displacement is the same at the north and the south limbs of the sun. Check of the constancy of atmospheric lines. Continuation of the study on solar rotation [with Miss Ware] and of the spectrum of Venus [with Nicholson].	20-foot Michelson interferometer. Russell, King, & Noyes: Tests of Saha's theory of ionization in solar and stellar atmospheres. Babcock: Interferometric study of auroral spectrum. Miller: Further observations on ether-drift effects with reduced residual effects (ca. 10%).
1922–1923	22	Continuation of above work with grating spectrographs on the wavelengths upon the international system of several hundred lines in the solar spectrum in the region 3650–6750Å. Very close agreement with Babcock's results via interferometer in the red region of 4500Å → "full confidence may be placed in the values of the entire list."	Investigation of the pressure in the sun's reversing layer: estimated to be below one atmosphere and probably in the neighborhood of zero. From the observed increase of ionization over faculae and Saha's theory, a partial pressure somewhat less than 10^{-3} is indicated. [With Babcock]: Further investigation of center-limb shifts; St. John uses the 150-foot tower telescope, Babcock the Snow telescope, the 30-foot spectrograph, and an interferometer. For 5400Å, a mean increase of 0.0054Å is found at the sun's limb as compared with the center, slowly increasing with the wavelength and dependent upon the intensity of the line.	Resignation of Hale as director in July 1922; Continuation of above work. Michelson: Determinations of the velocity of light by a combination of the methods of Fizeau and Foucault; new checks of the effect of the earth's rotation on the velocity of light.

Table 1. (continued)

Year	#	St. John's activities concerning spectral shifts	St. John's other activities [and collaborators]	His colleagues' activities
1923–1924	23	Confirmation of the gravitational shift of the sun's spectral lines based on over 330 iron lines, showing the combination of relativity displacements with those due to convection currents in the solar atmosphere, and explaining many of the difficulties encountered by observers in this field.	[With Miss Ware]: Continuation of spectroscopic investigations of the rotation of the sun, with no evidence of change over the results for previous years. [With Babcock]: Further investigations on pressure in the sun's atmosphere based on additional measurements of iron lines and new laboratory data on pressure displacements, confirming the conclusion that the pressures in the lower few hundred km of the reversing layer must be very low (0.13 ± 0.06 atm)	Continuation of above work. King: Extensive studies of laboratory spectra in the arc, spark and electric furnace; classification of spectral lines. Babcock: Special investigations of the Zeeman effect. Lectures by H. N. Russell and Ehrenfest on the interpretation of spectra and quantum theory.
1924–1925	24	Confirmation of the gravitational shift in 431 lines of the cyanogen band between 3729Å and 3883Å, out of which 184 lines were selected by Birge with regard to series relationship, and then compared with arc wavelengths as given by Uhler and Patterson. Close agreement of the results with predictions of Einstein's GTR is found.	[With Miss Ware]: Continuation of spectroscopic investigations of the rotation of the sun, with a mean of 1.936 km/s and a decrease of 1.7% during the last 6 years. [With Babcock]: Compilation of a list of about 2500 solar lines, referred to the standards adopted by the IAU in 1922, and construction of a reduction curve which enables a preliminary revision of Rowland's Table of Solar Wavelengths.	Continuation of above work. Establishment of a new "Solar Laboratory" by Hale, donated to the Carnegie Institution in 1925. Closer association with Caltech. Visits by Eddington, Russell, and Jeans. Russell studies the relative intensities of spectral lines belonging to multiple groups. Adams: Confirmation of the GRS in the spectrum of the companion of Sirius, a "white dwarf" with a density about 50,000 times that of water and a correspondingly large GRS. Babcock: Interferometric determination of displacements of iron lines under small changes of pressure, showing a proportionality between pressure and displacement between 0 and 1 atm.

Table 1. (continued)

Year	#	St. John's activities concerning spectral shifts	St. John's other activities [and collaborators]	His colleagues' activities
1925–1926	25		[With Miss Moore and E.F. Adams]: Focus on a preliminary revision of Rowland's Table of Solar Wavelengths, involving a determination of wavelengths on the international system, many new identifications (chemical origin), temperature and pressure classifications, excitation potentials, degree of ionization, with special focus on the red and infrared portions of the spectrum. [With Miss Ware]: Continuation of spectroscopic investigations of the rotation of the sun.	Continuation of above work. King: Further investigations on high-current arcs in air and in vacuum and about the classification of lines according to widening and shifts under different sources of excitation. Babcock: Precision measurements of the green aurora line.

Source: *Yearbook of the Carnegie Institution of Washington*, in which the annual reports of the Mount Wilson Solar Observatory (from 1918 known only as Mount Wilson Observatory) are given. Column 2 gives the volume number of the *Yearbook*.

As shown in table 1, St. John had already dealt with some other possibly disturbing influences of the precise position of spectral lines — e.g., Doppler effects due to radial and nonradial motions in sunspots[7] — on the so-called pole effect,[8] and on Julius' claims about the importance of anomalous dispersion for Fraunhofer lines in the sun's spectrum (see, e.g., Julius 1910, 1914–17, 1924; cf. Hentschel 1990c). With all these physical effects, St. John's main concern has always been to eliminate the effect, either by showing that it is irrelevant for the sun's physics or by understanding the physical mechanisms at work and thereby being able to correct the "raw data" for the then "known" effect (see, e.g., St. John 1910b, 1910–11, 1913a, 1914a versus Evershed; St. John 1915a–b, 1916b–d, 1918g and St. John and Ware 1916a–c versus Julius; and St. John and Babcock 1915a–b,

[7] As suggested by Evershed 1909–10.
[8] Discovered by Goos (in 1912–13), who had realized that the actual parameters of the electric arc used in the laboratory to produce a spectrum and the choice of the position of the slit with respect to the two poles were of influence in the precision measurement of laboratory emission wavelengths.

1917 about the pole effect). So when he realized that Einstein's GTR also implied a small shift of spectral lines, he immediately pursued the same strategy once again: to check whether GRS does exist and whether hypostazing it is consistent with the data at hand; as with the other effects, his initial attitude toward the new effect was skeptical, to say the least. In the next section we will see that from 1917 to 1922 he remained skeptical regarding the existence of GRS.

It was not before 1923 that St. John changed his mind — his first public "confession" was made on 17 September, 1923 at the autumn meeting of the American Association for the Advancement of Science in Los Angeles. The context of this event might easily be portrayed as a "battle of faith," taking place during the first half of the 1920s, fought out between the sects of pro- and anti-relativists to gain "converts" among the vast majority of the yet undecided. Any successful "conversion" was interpreted as another triumph for that "doctrine" — both from the high pulpit of science and from the latter's organs of dissemination to the common people: the popular scientific magazines and the science sections of daily newspapers. Each such triumph was accompanied by the strident outcry of the "heretics" of the antirelativistic camp, who emphasized the remaining difficulties with relativity theory.

But as neatly as these metaphors may work, this paper does not aim to present such a captivating picture, portraying scientific developments as a fight over "converts," in which "rationality" degenerates into the "ideology" of a particular clique of a few dominant scientists. I will look for ways to understand *both* St. John's antirelativistic conclusions of 1917 *and* his 1924 prorelativistic interpretation of spectroscopic data as results of a rational method, checking a theory through comparison against experimental data; and I hope to make plausible my claim that a clue can be found in a systematic analysis of the arguments that led St. John to his conclusions about GRS as a consequence of the GTR. Those readers primarily interested in the conclusions derived from this case study might now want to jump to the last section of this paper.

1917–22: St. John, the Skeptic

St. John certainly did not learn about the relevance of the GTR to the problem of the small shifts of spectral lines (long known to spectroscopists) before 1917, when he might have read the first review articles in English by Willem de Sitter and Arthur Stanley Eddington — the first to discuss the GTR in English with special emphasis on its astronomical consequences (see de Sitter 1916–17; Eddington 1917a–b; 1918a–c). Einstein's original papers after 1914, especially his fundamental papers in the *Sitzungsberichte der Preussischen Akademie der Wissenschaften* and in the *Annalen der Physik* of 1915 and 1916, were all written in German. Furthermore, their titles did not immediately suggest their possible relevance to astrophysics. The same is true of other standard reference texts about the GTR before 1917, such as those of Einstein's colleague Erwin Finlay Freundlich (cf. Hentschel 1992b). A further delay in the scientific reception of the GTR among astronomers was due to the fact that German scientific journals were not available in England and possibly also not in the United States, because of the mutual scientific blockade between the opposing countries involved in World War I. It is true that in earlier papers — for the first time as early as 1907 and then again in 1911 — Einstein had already mentioned the possible influence of gravitational potentials on spectral lines; but these remarks had gone unnoticed to astronomers, because they were made in papers that appeared in specialized physics journals devoted to themes at the margin of their interest. The quotations from Einstein's paper of 1911 that St. John used in his first papers about GRS in 1917 must have been copied from Eddington, whose survey is cited by St. John in the same passage of his paper (see St. John 1917a, 452; 1917b, 249).

Anyway, only in 1917 did St. John realize that GRS would be of direct relevance to his work in high-precision spectroscopy, because any disturbing influence that might cause a shift of spectral lines away from their "true" values had to be corrected before extensive tables of solar wavelengths could be put together. "Our knowledge of the motions, pressure, and many other phenomena in the solar atmosphere must be obtained from line displacements in the spectrum, but [if] here it would be possible to apply definite corrections, this would in many cases, however, modify our interpretations" (St. John 1917a, 451). As Karl Schwarzschild and John Evershed had already done before him (cf. Schwarzschild 1914; Evershed 1914c, 1918a, 1920a), St. John chose the so-called cyanogen band close to $\lambda = 3883$Å, because of its far-reaching pressure independence. Because he was well aware of the fact that this band was problematic due to its very high density of lines (cf. St. John 1915a; Birge 1924), he confined himself to the sharpest and most isolated lines in the spectrum,[9] altogether 43 lines, of which he attributed a higher weight to the 25 lines of lower intensity (group A), because the 18 more intense lines (group B) were also wider and less sharp. But it is remarkable that in 1917 St. John did not take into account the symmetry of lines, so important in later inquiries, as for example that by Grebe and Bachem. A line he regarded as sharp, and therefore suitable for analysis, could very well turn out to be asymmetric in the microphotometric analysis of Grebe and Bachem (see Grebe and Bachem 1919, 1920; cf. Hentschel 1992a).

After this selection procedure, St. John took into account the four possible explanations for shifts of spectral lines known to him at that time: gravitational

[9] "Since they show wide variations in character and surroundings, an estimate was made of the weight to be assigned to result for individual lines. This estimate, based upon the appearance of the lines in both solar and arc spectra, is the observer's a priori judgement whether the measurements would have high, medium or low weight." By "a priori" St. John means that the measurement is made *before* the shifts have been evaluated, so that conscious manipulation of results by preselection of suitable lines is prevented.

redshift (GRS), Doppler shift (due to relative motion between the sun's gases as emitters of light and an observer on the earth), anomalous dispersion, and pressure effects. St. John could skip the last of these in his further analysis, because he had chosen lines that had been shown to be pressure-independent in the high-pressure laboratory experiments in Baltimore and Manchester. Concerning the anomalous dispersion proposed by Julius as the possible cause of many solar phenomena, including the shifts of spectral lines (see Hentschel 1991), St. John referred to his careful studies about this effect made between 1915 and 1917, in which he had come to the conclusion that anomalous dispersion might not lead to a systematic redshift but only possibly to "sporadic" shifts both to the red and to the violet (see St. John 1915a–b, 1916b–c; St. John and Ware 1916). The remaining problem for St. John was to disentangle the GRS and the Doppler redshift, both of the same order of magnitude, and both effects being proportional to the wavelength of the spectral lines under consideration. The strategy by which he tried to achieve this "disentanglement" of two possibly superimposed effects was quite tricky and based on the following consideration.

On the one hand, the GRS should be the same for the light from all points on the sun's disk, because it should, according to the GTR, depend only on the sun's mass and radius. On the other hand, the Doppler redshift, induced by radial convection currents of gases in the sun's atmosphere should depend on the angle between this convection movement and the line from the point on the sun's surface directed into the spectral apparatus and the observer on earth; that is, it should be maximal for light stemming from the central part of the sun's disk and be minimal for rays stemming from the sun's margin. Thus in a first step St. John compared spectral lines from the sun's center with those produced with a vacuum arc in the laboratory leading to shifts of – 0.001Å for group A lines, and shifts of + 0.0014Å for group B lines, which if taken together results in no effective redshift. In the second step he reasoned: if there should nevertheless be a superimposed gravitational redshift, for central parts of the sun, this must just have been compensated for by a convection of light-emitting gases toward the observer, with a Doppler shift equal to minus the gravitational redshift. In the third step: this auxiliary hypothesis, needed for upholding the possibility of GRS in spite of the results in step one of the argument, could be tested in a further independent part of St. John's fairly complex consideration in 1917. Up to now he had confined his analysis to light emitted from the center of the sun, but now he brought into the discussion his knowledge about the measured center-limb differences of the sun's spectrum. From the theory of the classical Doppler effect, which is limited to frequency changes induced by movements in the line of sight, the Doppler shifts should disappear at the sun's limb; consequently, there the whole unreduced GRS of 0.008Å could be found. But this was not what was observed. His own very careful measurements had shown a null result for the low-intensity lines of group A and only a minor shift of 0.0036Å for the lines of group B — that is on average about 0.0018Å, approximately one fourth of the relativistic prediction. "The general conclusion from the investigation is, that within the limits of error the measurements show no evidence of an effect of the order deduced from the equivalence relativity principle" (St. John 1917a, 452; see also 1917b, 258 ff.). The end of the chain of reasoning by which St. John was led to reject a possible GRS was marked by his reference to other series of measurements that would further confirm his results, especially two series taken at the Mount Wilson Observatory. In those series he believed he had demonstrated conclusively the equality of H and K lines in the calcium spectrum as produced by an arc lamp and at the sun's limb, respectively, and the only very minor shift of iron lines (of 0.004Å as compared to 0.013Å) according to relativity theory for this part of the spectrum (see St. John 1917a, 452; i.e., St. John 1910). With the help of these last results, he also hoped to refute earlier results by Evershed and Royds at the Kodaikanal Observatory in India, who claimed to have found an agreement of their observations of 12 lines of the cyanogen band with the relativistic prediction (see Evershed 1920a–c, 1921a–b).

