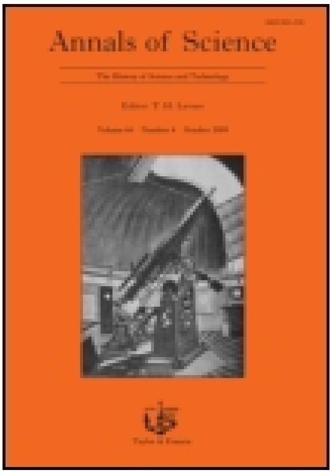

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Measurements of gravitational redshift between 1959 and 1971

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Measurements of Gravitational Redshift between 1959 and 1971

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Summary
The paper presents and discusses measurements of gravitational redshift (GRS) made between 1959 and 1971 by Pound and Rebka, Schiffer and Marshall, Brault, Blamont and Roddier, and finally by Snider. It emphasizes the importance of new measurement techniques such as wavelength modulation, electronic amplification, and scattering of atomic beams to the emergence of new tests of Einstein's GRS prediction, which were perceived by the scientific community as the first 'clean' verifications of GRS. In particular, the race to be the first to apply the Mössbauer effect to the GRS problem is described. As soon as the Mössbauer effect was stabilized, it was transformed into a measurement technology that in turn triggered new types of experimental tests of GRS.

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1. Research on gravitational redshift (GRS) prior to 1955

As is well known, gravitational redshift (henceforth abbreviated as GRS) was first predicted by Einstein in his survey paper for Stark's *Jahrbuch der Radioaktivität und Elektronik* in 1907, and reiterated in the framework of his 1911 Prague theory as well as in his 1915–16 general theory of relativity and gravitation (henceforth referred to as GTR).[1] Since the effect depends only on the equivalency principle as opposed to the full-fledged GTR, the 1907 prediction did *not* have to be altered (unlike the value for light deflection) but was stable throughout the entire complex development of the theory. In sharp contrast with the *theoretical* stability of this testable consequence of GTR, the *empirical* question as to whether this effect exists in the spectrum of the sun and other bodies with high gravitational potential $\phi \sim M/R$ was far from stable

[1] Einstein [1907], [1911], and, for example, [1916]. A name with the year in brackets refers to a citation in the Bibliography.

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throughout the second and third decade of this century.[2] From the mid-1920s onwards, interest in this question fell drastically, certainly due in part to the emergence of quantum mechanics, and only a few specialists occasionally tried to measure the GRS between 1925 and 1955.[3]

In the following I will deal with the revived interest in measurements of GRS *after* 1955 that led to a whole sequence of new experiments, the results of which were published between 1959 and 1972. If we disregard the complex issue of GRS in the Sirius-B spectrum (studied historically in detail by Norris Hetherington), we can say that it was only by means of these measurements that a 'clean' confirmation of Einstein's prediction of the GRS could be achieved. From a historiographic point of view, this time period has, up to now, been much less the focus of investigation than the first half of this century. While the correspondence files and lab notebooks of all the experimenters figuring prominently in the following account are not yet open to historical investigation in public archives, two of the main actors were so kind as to answer detailed questions relating to their former work and provided me with information I could not have obtained otherwise.

2. Terrestrial confirmations of GRS

2.1. *The Mössbauer effect*

From 1955 on, a young graduate student at the Munich Polytechnic, Rudolf (Ludwig) Mößbauer (b. 1929), worked at the Physics Department of the Max Planck Institute for Medical Research with the aim of realizing γ resonance absorption in nuclei.[4] Most other experimenters at the time thought such an endeavour a waste of time. Those few who were on the same track tried instead to *increase* the temperature of their γ-ray emitters to enhance the probability of resonance absorption due to an increased line width broadened by the Doppler effect. Mössbauer disregarded this common wisdom and *lowered* the temperature of his emitter to 90 K. When, instead of the expected decrease, he found an increase in the absorption rates, he was at first puzzled. What happened, expressed in a crude qualitative way, was that at low temperatures some of the emitting and absorbing atoms are not able to recoil in the crystal lattice so that the recoil momentum is taken up by the entire crystal. This means that the emitting γ-ray loses virtually no energy to the recoiling system and that no energy is lost on recoil in absorption so that the usual hindrance to resonance absorption is removed. Even after he was finally convinced that he had found a new way to produce recoilless γ resonance absorption, the idea still sounded absurd to most of his colleagues.[5] Energy resolutions of $E/\Delta E \sim 4 \cdot 10^{10}$, as Mössbauer had obtained them in his analysis of the 129 keV transition in the Iridium isotope 191,

[2] Compare, for example, Earman and Glymour [1980] as well as Hentschel [1993a] on the efforts of Charles Edward St John at Mt Wilson Solar Observatory from 1916 onwards; compare also Forbes [1961], and Hentschel [1994, 1995] on many other futile attempts by solar spectroscopists such as John Evershed at Kodaikanal Observatory and Erwin Finlay Freundlich at the Potsdam Einstein Tower to get a clearer confirmation of this effect in the solar spectrum.