Nevertheless, although St. John had good reasons for his claim to have linked consistently all spectroscopic evidence about shifts of spectral lines, in 1917 he remained careful about the status of these results, which he regarded as only tentative, regardless of whether they were against the GTR (as his own results were) or supported it (as Evershed's did).

> The Mount Wilson observations, based upon a larger number of lines, fail to confirm the results found at Kodaikanal. For the lines of highest weight there is no displacement to the red either at the center or at the limb. The measurements are inherently difficult, and results may be more or less influenced by the choice of lines and by the resolving power, definition, and dispersion of the spectrographs used. (St. John 1917b, 262; see also, e.g., Evershed 1914b; Evershed 1921a–b)

In this respect, the suspicion sometimes raised that scientists tend to present their results as indisputable cannot be upheld here. On the contrary, the astonishing degree to which supporters as well as opponents of relativity theory called for further experiments on the shifts of spectral lines in this phase of our case study runs very much contrary to a view of science trying to confine itself to the pursuit of group-specific interests. One could suggest that in winning support for future research, an unresolved problem could serve both pro- and antirelativists more than a closed dispute, but this would be as true for 1924 as for 1917. The fundamental difference between the situations in these two years, despite unchanged interests, is not covered by this sociological perspective on science; but we can hope to get some insight through our cognitively oriented analysis of the arguments employed at the end of this study.

In the contemporary reports about St. John's work one finds references to the still unresolved conflict with Evershed's results. While the latter seemed to favor Einstein's theory, the former contradicted it; and had St. John's skepticism turned out to be justified, the opinions about relativity theory — which had developed dramatically toward positive, even enthusiastic, praise after Eddington's famous light deflection results in 1917 (see Dyson and Eddington 1920) — could easily have changed again. The persistent anomalies in the data, which did not allow for any straightforward confirmation of the third prediction of Einstein's GTR, had already caused some irritation: "Further elucidation of the discrepancy between Kodaikanal and Mount Wilson will be awaited with much interest. It seems clear that the absence of the effect would either be fatal to Einstein's theory or at least involve a rather fundamental reconstruction of it" (St. John 1918a, 183; see also St. John 1918b). The question of how Einstein and other theoreticians reacted in this precarious situation, and especially the extent to which they employed the strategy mentioned in the above quote and tried to reformulate the GTR without GRS, or at least with a modified GRS, is discussed elsewhere (see Hentschel 1990b, 1992c; see also Earman and Glymour 1980), because to discuss it here would lead us too far astray, and also because quite different problems in the theory of science are connected to theorists' responses to experimental offshoots. Concerning the experimenters, it is telling that their claims about what can safely be inferred from their measurements are quite moderate — the more so if their results are negative, as in St. John's case from 1917 to 1922. In a survey article about the shift of spectral lines for a special issue of *Nature* in 1921 devoted to the theory of relativity, he stated:

> The evidence on this deduction from the Einstein theory is at present contradictory. . . . For statistical discussion the quantity of data available is as yet quite inadequate even in the case of iron, the most widely studied element. . . . For other metallic elements the data are even more deficient. With a sufficiently large and varied accumulation of material there is hope that the complex solar conditions may be analyzed, and the contributions to the observed effects arising from the various causes determined with some certainty. The pressing need is for data of the requisite accuracy and variety. This need adds interest to determinations of wavelengths and of pressure displacements, and to investigations of the characteristic behavior of spectrum lines, as all such data will have a part in solving one of the most absorbing questions in cosmic physics. . . . The present programme at Mount Wilson aims at an accumulation of varied and extensive data that will furnish a suitable basis from which to approach the general question of the behavior of Fraunhofer lines relative to terrestrial sources. (St. John 1921b, 789ff.)

This is a clear indication of the quasi-Baconian attitude that St. John shared with other astrophysicists concerning relativity theory at that time, after so many efforts to clear up the question on the basis of small sets of selected lines, presumably free of disturbing effects, had failed. Despite this empiristic attitude of St. John and his colleagues at that time, his measurements from then on were by no means theoretically unladen, naive samples.[10] They were designed to test the hypothetical GRS in conjunction with other effects that were all studied both theoretically *and* experimentally, such as influences of pressure, temperature, electromagnetic fields, and parameters of the electric circuits used to produce artificial light in laboratories. Theorists and experimentalists shared in this complex undertaking of disentangling these possibly superimposed effects: the theorists had to tell the experimentalists how these different effects behaved — i.e., how they varied in frequency, pressure, etc. — and the experimentalists in turn had to provide the theorists with clues about what effects it might be possible to explain from their point of view.[11]

This interaction between theoreticians and experimenters also lead to St. John's assuming the same predispositions his fellow theorists held against the STR, as the following excerpt from a letter by Dayton C. Miller to Joseph Larmor of 9 June 1921, shows particularly well. Larmor was a well-known Irish theorist who was among the most convinced opponents to relativity theory, and the astronomers at Mt. Wilson often consulted with him: "Dr. St. John is diligently at work on his spectroscopic measures and he is still hopeful of results favorable to the ether, but the work is so elaborate that it will be some months before he can draw conclusions. He has told me of your interest in this work" (Royal Society, London, Larmor Papers, Doc. 1428, 3). As late as 1922, St. John countered rumors about a confirmation of GRS in new data from the Mount Wilson Solar Observatory with an energetic denial (see figure 2).

Only at the end of 1923 did St. John retreat from his six-year-long persistence and publicly declare, backed by new results: "The quantity of data bearing upon the question of gravitational displacement of the Fraunhofer lines now at our disposal at the Mount Wilson Laboratory seems to justify a new discussion" (St. John 1923b, 93). Needless to say, this "new discussion" revoked St. John's prior skeptical remarks about the presence of GRS and ended with his carefully worded but unambiguous acceptance of its presence in spectroscopic data from the sun. Let us now turn to the detailed description and discussion of the arguments that caused this drastic change in St. John's contentions about GRS.

"Little Details that Might Interest You"

The mental path that had led St. John to withdraw from his former opinions becomes quite clear from letters he wrote both to his "boss" — the founder and longtime director of the Mount Wilson Solar Observatory, George Ellery Hale

[10] In the sense in which the spectral tables, as put together by Ångström or Rowland, could be looked upon.

[11] A very nice example of this intimate collaboration is the discovery of the Zeeman effect by Peter Zeeman in 1896, who was in close contact with Hendrik Antoon Lorentz, whose electron theory was in a sense a precursor of Einstein's special theory of relativity: see, e.g., Arabatzis 1992.

Physik. Zeitschr. XXIII, 1922. John, Bemerkung zur Rotverschiebung. 197

Bemerkung zur Rotverschiebung.

Von E. St. John.

In bezug auf das Gerücht, daß die letzten Beobachtungen auf dem Mount Wilson über die Verschiebung der Linien des Sonnenspektrums die Folgerungen aus der Einsteinschen Theorie stützen, habe ich zu bemerken, daß jenes Gerücht auf eine von dem Berichterstatter der New York Times mißverstandene Äußerung von Herrn Professor Schlesinger zurückzuführen ist, mit dem dieser eine telephonische Unterredung hatte. Ich habe keinen Grund gefunden, die Genauigkeit der früheren Beobachtungen an den Linien der Cyanbande zu bezweifeln, aber seit Herr Dr. King gezeigt hat, daß die Linien der verschiedenen Serien in dieser Bande mit der Temperatur ihre relativen Intensitäten ändern, und Herr Dr. Birge gefunden hat, daß die Mehrzahl der scheinbar einfachen Linien komplex ist und oft aus Linien sich zusammensetzt, welche zu verschiedenen Serien gehören, ist es klar, daß, wenn man die Unterschiede der Temperaturen auf der Sonne und auf der Erde in Betracht zieht, diese Linien für eine Prüfung der Theorie nicht so geeignet sind wie man zuerst dachte.

Vom Gesichtspunkt der Sonnenprobleme und der Relativitätstheorie aus ist es von größter Wichtigkeit festzustellen, ob die entsprechenden Wellenlängen der Linien im Sonnenspektrum und in den Spektren irdischer Lichtquellen mit den Forderungen der allgemeinen Relativitätstheorie in Übereinstimmung gebracht werden können. Mit Rücksicht auf den jetzigen Stand dieser wichtigen Frage erscheint es, um endgültige Schlüsse ziehen zu können, notwendig, ein ausgedehntes Programm aufzustellen, welches Beobachtungen in der Mitte und am Rande der Sonne umfaßt, Beobachtungen, welche sich auf einen möglichst großen Wellenlängen- und Linienintensitätenbereich erstrecken, und weiter diese Beobachtungen durch die notwendigen Daten von Laboratoriumsversuchen zu ergänzen; kurz, genügend Material für eine statistische Behandlung zu erhalten. Ein solches Programm ist auf dem Mount Wilson in Ausführung begriffen, doch ist die Sammlung der experimentellen Daten noch nicht vollständig genug für eine umfassende Diskussion, und ohne eine solche würde es voreilig sein, ein Urteil zu fällen.

Für 32 Eisenlinien finden Buisson und Fabry die Unterschiede zwischen den Wellenlängen im Vakuumlichtbogen und den Wellenlängen in der Mitte der Sonne von der Größenordnung des Einstein-Effektes und schließen daraus, indem sie sehr niedrigen Druck in der umkehrenden Schicht annehmen: „L'effet Einstein est la seule cause de déplacement des raies du spectre solaire". Auf dem Mount Wilson ausgeführte Beobachtungen an einigen hundert Linien zeigen, daß in verschiedenen Spektralbereichen die Unterschiede im Sonnenspektrum und im Spektrum des Vakuumlichtbogens im Vergleich mit der Einsteinschen Forderung sich sehr verschieden erweisen, und daß in demselben Spektralbereich die Werte für verschiedene Linienklassen sehr verschieden sind. Es ist demnach klar, daß das Problem kompliziert ist und eine einfache Annahme, wie diejenige geringen Druckes in der umkehrenden Schicht, die Resultate einer umfassenden Reihe von Beobachtungen mit den Folgerungen der Einsteinschen Theorie nicht in Einklang bringen kann, so daß die Notwendigkeit besteht, eine umfassende Untersuchung anzustellen, wenn die wirklichen Ursachen voneinander getrennt und klargelegt werden sollen.

Mount Wilson Observatory, 1. März 1922.

(Eingegangen 23. März 1922.)

Figure 2. Facsimile of St. John's declaration in the *Physikalische Zeitschrift*, 1922.

(1868–1938),[12] — and to Harlow Shapley (1885–1972), the director of the Harvard College Observatories.[13]

At the end of 1922, Shapley asked St. John about the most recent advances in solar physics, because he had to report on the current state of affairs in astronomy at a Christmas meeting of mathematicians, physicists and astronomers in Boston: "The symposium is on Space and Time, and of course relativity must get into it. As to the shift of the solar lines, probably you have not [14] much to add to the statement you made in England last May — namely, that the Mount Wilson results as yet say neither yes nor no" (Shapley to St. John, 27 November 1922, HUA, Sign. HAV. 630, 22, Box 17, Folder 126). St. John's reply is highly interesting to us, since it contains a detailed discussion of the pros and cons concerning GRS in his data, making explicit all the tentative strains of thought that St. John was pursuing at that time to clarify this question. We realize that in 1922 St. John was no longer happy with his own distinctly negative conclusions of 1917 about GRS. Obviously he tried to fit the superimposing features of pressure effects, temperature and chemical dependencies, center-limb shifts and all kinds of other complications into a consistent mosaic-like picture, but it did not work out, either with or without GRS. Concerning Shapley's inquiry about new developments in GRS since his papers covering this question up to 1921, St. John replied:

> No, there is nothing new to add to what I said last summer about the gravitational shift of the lines in the solar spectrum. It is still a complicated matter and we have not been able to convince ourselves of its influence in solar and stellar spectra. The strong point in its favor is the fact that as a general rule the solar lines shift to the red and we have no really good explanation of such displacements. The difficulties with the Einstein explanation arise from the variation in the amounts of the displacement which seem to have no definite relation to the calculated values, Fabry and Buisson to the contrary, notwithstanding. (St. John to Shapley, 8 December 1922, HUA, Sign. HAV 630, 22, Box 17, Folder 126)

A tentative inclusion of GRS into the network of superimposing factors causing the observed shifts of spectral lines in the sun's Fraunhofer spectrum as compared to terrestrially produced light seemed justified to St. John, because of the fact that *grosso modo* such a redshift was observed *on the average* and that there was no other "really good explanation" apart from the GTR. His discussion of the reasons that still opted against such an inclusion shows how controversial the issue was and how uncertain St. John still was about it at the end of 1922. It is remarkable

[12] About Hale, see Newall 1924, Stratton et al. 1938, Adams 1938, Millikan 1938, Wright 1972 and further sources cited therein. Hale's estate is kept partly at the Huntington Library, San Marino, and partly at Caltech, Pasadena; it is also available on microfilm in several other libraries.

[13] Shapley's estate is in the Harvard University Archives (henceforth cited as HUA).

[14] On the typed file copy available to me, the word "not" was inserted in Shapley's hand.

that even for St. John, one of the leading experts in the field of high-precision measurements of the sun's spectral lines, it was of great importance to be in some accord with the results of other teams in the world also pursuing this question. As the next paragraph of his letter to Shapley shows, not only St. John but his colleagues all over the world shared his opinion that a definite conclusion about GRS would have to wait for a couple of years and could be reached only through a concerted effort of many experts, so many ambitious attempts by one individual to solve the question by brute force having already failed.[15]

> The one thing I feel sure of is that it has not been shown by Fabry and Buisson, nor by Pérot, nor by the Bonn physicists. I saw Einstein and Freundlich in Berlin, and Kayser and Grebe in Bonn, and I got from them the idea that they were far from being satisfied with the present "proofs." Anyway, they plan a long campaign with the "Einstein Turm," and I was glad to learn that they, both in Potsdam and Bonn, feel that it will require a great deal of work to establish it convincingly, and that nothing is gained by such observations as those of Pérot and Fabry. It is certainly true that the Mount Wilson observations show that Pérot's observations are far from reliable and that Fabry and Buisson's two score of lines are far from sufficient. (St. John to Shapley, 8 December 1922, HUA, Sign. HAV 630, 22, Box 17, Folder 126)

After this devastating critique of all former attempts, especially of Alfred Pérot, Charles Fabry, and Henri Buisson in Marseille (see Pérot 1910, 1920a–b, 1921–22, 1922; Buisson and Fabry 1921), to demonstrate GRS with the help of small sets of data of selected lines, typically from only one spectral band, St. John described his own strategy based, in contrast to that of his colleagues, on large and heterogeneous data sets:

> As you know, our observations cover many hundreds of lines from the extreme red to the ultra violet, lines of different elements and of different intensities and classes of the same element both at the center of the image, and across the disk and at the limb. These same lines we also investigated in the laboratory. Any explanation of shift of the solar lines ought to take in the shifts at the limb where there is freedom from radial convection currents in the solar atmosphere. Strangely enough Fabry and Buisson neglect this in their remarks, though they assume zero pressure in the solar atmosphere, where formerly they assumed a pressure of five or six atmospheres to account for the difference between limb and center. They have considered only the difference between center and arc. We have tried to check up Pérot's

[15] See, e.g., the Annual Report of the Mount Wilson Observatory for 1921, *Yearbook of the Carnegie Institution of Washington for 1921*, 242–44: "Because of numerous fragmentary attacks upon this question, the situation is becoming more and more involved and unsatisfactory"; a brief overview by St. John himself is included. See also Hentschel 1992a about Grebe and Bachem.

> observations and they do not check. (St. John to Shapley, 8 December 1922, HUA, Sign 630, 22, Box 17, Folder 126)

Nonreproducible results, methodical differences about which factors should be included in the data analysis, uncertainty about the particular parameters important for data reduction, such as, e.g., the temperature of the sun or the pressure in its reversing layer — all these points made it impossible to come to a safe conclusion about the presence of a gravitation redshift in the order of magnitude predicted by the GTR in the data available from solar spectroscopy. No one could still hope for an easy solution; as St. John had made clear in the annual report of his observatory for 1921, the problem would have to be attacked in its whole breadth, without the former artificial isolation of such subissues as the center-limb variations, the intensity and chemical dependencies of the observed shifts, etc. — all of which were in fact inseparable parts of the problem. And this thorough analysis would require lots of time.

> Owing to the different and even inconsistent corrections applied to the observed sun-arc displacements, the resulting approximate agreement with the deductions from the Einstein theory fails to carry conviction. In view of the situation in which this important question now stands, it appears necessary, in order to reach a definite conclusion, to carry out an extensive program on sun-arc displacements, including observations at center and limb and covering the widest possible range in wavelength and line-intensity; to obtain, in short, a reliable body of data as a basis for statistical discussions. The problem must be envisaged as a whole and not in detached portions and a consistent and probable rôle found for the gravitational effect if the theory of relativity is to find confirmation in the displacement of Fraunhofer lines.
>
> In addition to data for disentangling the causes involved in the displacement of the solar lines, the program includes a study of the relative consistency of the solar wavelengths at the center and limb, the determination of a series of solar standards in the international system, and observations on a limited spectral region in common with the Kodaikanal Observatory. (St. John 1921d, 244)[16]

This overloaded program of St. John left little room for quick results from the Mount Wilson Solar Observatory for the next few years, and yet his opinions on this matter developed much faster than one might expect from these statements in the annual report or from his skeptical prognosis in his letter to Shapley of December 1922. The first hints of a change of mind on this matter are already traceable in a letter to Hale dated 13 May 1923, in which he reported the following "little things that might interest you," after he had already written loosely about this and that for several pages.

[16] From the *Yearbook of the Carnegie Institution of Washington for 1921,* which gives the full text of all annual reports of the Mount Wilson Solar Observatory.

I am driving [?] away on the gravitational shift and for the first time I am striking something that looks like that effect. To get free from pressure I am trying to use the high level lines, such as the Mg triplets and the green and violet H[?], the Al lines at 3900, Cu[?] 4229 and the D lines. It is difficult to obtain [?], become[s] more troublesome still when limb shift is taken into account. Lines of low intensity 0–2 give far too small a displacement at the center and not much limb-shift. There appears to me to be at least three things acting, and I am trying to reconcile the observations with some working hypothesis which at present is something like this: an Einstein shift for all lines; a Doppler effect for low-level lines decreasing the Einstein effect, this disappears at the limb showing as a limb-center displacement. For the accurate solar and terrestrial measure, it looks as though these lines might show a displacement at the center of about the right order and fractionally the same shift at the limb as at the center. If this turns out to be the case I do not see any other explanation of such a behavior. But the great body of solar lines give, on the face of the returns, great difficulty in the relativity view. Lines of medium intensity 5–10 on the assumption of zero pressure give no[?] large displacements and [the] great majority of lines, of medium intensity, no Doppler effect but a limb-center shift due to anomalous refraction, which according to Julius is small for weak and very sharp lines and larger for lines of medium intensity.

As yet this is only a working hypothesis but it has the virtue of directing investigation. Just now I am measuring some of these lines in King's furnace plates where the lines are fine and sharp and I think free from pole-effect and Babcock is preparing to get them in a way that will be far from the disturbances due to the CN lines in the furnace. I hope to live long enough to be able to satisfy my own mind at least as to the effective cause in the relative wavelengths in solar and terrestrial sources.

Well, enough of this. (St. John to G. E. Hale, 13 May 1923, Caltech; "[?]" represents uncertain reading.)

This passage in St. John's letter to Hale already contains in embryonic form all the elements of his argumentation in later papers: the choice of certain groups of lines to get rid of disturbing pressure effects, the inclusion of the line intensity as an important parameter, the layer-dependent discussion of his data under the assumption that the respective Doppler shifts of different layers in the sun's atmosphere could differ (because of changing convection currents in different strata of the sun), and finally the clever discussion of the possible overlap of all of these effects in the actual data. In May 1923, the remaining problem was how to explain why the majority of lines of medium intensity showed a redshift too small to satisfy the prediction of the GTR. How St. John solved this puzzle will be discussed in the next section on the basis of his publications from late 1923 onward. But first I would like to mention briefly how St. John's considerations were appreciated by his superior, the director of the Mount Wilson Solar Observatory.

Hale seems to have received St. John's letter during his trip to England in July 1923. After having replied to the other points raised in St. John's letter, Hale somewhat patronizingly referred to St. John's new endeavors in the following passage:

> I am greatly interested in your progress on shift of solar lines and am sure that you will ultimately be rewarded for your long and careful work. I have had to avoid discussions here [in London at the Royal Institution] but I must tell Jeans what you are doing and see if he has any new views as to the line shifts in the sun. (Hale to St. John, 10 July 1923, Caltech)

The last sentence of this passage refers to James Hopwood Jeans (1877–1946), who had proposed in the early 1920s an interpretation of the invariant *ds* differing from the standard interpretation by Einstein in terms of the readings of ideal clocks and measuring rods distributed in space-time (see, e.g., Jeans et al. 1920, 73ff.; Hentschel 1990b). The fact that Hale and, indirectly through Hale, St. John also had contact with Jeans and his English colleagues, who did not agree with Einstein, reflects St. John's social environment and the fact that around 1923 most American astrophysicists in those scientific communities, were not yet prepared to accept the GTR unequivocally or even with modifications. But it is equally interesting to see Hale report that he would try to avoid discussion about relativity; this carefulness already prepared the way for a possible later retreat from the opposing stance toward relativity, which was then dominant among astrophysicists. Relativity theory was no longer the *ab initio* far-fetched invention of a foreign theorist but rather a possible, yet not very plausible, option that must be tested just to see what could be made out of it in pursuit of theoretical explanations of spectroscopic data not yet fully understood. After this preparation of a possible shift of opinion, induced here by Hale and applicable also to St. John with regard to his relation to Larmor, St. John's declarations at the thirtieth meeting of the American Astronomical Society at Mount Wilson and at the autumn meeting of the American Association for the Advancement of Science (see St. John 1923a–b) was like a sudden disclosure of a change of opinion, whereas in fact that change had been much longer in preparation and had only remained invisible to the greater public.

1923: St. John's Conversion and Why It Occurred

The above extracts from St. John's letters allow us a fascinating insight into the interesting period of his "conversion," during which the view firmly adopted later is as yet nothing but a "working hypothesis" to be tested for its strengths and weaknesses — just because there seems to be no other plausible alternative that would offer at least the prospect of an explanation for the anomalies in the observed spectral shifts of the sun's Fraunhofer spectrum. In St. John's publications after 1923, this trial of a working hypothesis was transformed into a convincing chain of reasoning, which (at least he himself was so convinced) would be irrefutable, even for his most skeptical colleagues.

What St. John needed most was a better quantitative basis. A first step toward this goal was getting rid of the complication due to the eventual influences of pressures other than atmospheric pressures, a factor that was long believed to be the principal cause of the sun/earth differences. From around 1895 till about 1920 it had been estimated that in the sun's so-called reversing layer (which transforms the emission spectra of glowing gases beyond into Fraunhofer absorption spectra) pressures of about 5–6 atm would reign. These estimates had been made on the basis of the observed shifts and in comparison to laboratory measurements of spectra under different pressures (from nearly 0 to about 100 atm) (see, e.g., Humphreys 1908; Duffield 1907–15; Ayyar 1915; Stark 1915; St. John and Babcock 1914, 1924; Babcock 1928). According to their pressure dependence, all chemical elements had been classified as belonging to one of four groups (a,b,c,d), all groups showing linear dependence, if the pressure was increased, but with different coefficients of proportionality. St. John employed this well-confirmed knowledge about the effect of pressures in his own research as follows: If there really was an increased pressure in the sun's atmosphere and if this were the decisive cause of the observed shifts, then the group dependence as found in the laboratories should also be found in the sun's spectrum, if one compared the shifts of spectral lines of elements belonging to different pressure groups. That is, in one and the same spectral region, the difference $\lambda_{sun} - \lambda_{earth}$ should be larger, the higher the pressure dependence coefficient in the group of spectral lines chosen for analysis.

Table 2. Inferred pressure of the reversion layer in the sun's atmosphere as a function of the spectral region and of the pressure group of the spectral lines*

Group	Region (in Å)	# (lines)	Pressure (in atm)
a–b	3800	41	-1.0
a–b	5050	92	+0.9
b–d	4100	62	+0.5
b–d	4550	37	-0.2

* Cited from St. John 1923–24b, 23.

Following this reasoning, a comparison of strongly and weakly pressure dependent spectral lines should allow St. John to infer the real pressure in the sun's reversing layer. Schematically:

$$\frac{\text{shift of lines in the d group} - \text{shift of lines in the a group}}{\text{coef.[d]} - \text{coef.[a]}} = \text{pressure in atm}$$

Applying this method to a comparison of the groups — a compared with b and b with d, in each case for two different spectral regions, St. John got the results shown in table 2.

The inferred pressures of the reversion layer fluctuated around an average of zero; even nonphysical negative values appeared. Next, St. John adopted a style of argumentation resembling that of indirect mathematical proofs — bringing a starting assumption to a contradiction proves the correctness of the statement contrary to this initial assumption: The premise of pressure dependence as the real cause of the observed shifts had run into physical contradictions; ergo, pressure differences between the sun and the earth are not the decisive factor determining the spectral anomalies under discussion. I might add at this point that with this conclusion, St. John was basically right. Around 1920, Pérot and others had already questioned the initial assumption of pressures around 5 atm, and not much later it was settled that the actual pressures in those layers where the Fraunhofer lines are produced are only very small (minute fractions of one atmosphere) (see, e.g., Pérot 1921–22, 1922; Saha 1920, 1921; Fowler and Milne 1923; see also St. John 1923–24b, 23; St. John and Babcock 1924).

After these first definite but negative results, St. John employed a similar strategy in his check of other possible causes, again differentiating the spectral classes and regions, generating elements, line intensities, and other parameters. The more he introduced, the more individual measurements he needed to correlate all the possible physical factors. At first, he decided to concentrate on one easily measurable and identifiable element and vary the other parameters for this element. Soon he had collected hundreds of measurements of iron lines in the sun's spectrum; laboratory comparisons were available to him, due to measurements by his colleagues at Mount Wilson and at many other places around the world, since iron was one of the elements most frequently measured. Next St. John classified all his measurements into eight groups (see table 3), utilizing the formerly found pressure dependence and other parameters, especially line intensity, which he tended to regard as an indication of the height of the layer in which the spectral lines were produced.

By classifying his measurements according to the line intensities, St. John hoped to retain in each line of table 3 only lines stemming from the same stratum of the sun's atmosphere. Indeed, his comparison with the predicted values of the GTR showed remarkable systematic deviations correlated to line intensity, and therewith (for St. John) to the height of the absorbing layer: high deviations for lines presumably produced in layers far up, and large deviations for lines produced from deep layers of the sun's atmosphere. These systematic deviations were not without plausibility. It could very well be that in different layers there were changing convection currents, and in fact the last column of his table shows that to get agreement between theory and experiment, he had only to assume that in the deeper layers (with weaker but more numerous lines) the hot gases would show radial convection away from the center, while in the outer layers (with intensive but less numerous lines) the cooler gases would drift back toward the center. Circular patterns of rising hot gases and falling cooler gases are known (e.g., from the earth's atmosphere), so they appeared to St. John to be highly plausible. So the basic idea of St. John's data analysis so far is that the GRS equal for all lines was modified by a Doppler redshift dependent on the absorbing layer in which the individual lines were produced. This could explain why in former measurements averaging over larger sets of lines, possibly stemming from different strata of the sun, no consistent result could be found. St. John's analysis could also explain why most former measurements had shown a redshift much too low in comparison with the prediction: 6/7 of the iron lines in St. John's data base stemmed from layers deep within the solar atmosphere with a high upward convection current effectively masking the gravitational redshift by a superimposed Doppler shift to the violet. A quantitatively similar result should also appear for other elements apart from iron, such as C, N, or other metals that had sometimes been used before by astrophysicists in redshift measurements. In fact, the erroneous evaluation of shifts would always occur as long as one did not apply the layer-dependent correction for the convection currents first employed here by St. John. Only when this correction for superimposed Doppler shifts was done before the final analysis

Table 3. Interpretation of the 231 spectral lines of St. John's measurements of 1923–24*

Group[1]	#[2]	I[3]	λ[4]	$\Delta\lambda_{GTR}$[5]	$\Delta\lambda_{exp.}$[6]	Exp.–Calc.[7]	Convection[8]
a	17	12.0	3826	0.0080	0.0120	+ 0.0040	0.30 km/s down
b	24	14.0	3821	0.0080	0.0112	+ 0.0032	0.25 km/s down
b	10	10.4	4308	0.0091	0.0113	+ 0.0022	0.16 km/s down
a	10	6.0	5419	0.0115	0.0112	- 0.0003	≃ 0 km/s
b	95	4.6	4166	0.0088	0.0072	- 0.0016	0.10 km/s up
b	36	5.2	6294	0.0133	0.0115	- 0.0018	0.10 km/s up
d	106	4.55	4763	0.0100	0.0069	- 0.0031	0.20 km/s up
a	33	3.3	4957	0.0105	0.0074	- 0.0031	0.20 km/s up

[1] Pressure-dependence group
[2] Number of lines in this category
[3] Intensity
[4] Wavelengths in units of Ångström
[5] Set value of the GRS according to the GTR
[6] Average measured value
[7] The difference between the prediction of the theory (GTR) and the experimental outcomes of GRS
[8] The radial convection currents needed to harmonize spectral observations and theoretical expectations.

* See St. John 1923b, 94; 1924c, 534; 1926, 65.

was something physically meaningful arrived at and a chance obtained to trace down the GRS hidden for so long.

This retrospective explanation of former failures also applied to St. John's own earlier measurements, as he was ready to admit. In 1928, he published his studies of 515 lines in the cyanogen band, among them also the 43 lines of this molecular band spectrum he had used for his analysis in 1917. The analysis confirmed his positive conclusion about the presence of GRS in the sun's spectrum, contrary to his earlier claims. With regard to his own past errors, due to an insufficient data base, he called his four dozen lines used a decade earlier his "forty thieves."

> My original investigation was confined to some 40 lines and gave negative results. In view of later work on the complete band, these lines might be called the "Forty Thieves." The present investigation includes the whole [cyanogen] band. . . . It is assumed that random errors introduced by faulty measures, blends, and overlapping series are as likely to be positive as negative, and that their effect will be practically eliminated from the mean . . . . The 43 lines in my original investigation are included among the 515 lines. Their remeasurement agrees well with the original measures, which failed to show displacements to the red in agreement with the Einstein theory of gravitation. Their influence, however, is counteracted in the final mean, based upon the far greater number of lines. (St. John 1928a, 236ff.)

To counter the possible objection that all observed shifts could be equally well explained by *only* hypothesizing radial convection currents inducing Doppler red- and violetshifts, St. John decided to include the center-limb variations of the observed shifts in his analysis. Because radial convection currents are nearly vertical toward the optical path from the sun to the earthbound observer, and because the classical Doppler effect is strictly confined to relative motions between emitter and absorber in the line of sight, the Doppler shifts should completely disappear at the sun's margin and only the GRS should remain there, while in the sun's center, the Doppler shifts would be superimposed. In fact at the sun's limb a small shift toward the red was observed (the so-called Halm or limb effect), but its order of magnitude was not the one expected according to the GTR prediction ($2 \cdot 10^{-6}$). For St. John, this showed two points at once: first, that convection currents *alone* were insufficient to explain the observed shifts in the sun's Fraunhofer spectrum; second, that even hypothetical convection currents *and* GRS together could not yet cover all the spectroscopic observations. St. John needed an additional hypothesis that could explain the remaining difference between the GTR's prediction and the observations of the sun's limb. From the end of 1923 he thought he had found this additional cause for shifts in a possible molecular scattering of light according to the Rayleigh-Schuster formula. Rays stemming from the sun's margin have to cross a much longer area of rarified gases than central rays, because of the sun's spherical shape, so the limb rays undergo a higher probability of being scattered than the central ones. But for each scattering process, the probability of a widening of the line towards the red is higher than toward the violet.[17]

To resume the former reasoning, in 1923–24 St. John for the first time had combined three quite different physical effects (Doppler shifts, GRS, and anomalous scattering) in his explanation for the observed shifts of solar spectral lines in comparison to earth-produced spectra; and he had also managed, using an ingenious chain of reasoning, to disentangle all of these superimposed effects in the data at his disposal. In later years, St. John refined this reasoning but never modified it further; he supplied additional data (for other elements and for other line intensities),[18] and he refined his model of solar convection currents,[19] but he never revoked his basic claim that it is this superposition of the three principal causes — Doppler, gravitation, and scattering effects — that is responsible for the sun's spectral shifts.

> The conclusion is that three major causes are producing the regular differences between solar and terrestrial wavelengths, and that it is possible to disentangle their effects. The causes appear to be the slowing up of the atomic clock in the sun to an amount predicted by the theory of generalized relativity, radial velocities of moderate cosmic magnitude and in probable directions, and differential scattering in the longer paths traversed through the sun. The first obtains for all lines in all parts of the sun, the second appears regularly and continuously, downward at very high and upward at very low levels, while the third manifests itself in the so-called limb-effect. (St. John 1923b, 96)

In addition to this exhaustive research program concerning the solar and the stellar spectra, St. John also made independent checks of the theory of relativity. In the 1920s he organized a systematic search for positive effects in Michelson-Morley type experiments, because Dayton C. Miller had claimed to have found small positive effects in this experiment, contrary to what the special theory of relativity (STR) would let us expect. Again, he found the STR confirmed,[20] and since the GTR reduces to the STR within certain limits (of vanishing curvature-

[17] The coefficient of scattering is proportional to the square of the index of refraction (Rayleigh-Schuster), and the latter is larger on the red side than on the violet side of each spectral line due to the anomalous dispersion effect. See, e.g., St. John 1923b, 95; 1926, 67; Rayleigh 1871, 1881, 1899; cf. Schuster 1905.

[18] In 1924, St. John published his high-dispersion results for the spectra of Sirius, Procyon, and Arcturus; in 1926 he presented a study about the correlation of redshifts and the presumed height of the absorption layer for Ti-lines; and finally in 1928 he published his most extensive search for GRS in a total of 1,537 lines stemming from the sun's center and 133 from the sun's limb, respectively.

[19] E.g., by also discussing the nonradial convection currents, first hypothesized by Evershed (the Evershed effect), see, e.g., Evershed 1909–10, 1914d; St. John 1913, 1914a.

[20] See, e.g., the annual reports of the Mount Wilson Solar Observatory, reprinted in the *Yearbook of the Carnegie Foundation of Washington for 1920–28* and also St. John's report, "The Michelson-Morley Experiment and the Predictions of General Relativity: The Observational Status of the Theory of Relativity," typescript, 11 pages, The Albert Einstein Archives, The Hebrew University of Jerusalem, Doc. 22 250.

tensor), a necessary prerequisite for the possible upholding of the GTR was fulfilled. The network of independently confirmed hypotheses around the theory of relativity became more and more stable. St. John the skeptic had become Einstein's ardent supporter.

1923–24: The Impact of St. John's Conversion

St. John's conversion to relativity theory crystallized the public's opinion on the empirical status of this theory. The first popular report about it was given in the supplement of *Science* on 28 September 1923, and even this first notice was nothing less than a triumphant proclamation of the "confirmation of the third prediction of Einstein." St. John had released his bombshell before the delegates of the autumn meeting of the American Association for the Advancement of Science, and his audience immediately compared the drastic change in the status of the experimental verification of this extensively examined prediction of the GTR to the spectacular confirmations it had got through the light deflection test in 1919 and through Einstein's success in explaining the motion of Mercury's perihelion — an anomaly of classical astronomy since the time of Leverrier and Newcomb. "Dr. St. John's announcement completes the list of Einstein's predictions, all of which have been verified" (St. John 1923a). Of course, this enthusiastic report simplified the many remaining difficulties with GRS, as well as with light deflection in the gravitational field; in fact, both themes remained controversial among the few real experts of these high-precision measurements for many years. But the simplifications of "complete verifications" and the like were produced not only by scientifically untrained popularizers; the scientific community itself produced them, obviously with the implicit aim of presenting a definite result, to escape the inconclusive terms "provisionally," "perhaps," "as yet unconfirmed," etc. For example, in 1923 H. H. Turner of the Oxford Observatory still wrote, both in London's *Times,* and in the weekly magazine *Science:*

> These clouds which have hung about the third test have now been dissipated. Mr. C. E. St. John, of Mount Wilson, who had thrown the gravest doubts on the experimental facts, has now come round definitely in favor of the Einstein result. ... The conversion of Mr. St. John is of obvious importance, and the joint testimony of these former opponents [in addition to St. John also John Evershed, K. H.] leaves the matter now in no reasonable doubt. (Turner 1923b)

Turner's allusion to Evershed's earlier series of measurements (see Evershed 1920a–c, 1921a–b, 1923) was by no means accidental; the latter, long-time director of the Kodaikanal Observatory, had complained in a letter to Turner dated 22 December 1923, that in the general euphoria about St. John's results, it would be forgotten that it was he who had, already in 1918, obtained tentative confirmations of GRS at the Kodaikanal Observatory, but at that time no one had taken him seriously due to his conflict with St. John's results of 1917 (Royal Astronomical Society, Evershed Papers, Box 1, Evershed-Turner correspondence). As always, success had many fathers.

But the quote from Turner's announcement in 1923 also reveals something else. The metaphor of dissipating clouds reveals the irritation of the scientific community, accustomed to speedy "enlightenment," that must have encouraged the dark cloud cover to accumulate, that obliterated the empirical status of GRS for such an unusually long time. Now the pretentious sentences and the qualifying afterthoughts, all that hedging with provisos so typical of science talk — all that was now superfluous. But St. John's final statement in his first article of 1924 was still quite cautious: "The probability is so great, that we can, I feel, confidently adopt the general theory as a sound working hypothesis in solar investigation" (St. John 1924c, 535).[21] It was transformed into simple affirmative sentences and into newspaper headlines around the world, such as "Sun Proof of Einstein's Theory," "Einstein's Gravitationstheorie bestätigt," and the like (see figures 3 and 4 below). The message offered to all nonexperts was the unqualified and final confirmation of a long-awaited "Einstein effect" in the sun's spectrum.

It is remarkable that Einstein, who had shown considerable enthusiasm after he learnt about the confirmation of the light deflection measurements by Crommelin and Eddington in 1919, did not join in with all those who praised St. John as the redeemer of all skeptics. Read for instance his carefully worded and cautious reply to a journalist's query about his opinion concerning St. John's results, published in the *New York Times* of 27 April 1924 (see figure 3):

> There is still no absolute certainty that the observations have proved the reality of this effect, although most experts who have worked along these lines are now convinced that the theory has been proven . . . but I repeat that even now the phenomenon is not quite explained because it has affected too many as yet unknown factors.

This surprising hesitation can be partly explained by the fact that Einstein had already suffered some bad experiences with the earlier attempts of two Bonn physicists, Leonard Grebe and Albert Bachem, to confirm GRS — eager attempts that had failed, although Einstein had tried his best to convince his colleagues of the inherent plausibility of the Grebe-Bachem results (see Hentschel 1992a). So he certainly did not want to take any further risks. His hesitation might also have been due to his idiosyncratic tendency always to swim against the mainstream, so in the period of general enthusiasm about St. John's achievements, Einstein showed some signs of skepticism. But it must also be said that later developments

[21] Incidentally, St. John's use of the term "working hypothesis" is analogous to Einstein's introduction of the photon hypothesis as a "heuristic point of view" ("heuristischer Gesichtspunkt"). It shows St. John's *reservatio mentalis* still present in 1924; but it was later to disappear in his papers.

OFFERS SUN PROOF OF EINSTEIN THEORY

Dr. St. John Says 'Third Effect' Is Confirmed at Mt. Wilson Observatory.

GRAVITY SHIFTS SPECTRUM

Absence of Solar Pressure Is Also Declared a Fact Adduced by Recent Research.

Special to The New York Times.

CHICAGO, April 24.—Dr. Charles E. St. John, solar physicist of the Mt. Wilson Observatory staff at Pasadena and identified with important investigations concerning the Einstein theory of relativity, states that the third prediction in the general theory of relativity has been confirmed by the results obtained in the last few months of his work on the sun.

Speaking with the authority of one who has done more than any one else in the world in exploring and sounding the sun's atmosphere, Dr. St. John explained in an interview why the "relativists" and "non-relativists" have been watching for definite results from the work at Mt. Wilson.

"This 'third effect' predicted by the Einstein theory, the gravitational displacement of solar spectrum lines and its proof or disproof, has been the centre of great interest," Dr. St. John stated; "because among the mathematical physicists whose opinions carry great weight there has not been complete agreement.

"Einstein says the theory stands or falls according as the displacement exists or not. These latest results which I have been able to obtain at Mount Wilson show that it does exist in the amount predicted by him. Eddington considers the displacement of the Fraunhofer lines a necessary and fundamental condition for the acceptance of the theory, while Sir Joseph Larmor deduces that even according to relativity, the displacement should be only half of that predicted by Einstein."

Declares Sun's Gravitation Stronger.

Describing the "third effect" in less technical terms for the layman, Dr. St. John emphasized the important part played by the spectroscope in studying the modern problems of astronomy and physics.

"This instrument stretches the sun's light out into a beautiful band of prismatic colors, crossed by dark lines, the Fraunhofer lines," he explained.

"The features most important to the explorer of the sun are these dark lines, which he is able to interpret according to their changes in position and intensity and their behavior under different conditions. They are the code which brings to him the messages from the sun or some other more distant star.

"It is in this way that he is enabled to read the evidence that the sun is a stronger gravitational field than the earth, as it is revealed by the spectroscope, which shows the slight shift of these solar spectrum lines toward the red region, thus fulfilling the third prediction of Einstein."

One of the generally accepted theories apparently to be swept aside by the latest researches in relativity, Dr. St. John said, is that high pressure existing in the sun's atmosphere explained the difference between lines of the solar spectrum and similar lines produced in the laboratory. In the light of recent observations, the amount of pressure in the solar atmosphere is declared to be almost zero.

No Pressure in Sun's Atmosphere.

"This discovery of absence of pressure in the sun's atmosphere," Dr. St. John pointed out, "is a very significant step, in that it makes the lines of all elements available to investigation along new channels.

"Only a few years ago we thought the pressure in the atmosphere of the sun was at least 100 pounds to the square inch and that of Sirius three times that amount. In recent contributions from Mount Wilson Observatory on 'Pressure and Circulation in the Sun's Reversing Layer' and 'Convection Currents in Stellar Atmospheres,' it is shown that in the sun's atmosphere the pressure is so extremely low that displacements of the lines due to this cause are negligible."

He also pointed out that low pressure in the atmosphere of the sun is not peculiar to the sun, but only a particular example of stellar conditions as found in Sirius, Procyon and Arcturus.

"Of course we believe tremendous pressure must exist in the centre of the sun," said Dr. St. John, "but up to the present we have only been able to explore its outer envelope to a depth of about 10,000 miles."

In answer to the question as to how the confirmation of the Einstein gravitational shift would affect other questions still under investigation, the expert in solar physics said he felt sure it would have a very important bearing on the quantum theory.

Hopes to Utilize Stellar Energy.

"This opinion is also shared by Eddington," he continued, "and he goes so far as to say that if the displacement of solar lines is confirmed, it will be the first experimental evidence relativity holds for the quantum phenomena.

"To the man on the street who asks 'What is all this scientific investigation?' I should like to say that our study of the sun is aiming toward the discovery and mystery of great sources of energy stored up in the sun and stars for man's use," Dr. St. John declared.

"We are using up our sources of energy in the world and we must learn to store up the sun's energy or learn how to get energy out of matter. The world is going to be up against it some day unless we can find out how to do some of the things going on in the sun, that great unexplained engine of energy."

Asked to state his opinion of America's position scientifically among nations, Dr. St. John said:

"Of course Americans have long been recognized as great investigators rather than great originators. We have not yet developed the thoroughness and patience typical among thinkers of the older nations, due, no doubt, to our great energy and vigor as a young nation.

"But it is rather curious that in this country, with its reputation for materialism, so much money has been given for scientific research and equipment. To mention one example only, the observatory at Mount Wilson has the largest, most complete equipment and buildings of any observatory in the world."

ST. JOHN'S REPORT GRATIFIES EINSTEIN

But Exponent of Relativity Theory Seeks More Proof of the "Third Effect."

SILENT ON SOLAR PRESSURE

He Declares Mt. Wilson Sun Researches on Gravity Changing Spectrum "Valuable Evidence."

By T. R. YBARRA.

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By Wireless to THE NEW YORK TIMES.

BERLIN, April 26.—The news that Dr. Charles E. St. John of Mount Wilson Observatory had reported confirmation of the third effect predicted in the Einstein theory, namely, the gravitational displacement of the solar spectrum on the Fraunhofer lines, was received with great satisfaction by Professor Einstein today, as until a few months ago Dr. St. John was unconvinced of the reality of this effect.

"Hardly a year ago Dr. St. John was in this very room here and discussed the question with me," Professor Einstein told THE NEW YORK TIMES correspondent who brought him the news.

"Dr. St. John's work is certainly extremely valuable and makes the reality of the effect in question very probable now. His essay on the shifting of the spectral lines of the sun, which appeared some months ago, has already thrown much new light on the problem and he may since have made further progress.

"There is still no absolute certainty that the observations have proved the reality of this effect, although most experts who have worked along these lines are now convinced that the theory has been proved. Dr. St. John's work is additional and valuable evidence in favor of the theory, but I repeat, that even now the phenomenon is not quite explained because it has affected too many factors unknown as yet."

Professor Einstein was also told that Dr. John's observations had upset a generally accepted theory by showing the amount of pressure in the solar atmosphere to be almost zero, but he refused to comment on this until further details were available.

Figure 3. Facsimile of article in the *New York Times*, 25 April 1924, with the exaggerated headline "Proof of Einstein Theory," and of the careful reaction of Einstein two days later.

Beilage zur Vossischen Zeitung. 1924 29. April

Einsteins Gravitationstheorie bestätigt.

Die Rotverschiebung der Spektrallinien auf der Sonne.

Von

M. v. Laue,

ord. Professor der Physik an der Universität Berlin.

New York, 25. April. Der bekannte Sonnen-Physiker St. John vom Mount-Wilson-Observatorium erläßt heute eine Erklärung, die neuerlichen Forschungen auf diesem Observatorium hätten die völlige Bestätigung der dritten Voraussage in Einsteins Gravitationstheorie über die Abweichung der Linien des Sonnenspektrums ergeben.

Bekanntlich hat Einstein schon bei der ersten Grundlegung der allgemeinen Relativitätstheorie drei der astronomischen Beobachtung zugängliche Konsequenzen aus ihr gezogen. Erstens die Perihelbewegung des Merkur, die, als Tatsache lange bekannt, auf eine ungezwungene Deutung bis dahin vergeblich gewartet hatte, zweitens die Lichtablenkung an der Sonne, welche daraufhin bei den Sonnenfinsternissen von 1919 und 1922 gesucht und in guter Uebereinstimmung mit der Vorausberechnung gefunden wurde, und drittens die durch die Schwere verursachte Rotverschiebung der Spektrallinien auf der Sonne und jedem anderen Himmelskörper von ähnlich großer Masse und ähnlichen Dimensionen. Auch diese dritte Folgerung haben schon manche Physiker und Astronomen mit Beobachtungen der Fraunhoferschen Linien an der Sonne verglichen; z. B. die Bonner Physiker Grebe und Bachem im Jahre 1920. Aber ihre Ergebnisse, welche das Dasein der Verschiebung zu bestätigen schienen, fanden nicht allgemeine Anerkennung, weil so geringe Veränderungen in der Lage der Spektrallinien, wie die in Frage stehenden, auch durch mancherlei andere Ursachen hervorgerufen sein können. Als solche Ursachen kommen in Betracht einmal die hohe, mit irdischen Mitteln bisher unerreichbare Temperatur der Schichten auf der Sonne, in welchen die Fraunhoferschen Linien entstehen, sodann der dort herrschende Druck, weiter Strömungen in jenen Schichten (Dopplereffekt) und schließlich noch gewisse Erscheinungen, die mit der Brechung und Zerstreuung des Lichts in jenen Schichten zusammenhängen. Hinzukommt, daß die Deutung von Spektralaufnahmen, sobald es sich um die hier erforderliche Genauigkeit handelt, eine Kenntnis der spektrographischen Apparate und Methoden mit den ihnen anhaftenden Fehlerquellen voraussetzt, wie sie nur sehr wenige unter den Physikern besitzen. Die Spektroskopie, obwohl im Prinzip nur ein Zweig der Physik, ist deswegen dabei, sich in ähnlicher Weise selbständig zu machen, wie das andere Zweige, etwa Chemie und Mineralogie, *schon lange getan haben.*

In Fachkreisen wußte man, daß der vortreffliche Spektroskopiker der Sternwarte auf dem Mount Wilson, St. John, sich auf das lebhafteste für die Frage der Rotverschiebung interessierte; er stand der Deutung als Schwerewirkung aber anfangs recht skeptisch gegenüber. Die oben wiedergegebene Kabelmeldung weist die weitere Oeffentlichkeit darauf hin, daß er sich jetzt nach jahrelangen Bemühungen zu einer anderen Stellungnahme durchgerungen hat. Den Physikern war dies freilich schon seit dem Februar dieses Jahres bekannt, in welchem in der „Zeitschrift für Physik" eine Veröffentlichung darüber aus seiner Feder erschien, die bei dem Herausgeber dieser Zeitschrift am 6. Dezember 1923 eingegangen, also Ende November 1923 von ihrem Verfasser abgesandt war. Daß zu den Gründen, welche St. John dort anführt, noch weitere hinzugekommen wären, ist nicht bekannt. Deswegen sei hier eine kurze Mitteilung über den Inhalt jener Veröffentlichung gegeben.

Die früheren Beobachtungen der Rotverschiebung waren fast immer an gewissen Liniengruppen im blauen Teil des Spektrums gemacht, welche jedenfalls vom Stickstoff herrühren, aber meist mit dem irreführenden Namen „Cyanbanden" bezeichnet werden. Man wählte sie aus, weil sie nach der damaligen Kenntnis sich durch Temperaturveränderung nicht beeinflussen ließen, also eine der oben genannten möglichen Verschiebungsursachen fortfiel. Spätere Untersuchungen des Amerikaners King haben aber dargetan, daß die Temperatur einen zwar geringen, aber immerhin merklichen Einfluß hat. Deswegen knüpft St. John seine Erörterungen an das Eisenspektrum an oder doch wenigstens an eine Gruppe von 300 bis 400 unter den viel zahlreicheren Linien, die ihm angehören. Einen Einfluß der Temperatur zeigen diese nicht, und der Druck ist in den Schichten, in denen sie entstehen, so gering, daß auch er als Ursache einer Verschiebung fortfällt. Die Erscheinungen der Lichtbrechung, auf welche wir oben hinwiesen, müßten, wenn sie in Betracht kämen, die stärkste Verschiebung bei den Linien geringster Intensität ergeben; da davon nichts zu merken ist, scheiden auch sie aus der Diskussion aus. Und dasselbe gilt von der Zerstreuung des Lichts, sofern man sich auf Beobachtungen in der Mitte der Sonnenscheibe beschränkt. Denn die Schichten, welche dies Licht in der Sonnenatmosphäre zu durchlaufen hat, scheinen nicht dicht und nicht dick genug. Richtet man das Spektroskop freilich auf den Sonnenrand, so fällt Licht hinein, das wesentlich dickere Schichten durchsetzt hat, und daß man denn die Linien an anderer Stelle findet, als bei der Beobachtung der Mitte, deutet St. John als einen Zerstreuungseffekt.

Vergleicht man nun jene 300 bis 400 Eisenlinien, die von der Mitte der Sonnenscheibe zu uns kommen, mit den entsprechenden Linien im irdischen Eisenspektrum, so zeigen sie ausnahmslos eine Verschiebung zum roten Ende des sichtbaren Spektrums; und diese hat auch **ungefähr** die nach der Relativitätstheorie berechnete Größe. Teils sind sie größer, teils kleiner, sie schwanken zwischen dem 1½fachen des berechneten Betrages und ¾ davon. Diese Differenzen lassen sich aber ungezwungen auf Strömungen zurückführen, deren Geschwindigkeiten nach den heutigen Kenntnissen *durchaus möglich erscheinen.* Sähe man von der Schwerewirkung aber ab, wollte man also die Strömungen als einzige Ursache gelten lassen, so fände man Geschwindigkeiten von unwahrscheinlicher Größe. Das ist wohl der Hauptpunkt in der Beweisführung.

St. John faßt seine Meinung in die folgenden Sätze zusammen, die wir wörtlich übernehmen: „Linienverschiebungen, wie sie von der allgemeinen Relativitätstheorie vorausgesagt werden, vereinigt mit geringen Doppler-Verschiebungen, bieten die wahrscheinlichste Erklärung für die Unterschiede zwischen den Wellenlängen in der Mitte der Sonnenscheibe und den Wellenlängen des Bogens im Vakuum."

Figure 4. Facsimile of the report given by Max von Laue in the *Vossische Zeitung,* 29 April 1924.

turned out to provide some basis for Einstein's intuitive reservations about St. John's results. There were indeed more factors involved in addition to the three dominant ones to which St. John had confined his analysis;[22] I will return to them later. But Einstein's reaction remained an exception to the rule; most statements about the status of the search for GRS in the sun's spectrum were made in a self-confident, assertive tone — of course, the more affirmative the statement, the less the commentator knew about the very complex chain of reasoning that had led to St. John's claims in 1923–24. Certainly there remained some skeptics among astronomers and astrophysicists, and not all of them were just idiosyncratic, like Einstein, or dogmatic antirelativists, like L. Glaser, J. Riem, C. Poor or A. Reuterdahl, to name just a few. Some of them (e.g., Burns and Warga) had good reasons to contradict St. John's findings, and I will turn to their arguments in a moment. Warga brought forward other critical theses (see Warga 1928, 155), but somehow these afterthoughts no longer appeared in the popular and semipopular reports about the GTR and particularly GRS. From 1924 on, only additional "new confirmations of relativity theory" became known, such as Walter S. Adams' spectacular results about GRS in the spectrum of the companion of Sirius — a "white dwarf" with a ratio of mass to radius many times higher than the sun, and therefore with a redshift about twenty-five times as strong as the one in the sun's spectrum,[23] another result that was rediscussed only much later and found to be inadequate.

But we should not anticipate insights achieved only much later in our historical analysis. Judged from the point of view of a well-informed scientist around 1925, the convictions shared by the vast majority of physicists, astronomers, and astrophysicists tended toward the inclusion of GRS as one among several causes determining the observed shifts in the sun's spectrum. In this sense, GRS became a corroborated consequence of the GTR, just as light deflection in gravitational fields and the minute deviations of planets closer to the sun from their Kepler orbits had become experimental "proofs" for the GTR long before. And St. John did his best to encourage this — e.g., by writing review-like reports about the observational status of relativity theory that were not confined to his specialty of spectroscopic measurements but also included analyses of light deflection measurements since 1919 and repetitions of the Michelson-Morley experiments, etc.[24] A new boom of publicity came when St. John and his colleagues of the Mount Wilson Solar Observatory were visited by Einstein during his trip to the United States in 1931 (see figure 5). Einstein contacted St. John, who was certainly by that time the leading solar spectroscopist, to inform himself about the horizon of knowledge in solar physics (see St. John 1931a–b).

[22] See, e.g., Warga 1928, 155, or Mitchell 1936, 392 for a discussion of further element and intensity dependencies that would conform neither to Burns' intensity theory nor to St. John's level theory.

[23] See Adams 1925a–c as well as, e.g., "Chats in Science," 233ff., *Daily Science News Bulletin,* 12 September 1925: "Heaviest Little Star Upholds Einstein Idea." See also the discussion of the Sirius-B GRS in Hetherington 1980; Hetherington 1984, chap. 6.

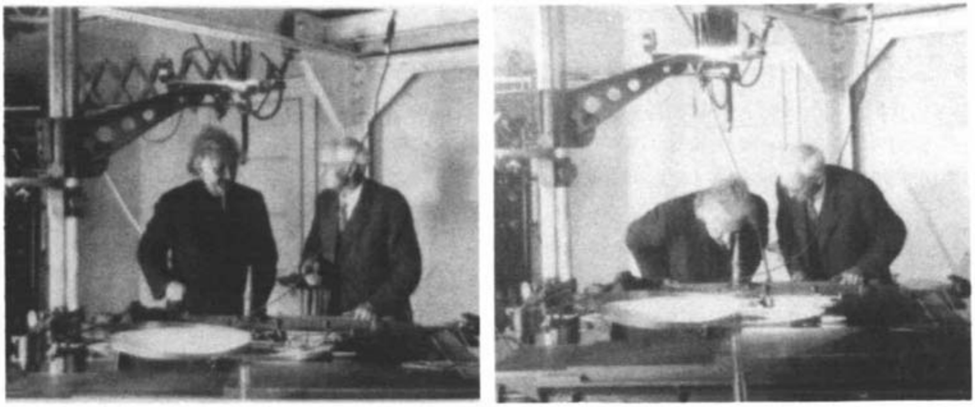

Figure 5. Photos made during Einstein's visit at Mount Wilson, on 29 January 1931. From Sugimoto 1989, 116.

In Germany, the response to St. John's results was by no means less enthusiastic than in the United States or in England. On 29 April 1924, under the headline "Einstein's Gravitation Theory Confirmed" ("Einsteins Gravitationstheorie bestätigt"), Max von Laue commented on St. John's "declaration" ("erlassene Erklärung"), according to which his research had led him to a complete confirmation of the third prediction of the GTR ("die völlige Bestätigung der dritten Voraussage"; see figure 4). Von Laue did not forget to underline the fact that it was St. John who had had quite a skeptical opinion about the empirical status of Einstein's theory, knowing well enough that this could only increase the perceived value of his changed convictions (von Laue 1924, 3).[25]

Turner as well as von Laue frankly employed the theme of the "conversion" of a skeptic, thereby further accentuating the importance of St. John's change of position. It is interesting to note the disparity between this rhetorical image and that of scientific experimentation. If the preconceptions and convictions of the individual experimenter were as unimportant and irrelevant as the common perception of intersubjective experiments suggests, then the fact that St. John corrected these "private opinions" about relativity theory around 1923 should be fairly irrelevant. But on the contrary, the popularity of the idea of his conversion demonstrates that the close connection between an experimenter's predilections, his theoretical opinions, and his occupation, on the one hand, and the results of his

[24] See St. John's talk on "the observational status of the theory of relativity," The Einstein Archives, 22 250, as well as St. John 1928a, 1930b, 1932.

[25] Two months before, on 2 February 1924, in the same newspaper, the *Vossische Zeitung*, an anonymous short notice about St. John's results had already appeared with a similar headline, but that time ending with a question mark: "A New Confirmation of Einstein's Theory?" ("Eine neue Bestätigung der Einsteinschen Theorie?").

experimental practice on the other, was already obvious to interested observers of science around 1925, not only to recent philosophers of science. Possibly, this is because around 1925, the boundary separating pro- and anti-relativists had already been dissolved for some time (at least since 1919 and possibly since 1910).[26] That is why St. John's case could arouse such wide interest: He had crossed a line between two clearly separated and well-defined camps.

The later debates focusing on St. John's results within the scientific community did not, however, reach the same level of semipopular or popular reports. Nevertheless, we should devote some time to them, since they contain lessons on the degree of consistency that St. John was able to reach in 1924. As always, by looking at the contemporary controversies we learn more about the possible weaknesses of scientific claims, because it is the potential rivals, educated with the same textbooks and with the same instruments, who can best recognize implicit assumptions, questionable experimental techniques, or other possible sources of error and confusion.

One of the most remarkable factors in these later intrascientific disputes concerns the appropriateness of St. John's application of anomalous dispersion to his explanation of the remaining center-limb shifts. There is an ironic twist to this, because it was St. John who, between 1913 and 1917, had published very critical studies about Willem Henri Julius' assertions (published between 1900 and 1925) that anomalous dispersion would play an important role in solar physics.[27] Among other things, Julius had claimed that the spectral shifts could be explained by anomalous dispersion, and St. John had refuted his claims as thoroughly as he could. But now St. John himself had used Julius' hypothesis, not mentioning his earlier "objections."

Still, it was Julius who in 1910 had pointed toward the possible influence of anomalous scattering of light (see Julius 1910c, 423–29). And although St. John did not find any evidence for Julius' predictions (as, e.g., the mutual repulsion of spectral lines), now in 1924 he suddenly needed this mechanism to explain the remaining puzzle of center-limb variations in the sun's spectrum. Moreover, it was precisely Julius who was most opposed to St. John's use of his own theory to account for the unresolved marginal effect. In one of his last articles, published a year before his death in 1925, Julius stressed the fact that anomalous molecular scattering could lead only to a *symmetrical broadening,* and not to an *asymmetrical shift* of absorption lines, because in the Rayleigh formula only the square of the index of refraction $(n - 1)^2$ appears (see Julius 1924; cf. Dirac 1925 about Compton scattering in stellar atmospheres).[28]

[26] For details about these disputes over relativity theory and its interpretations, see Hentschel 1990a, especially Sec. 3.4 about the martial metaphors often used in this context.

[27] See preceding footnote; cf. Hentschel 1991.

[28] As support for his refutation of St. John's arguments, Julius cited two review articles about GRS, Croze 1923 and Glaser 1923, in which St. John's conversion around 1923 was not mentioned.

Another important thread in the web of arguments about St. John's findings concerns the intensity dependence of shifts of absorption lines, as discussed by Warga (1928) or by Burns and Meggers (1926), all working at the Allegheny Observatory. The shifts observed by all of them did not follow the correlation as suggested by St. John's working hypothesis of a strong interdependence of line intensity and effective absorption level, nor did it fit with Burns' phenomenological intensity rule. The observed redshifts reached the order of magnitude predicted by the GTR only for line intensities of 8 to 40 (see figure 6; see also Warga 1928; Burns and Meggers 1926; Brunn 1930, 168ff.; Mitchell 1936, 392–94).

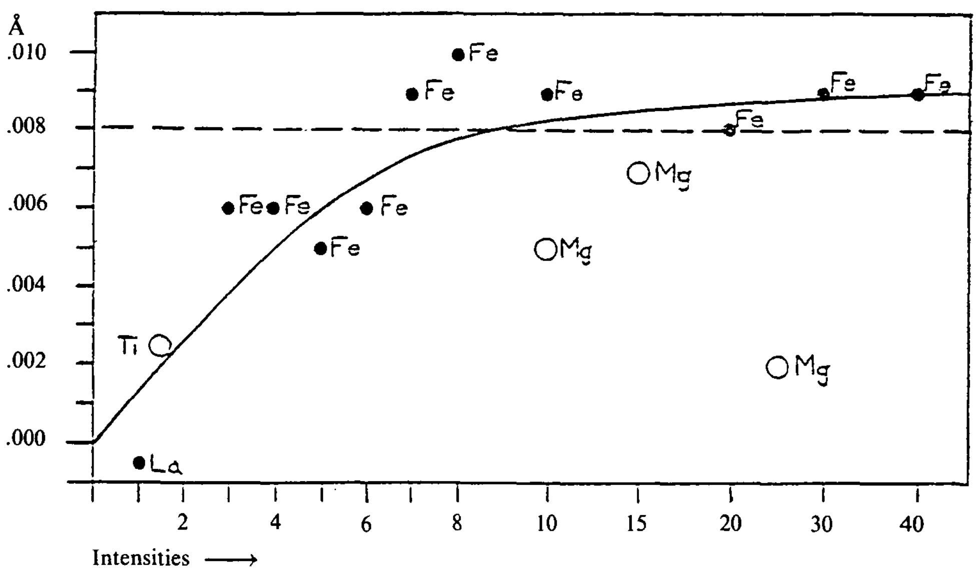

Figure 6. Differences between solar and terrestrial spectra, plotted against line intensities. From Warga 1928, 155.

The abscissas are Rowland's solar intensities; the ordinates are the mean values of solar minus vacuum arc wavelengths for the 3600 to 4100 Å region. The Fe shifts are those of Keivin Burns. The dotted line represents the mean shift predicted for that region by Einstein.

Systematic Evaluation of the Interplay of Theory and Experiment in This Case Study

In the attempt to clarify why St. John changed his views on GRS between 19 17 and 1924, I would like in this final section to analyze in greater detail the arguments that led him to this "conversion." I hope to be able to demonstrate that his conversion, though appearing at first like a sudden radical and irrational break with his former convictions, can be understood as a continuous and indeed very rational process of the weighing of arguments, with the aim of weaving them into a coherent network that could always be extended by future data and hypotheses.

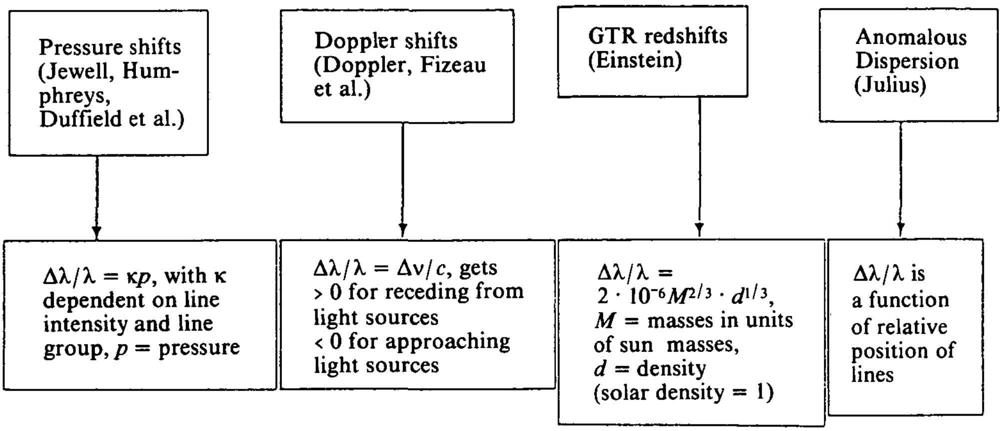

Figure 7. The four possible sources of shifts of spectral lines considered by St. John in 1917, and their specific consequences.

As a starting point for this analysis of St. John's argumentation, let us consider figure 7, illustrating St. John's point of departure in 1917. Confronted with these four possible causes of shifts of spectral lines, St. John did not opt blindly for any one of them; nor did he simply choose the one that best suited his purposes. On the contrary, he first tried to successfully eliminate all possible options, and later tried to extricate those that resisted elimination. His first steps in this elimination process are rooted in his own earlier work (before 1917), as well as in the work of many of his colleagues in the laboratories in Baltimore and Manchester (e.g., Humphreys, Duffield), who had closely studied the pressure dependence of spectral lines since 1895. He, as well as many other experimenters of the time, followed both options — namely, of choosing lines that were considered to be pressure-independent and of correcting for the pressure dependence by comparing lines showing different degrees of dependence.[29] Because he had so much data and so many reliable studies about this effect, he could in fact isolate this dependence from all the others, relying on arguments that are even today fundamentally sound. As for the other possible causes, such an easy approach would not work. Nevertheless, he tried it with Julius' theory of anomalous dispersion, which he considered by 1917 to be fundamentally wrong and in contradiction to his own data. That is why he at first put this theory (and its presumptive consequences)

[29] As did, for instance, Karl Schwarzschild at the Astrophysikalisches Observatorium in Potsdam, or John Evershed and his collaborators at the Kodaikanal Observatory and later also E. F. Freundlich at the Einstein Tower in Potsdam.

aside, only to take it up again much later, after all his efforts to do without it had failed.

When St. John first encountered Einstein's GTR around 1917, he must have thought of it in terms very similar to Julius' theory: Both theories appeared to him to be a real danger to the whole business of high-precision spectroscopy, because both implied (among many other consequences not so interesting for St. John at that point) shifts of absorption lines in the sun's spectrum away from their "true" value. If these effects were "real," then the values of Fraunhofer lines that he had published in extensive tables were not the "real" values but would first have to be corrected for these effects.[30] That is why St. John had devoted so much energy to the search for conclusive arguments against Julius' theory of anomalous absorption, and that is also why he invested even more time in the search for equally convincing arguments against the GTR's redshift. He could not live with the uncertain conjecture that GRS was a real phenomenon; what he needed was a clear statement about whether GRS did or did not exist in the sun's spectrum.

How did St. John approach this principal aim, which had already been the goal of many other skilled experimenters before him — all of whom had failed to produce an unambiguous answer? He first asked himself which theories, apart from the one being tested (GTR), would imply experimental consequences of a comparable or even equal order of magnitude. Certainly there was the possibility of Doppler shifts. Because the GRS prediction of the GTR was so small, it could be mimicked by convection currents of minute order (only 0.6 km/s, which is nothing unusual in the atmosphere of a star with a high temperature core and a rapidly cooling gaseous atmosphere from the core outward). Both effects were proportional to the first power of the wavelength. And yet there was one difference in the behavior of the effects according to both theories: The GTR predicted a gravitational redshift value dependent *only* on the ratio of mass to radius for each star, while the Doppler effect should, according to classical electrodynamics, vary with the point on the sun's surface selected by the spectroscope.

In 1917, St. John utilized all the experimental and theoretical information at his disposal to construct the following indirect "proof" against the existence of GRS,[31] here only sketchily summarized (see the earlier sections of this paper).

Specifically, St. John's 1917 indirect proof against the GTR redshift went as follows: Assuming a redshift in the order of 0.008Å exists in the cyanogen bands of the solar spectrum, then the very much smaller means of absorption from these

[30] In the same way in which earth-produced spectra had, for instance, to be corrected for the so-called pole effect, etc.

[31] The word "proof" has been placed in quotation marks since there certainly is a difference between a mathematical proof and a physical demonstration, which will never be more than convincing. I would like only to point to the clear logical structure of his arguments of 1917, closely resembling the form of indirect proof — in which some assumption is made and consequences are drawn from it until an unresolvable contradiction appears, which then shows that the initial assumption was false, thereby demonstrating that its converse is true.

bands for the lines chosen by St. John must thereby be explained, since countering Doppler shifts compensate exactly for the relativistic redshift effect. Out of this hypothesis follow quantitative conclusions about hypothetical convection currents in the reversion layer (0.63 km/s upwards). Furthermore, a definitive conclusion about the center-rim variation of this hypothetical Doppler redshift can be drawn from this: It must disappear at the rim, which means that the full relativistic GRS effect should then be measurable. That this did not agree with experimental data was final proof for St. John in 1917, since the hypothesis of the existence of GRS could not be maintained, even with the help of the following supporting hypotheses:

1. Elimination of pressure effects by the choice of lines that were demonstrated to be pressure-independent (cyanogen band).
2. Elimination of anomalous dispersion with the argument that this effect could be nothing but sporadic.
3. Discrimination between Doppler shifts and gravitational redshifts as follows:

Theory: GRS is a *theoretical prediction* of a shift of 0.008Å for the sun's visible light in the green region, which is independent of all other parameters and the equivalent of a Doppler shift induced by a move of the light emitter away from the observer with a speed of 0.634 km/s.

Observations: Particularly clear, sharp, and easily measurable lines in the cyanogen band show very small, in the average zero, redshifts.

Conclusion: Should there still be a GRS, this could only be if the GRS were *masked* by a numerically equal but counteracting shift to the violet, induced by hypothetical convection currents toward the observer, which would mean an outward flow of the gases in the reversion layer in the order of 0.6 km/s.

This Doppler violet shift should be maximal for rays from the sun's center, and it should be vanishingly small for rays stemming from the limb of the sun's disk, because here the hypothetical radial convection current is vertical to the line of sight.

This in turn would mean that at the sun's margin the full gravitational redshift should appear, unmodified by any possible Doppler shifts due to radial convection currents.

Now, observations of the center-limb variation of the sun's absorption spectrum show a slight dependence, but not so drastically as would be expected from the above considerations. The observed average shifts, around zero for central rays, are on the average not much more than 0.0018Å for marginal rays — that is, less than one-quarter of what we would expect from the GTR's prediction.

Ergo, St. John concluded in 1917 that the attempt to conform the GRS to the observations and to the whole structure of his background knowledge had failed.

Reconsidering this rather involved chain of reasoning, let me just draw attention to one feature that I find especially remarkable. To pursue his goal of discriminating between GRS and hypothetical Doppler shifts, it was absolutely essential for St. John to include the center-limb shifts in his discussion, because without them he would not have had any option for possibly disentangling these two phenomenologically similar effects. This is an extension of the data involved in the discussion and an accompanying move in the data analysis toward a field of study that had long remained comparatively isolated and had only been relevant to research on the rotation of the sun.[32] It is in this move toward another field of research, both in the observational and in the theoretical side of scientific questions, that I see a typical feature of scientific progress. By following this technique, hitherto unrelated fields of research are connected and woven into a network of scientific knowledge of ever increasing complexity. The more nodal points there are in this web, and the more complex the resulting patterns in the scientific networks become, the more difficult it is to weave new knowledge into this net. This explains the increasing tendency of science to repel certain hypotheses offered by alternative theories such as those by Julius or Einstein. While Julius did not succeed in integrating his theory into the network of astrophysical knowledge of his day, Einstein did finally succeed, with the help of scientists such as St. John, who took on the painstaking work of looking for ways in which a harmony of the new theory with the existing background knowledge could be achieved. Because this involves a considerable amount of reweaving of sections of the original web and an accurate overall view as to where the net might be slightly changed while avoiding too drastic consequences at other parts, it has only rarely happened in more recent science. This "network" metaphor for scientific research, utilized more recently by scholars as varied as Mary Hesse, John Law, Bruno Latour, Larry Laudan and Paul Thagard, also explains the increasing importance of instances of rejection.[33] A metatheoretical consequence of this situation is the rise of Popper's falsificationism, which took the place of earlier inductivistic or empiricist models of scientific progress, all of which had put their stress on the confirmation rather than the rejection of hypotheses.

Allow me at this point to simplify further. It seems to be a characteristic of scientific argumentation that new results are built into existing well-established chains of reasoning on a tentative basis (here it is the hypothetical presence of GRS), which is then followed by a check of the consistency of the new intersection point of the whole network of hypotheses, starting with those assumptions most intimately connected with the new hypotheses and then proceeding to the indirectly coupled ones. Certainly, the metaphors employed here — "network," "context," and the relations of proximity within this "network" — are all still quite vague, because the description of scientific knowledge as a network rather than as a

[32] See, e.g., the pioneering paper by Halm 1907 or, e.g., Buisson and Fabry 1910; Adams 1910.

[33] For instance, the overwhelming majority of experiments on gravitation around 1960 served only to exclude theoretically possible alternatives to the GTR, none of which survived; the same is true for high-energy physics experiments after the establishment of the so-called standard model of electroweak interactions.

linearly ordered chain from experiments to high-level theory, has not yet been developed very far. St. John's approach seems to be a case in point for this model of scientific reasoning: The four initial hypotheses to explain shifts of spectral lines were *not* discussed separately by St. John but rather combined within a network of steadily increasing complexity. We saw in particular how the possible compensation of one effect by another one (GTR's redshift and Doppler violet shifts) forced St. John to take into account a further physical phenomenon (the center-limb shifts), hitherto discussed independently. In more recent empirical research the resulting network of interrelated hypotheses has become even more complex than St. John's, just because with an increased number of intersection points, the number of mutual connections that must be checked for consistency increases proportionally. This case study should hopefully at least have made clear the method by which these networks are woven.

We find further support for this model of scientific endeavor in the fact that St. John did not conclude his paper of 1917 without pointing out that the results presented at that time were consistent not only with each other but also with earlier results published by him in connection with Hale's work about the spectroheliograph (see St. John's papers 1910–13). These prewar results had led to estimates about the radial convection currents in the sun's atmosphere very different from the hypothetical motions of the absorbing gases needed to compensate for the GRS. This meant that hypothetical Doppler shifts would have to be introduced only to explain away the existence of the GRS predicted by the GTR: It would be an ad hoc hypothesis without any independent justification apart from covering the one fact it was designed for. Such ad hoc hypotheses have always had a bad standing in scientific theories; now we see the reason for this methodological premise: They refuse to link up with the otherwise more strongly interconnected hypotheses in the network. The pertinent strategy to emerge out of all these considerations is the following: *Any hypothesis is tested by its tentative inclusion in the preexisting network of knowledge; it is then checked for its consistency with the rest of the network.*

I shall now demonstrate that this conclusion, derived from St. John's case up to 1917, applies equally well to his changed argumentation in 1923. Instead of an apparently sudden and irrational "conversion," remarked on at the beginning of this paper, the change from 1917 to 1923 is revealed as a gradual and rational extension of the network of consistent relations, to which GRS is added in 1923 as a useful and no longer disturbing intersection point. One might object at this point that a very important difference between St. John's 1924 measurements and the earlier ones is the fact that he now had 100 lines instead of 43; but it would not be fair to St. John's own remarks about the improvements he made during those six years for us to try to seek, in an empiricist manner, the reason for his changed interpretation just in the increased size of his data base. More different than the change in the data was St. John's interpretation of them.

At the outset, St. John's argumentation in 1923 looked pretty similar to the one in 1917: He tried to exclude the pressure dependence by choosing a pressure-independent group of lines; he again considered possible compensations; and he once more included the center-limb variations. The decisive difference from his former argument was the inclusion of one further step, in which he linked up with yet another field of knowledge not previously included in his considerations. The way in which this was done is very similar to the one discussed earlier. Whereas in 1917 he had allowed for a possible compensation of GRS by a counteracting Doppler shift, now in 1923 he allowed for yet another effect (anomalous scattering) to modify the observed center-limb variations, which did not accord with the sinusoidal dependence of the angle between observer, sun center, and surface point chosen for spectroscopic analysis that one would expect of simple radial convection currents. In both cases St. John took into consideration the possibility that the pure, "simple" effects expected from the theories might be shielded or deformed by superimposed effects. And in both cases the problem he had to deal with was how to most obviously disentangle the physical dependencies, all superimposed on one another.

Seen from this perspective, the most important insight gained by St. John in 1923 was the realization that anomalous scattering might change the observed frequency of light rays that pass through the sun's outer atmosphere. This hypothesis was of heuristic value, since it suggested a new dependence of the data on one parameter: Selective scattering of light should be larger for rays coming from the limb areas of the sun's atmosphere than for central rays, because for purely geometrical reasons the former have a much longer way through the gaseous atmosphere of the sun before they reach the earthbound observer. This specific dependence anticipated for the new effect was ideally suited to be superimposed on the usual center-limb variations, as one would expect them from convection currents; it helped diminish the resulting difference between center and limb wavelengths, which would otherwise have been too large in comparison with the data at his disposal. In other words, St. John succeeded in constructing a hypothetical, but at least fairly consistent, *model of GRS ⊕ Doppler redshift, induced by layer-dependent convection currents ⊕ previous knowledge about pressure (in)dependence of spectral lines and bands ⊕ center-limb variations ⊕ the theory of selective molecular scattering.* Figure 8 depicts the extension of the argumentative web, enriched by the further (dashed) argumentational loop. Once again, it also clarifies the analogy to the former improvement of the argument. In spite of the drastic difference in the final conclusions, the method by which they had been drawn is one and the same.

I do not wish to imply that St. John's results of 1923–24 were forced upon him by the impetus of drawing consequences from previously made premises. Though I want to stress the many implications each hypothesis has when embedded into a network of others, there still remains a place for the scientist's intuition about where to look for coherent relations, where to extend the existing web of assumptions, and where not to make changes. Imagine that St. John had not been aware of the theory of scattering of light by small particles as formulated by Lord Rayleigh and Arthur Schuster (see note 17 above). He would then not have had the chance to harmonize the overall network of assumptions in the way he did in 1923. Suppose he had had similar prejudices against that theory, as he had against the GTR around 1917; he would then very probably have chosen another route of argumentation, either looking for yet another possible superimposed effect or

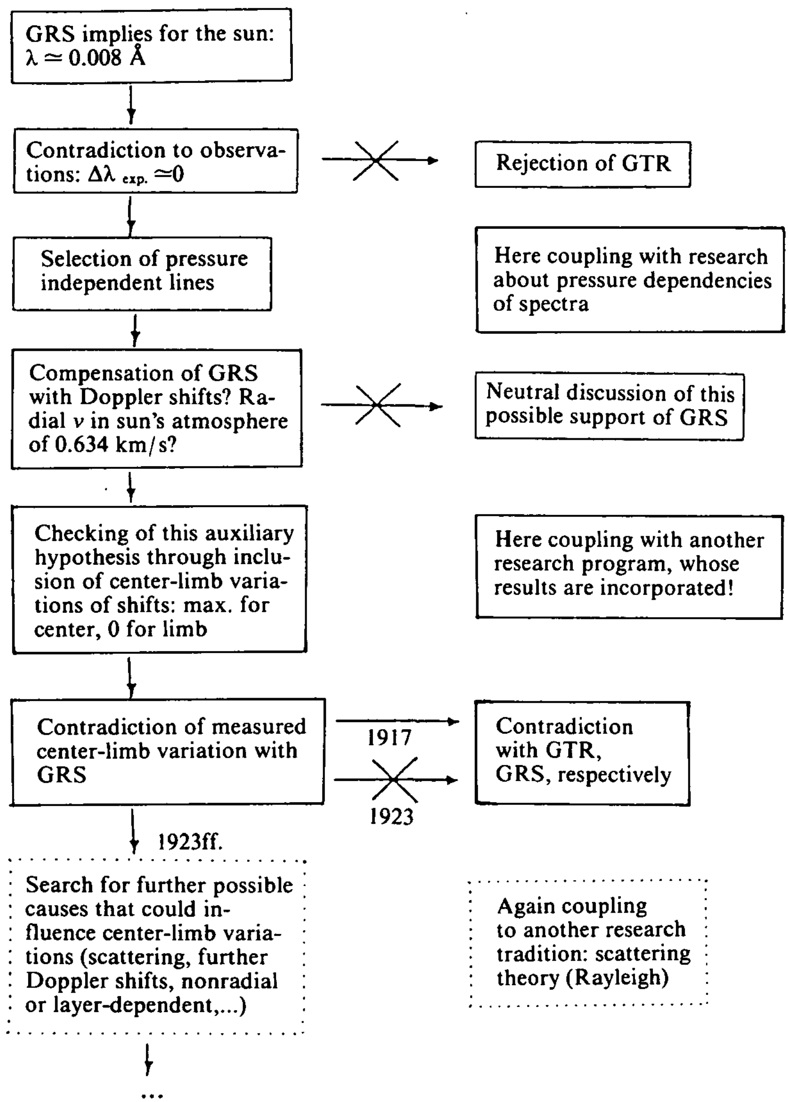

Figure 8. Survey of the chain of reasoning in St. John (1917); In the dashed boxes, are the continuations of St. John's arguments after 1923–24. In the right column are the reasons a line of thought was not pursued further. GRS again means "gravitational redshift; GTR, "general theory of relativity and gravitation."

again coming to the conclusion that GRS was not present in his data.[34] So there is some elbowroom in the fixing of argumentation; that is another reason why scientific results are open to revision, in principle. On the other hand that is also why corrections of earlier claims, even if as dramatic as St. John's revoking of his own thesis of 1917, are not necessarily drastic ruptures with the past. St. John's case proved to be the result of an expansion of the network of theoretical assumptions as well as of experimental data woven into it through reasoning.

It may very well be that *external* circumstances had motivated St. John to change his opinion about the status of the GTR and to take more seriously its prediction of a GRS — for example, the improvement of other experimental checks, such as the light deflection test carried out by Eddington and Crommelin in 1919 resulting positively, and the outcomes of repetitions of the Michelson-Morley experiment carried out at Mount Wilson in the twenties, mostly with results confirming the theory of relativity. More insight into his motives may well be gained from other correspondence. But St. John's remarks in letters to Hale and Shapley are very much confined to *internal* arguments, all within the context of spectroscopy. Whatever other hidden motivations St. John might have had, it should be clear that his arguments given in 1917 and in 1923 were much more than mere rhetorical papier-mâché, used only as the arbitrary packaging of prefabricated arguments: they were, rather, the protocols of a process of reasoning, each element of which could determine the success or failure of the whole theory if found inaccurate or unessential.

One final remark: In many places, this paper employs imprecise metaphors, such as "network," "web," "hierarchy," etc. To be more precise, one would need a model of scientific reasoning that (in both its theoretical and its experimental aspects) would *not* be reduced to mere strings of argumentation (from artificially isolated theoretical axioms down to equally artificially isolated experimental results, as e.g., in the DN or the Hempel-Oppenheim models). Rather, such a model would allow for the concise understanding of all the complicated interrelations between assumptions of varying generality belonging to different fields of knowledge, yet all woven together into a broader system of knowledge (e.g., in this case study, bringing together the GTR and classical electrodynamics plus further auxiliary theories such as scattering theory, later also quantum mechanics, etc.). What we need is a better understanding of how science is "networking" (as Bruno Latour puts it); we need to know more precisely what we mean when we say that two assumptions are closely related to each other; we need to be more clear about "degrees of relevance" of one experimental result for different parts of a theory. We might be able to achieve all this by making further use of the *network model* of science, because in networks we have relations of proximity and distance, depending on the number of nodal points between two

[34] St. John himself speaks about his interpretation of his redshift measurements as a "harmonizing interpretation" (St. John 1926, 65).

components of it. Furthermore, each network has a characteristic pattern according to which it has been constructed, and it might be very interesting to compare these patterns for different theories or different stages of one theory. The descriptive impact of such a model will be larger than idealized typologies of scientific endeavor such as inductivism, deductivism or fallibilism, which were constructed only with a linear model of scientific reasoning in mind — starting either from the conclusion (or end) to the point of departure (or beginning), or vice versa.

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Bibliography

List of Abbreviations

AAAS	*Australian Association for the Advancement of Science* (Sydney)
AN	*Astronomische Nachrichten* (Kiel/Berlin)
Ann. Chim. Phys.	*Annales de Chimie et de Physique* (for some time also: *Annales de Physique*) (series) (Paris)
Ann. Sci.	*Annals of Science* (London)
ANSEN	*Archives néerlandaises des Sciences exactes et naturelles publiées par la Société Hollandaise des Sciences à Haarlem* (series) (The Hague/Haarlem)
APJ	*Astrophysical Journal. An International Review of Spectroscopy and Astronomical Physics* (Chicago)
ARDMtW	*Annual Report of the Director of the Mt. Wilson Observatory* (Pasadena), for some time reprinted in the Carnegie Institution Yearbook (Washington)
AVCIF	*Atti del V Congresso Internazionale di Filosofia* (Naples) (1924)

BBS	*Bulletin of the National Bureau of Standards of the USA* (Washington)
BMNAS	*Biographical Memoirs of the National Academy of Sciences of the USA* (New York)
CJPL	*Contributions of the Jefferson Physical Laboratory* (Cambridge, Mass.)
C.Mt.W.	*Contributions from the Mt. Wilson Solar Observatory* (Pasadena)
CRAS	*Comptes rendus hebdomadaires des séances de l'Académie des Sciences* (Paris)
DSB	*Dictionary of Scientific Biography*, 1970–1980. Edited by C. C. Gillespie. 16 Vols. New York: Charles Scribner's Sons
HSPS	*Historical Studies in the Physical and Biological Sciences*
JOSA	*Journal of the Optical Society of America and Review of Scientific Instruments*
J. Phys.	*Journal de Physique théorique et appliquée* (series) (Paris)
JRASC	*Journal of the Royal Astronomical Society of Canada* (Toronto)
JRE	*Jahrbuch der Radioaktivität und Elektronik* (Leipzig)
KOB	*Kodaikanal Bulletin* (Madras)
MKO	*Memoirs of the Kodaikanal Observatory* (Madras)
MNRAS	*Monthly Notices of the Royal Astronomical Society* (Oxford)
Natw.	*Die Naturwissenschaften. Wochenschrift für die Fortschritte der Naturwissenschaften, der Medizin und der Technik* (from Vol. 10: *für die Fortschritte der reinen und angewandten Naturwissenschaft)* (Berlin/Heidelberg)
NYT	*The New York Times* (New York)
Obs.	*The Observatory* (Hailsham, Sussex)
P. Amst.	*Proceedings of the Section of Sciences* (Amsterdam) (English translation of VKAWA)
PAO	*Publications of the Allegheny Observatory of the University of Pittsburgh* (Lancaster)
PASP	*Proceedings of the Astronomical Society of the Pacific*
Phil. Mag.	The London, Edinburgh, and Dublin *Philosophical Magazine* and Journal of Science (var. series) (London)
Phys. Rev.	*The Physical Review. A Journal of Experimental and Theoretical Physics* (var. series) (Ithaca/Lancaster)
PNAS	*Proceedings of the National Academy of Science* (Washington)

Pop. Ast.	*Popular Astronomy* (Northfields, Minnesota)
PRSL	*Proceedings of the Royal Society, London* (series) (London)
PTRSL	*Abstracts of the Papers Printed in the Philosophical Transactions of the Royal Society* (London), Ser. A (Math. & phys. sciences)
QJRAS	*Quarterly Journal of the Royal Astronomical Society* (London)
SB. Berlin	*Sitzungsberichte der Königl. Preussischen Akademie der Wissenschaften* (from 1922, its mathematical-physical class) (Berlin)
SHPS	*Studies in History and Philosophy of Science* (London)
TIAU	*Transactions of the International Astronomical Union* (Manchester)
Verh. d. D. Phys. Ges.	*Verhandlungen der Deutschen Physikalischen Gesellschaft* (Berlin/Weinheim)
ViA	*Vistas in Astronomy, an International Review Journal* (Oxford)
VKAWA	*Verslag van de gewone Vergaderingen der wis- en natuurkundige Afdeeling Koninklijke Akademie van Wetenschappen te Amsterdam* (Amsterdam)
VZ	*Vossische Zeitung* (Berlin) (A=evening ed.; M=morning ed.)
WRPMDP	*Walther-Rathenau Program Multidisciplinary Discussion Papers* (Berlin)
Z. Phys.	*Zeitschrift für Physik*, published by Die Deutsche Physikalische Gesellschaft as a supplement to their proceedings; *Verhandlungen* (Braunschweig/Berlin)

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