[3] Eisenstaedt [1986] has coined the expression 'low water mark' of empirical tests of GTR to describe this situation.

[4] On Mößbauer see, for example, F. v. Feilitzsch, in Magnus (ed.) [1993], 107; since his name is usually spelled 'Mössbauer' in Anglo-Saxon texts, I will stick to this tradition in the following. The option to work at the Heidelberg Max-Planck Institute with its much better instrumentation (including a cyclotron) was given to him owing to the connections of his supervisor, Heinz Maier-Leibnitz, to the head of the MPI, Walther Bothe.

[5] On the history of the discovery of the Mössbauer effect, for example, his Nobel prize speech: Mössbauer [1962] as well as Frauenfelder [1962], Wegener [1966], and Hentschel [1995], §11.2.2, especially 813, footnote 65 on Mössbauer himself.

were then almost unheard. One of the few exceptions was Robert W. Dicke at Princeton who had a similar idea at the time but did not follow it up; according to his own later testimony: 'I considered narrowing γ-radiation from radioactive nuclei in a solid in this way, but unlike Mössbauer did not think that it was feasible'.[6]

In the months following Mössbauer's first publications in the spring of 1958, the scientific community did not respond since most physicists were still undecided as to whether to believe the newcomer's claims or not.[7] On 1 September 1959, however, the first two experimental confirmations of Mössbauer's results gained with the 129 keV transition of Ir^{191} which allowed an energy resolution $E/\Delta E \simeq 4 \cdot 10^{10}$ appeared in the *Physical Review Letters*,[8] and in 1960 the enormous jump in the scale of experimentally resolvable energy $E/\Delta E$ became obvious. Owing to the use of other nuclear isotopes such as the 14·4 keV transition in Fe^{57} in particular,[9] energy resolutions of $E/\Delta E \sim E_0/\Gamma \sim 3{\cdot}2 \times 10^{12}$ were reached independently at the Lyman Laboratory of Physics of Harvard University, at the Atomic Energy Research Establishment in Harwell (England), at the University of Illinois in Urbana and at the Argonne National Laboratory. Thus a 100-fold reduction in the energy resolution compared with the example reported by Mössbauer and others was achieved.[10] This transition of the iron 57 isotope could be induced in comparatively easy-to-prepare radioactive Co^{57} sources mounted on iron discs at temperatures up to 200°C, later even up to 1000 °C, and the absorbers were also simply iron discs with a 75% Fe^{57} constitution. Furthermore, even at room temperature a considerable portion of the nuclei underwent recoilless γ emission.[11] Thus from 1960 on, applications of this new effect of recoilless nuclear γ-ray resonance absorption, also simply called the Mössbauer effect,[12] boomed, most prominently of course in nuclear physics and solid state physics (halflife measurements, nuclear hyperfine structure, nuclear Zeeman effects, isomeric shifts, nuclear magnetic dipole and electrical quadrupole moments, parity experiments, oscillation modes of crystals and ferromagnetism). But applications of the Mössbauer effect were also found in quite different fields of physics; among these, measurements of a possible anisotropy of inertia, of second-order Doppler effects and other effects of GTR. In 1962 the first textbook on the Mössbauer effect appeared. This book compiled the most important original papers published up to December 1961.[13] A contemporary summarized the long 2-year incubation period from first publication to surge in the big wave of applications (Table 1).

[6] Dicke, in an as yet unpublished scientific autobiography, archived at the American Institute of Physics (henceforth abbreviated AIP), dated January 1975, p. 6.

[7] According to Fraunfelder [1962], 12, the conformation of Mössbauer's experiments in Los Alamos was only the result of a 5 cent bet on whether Mössbauer was right with his claims.

[8] Directly succeeding one another: Craig *et al.* [1959], and Lee *et al.* [1959].

[9] With $E_0/\Gamma \sim 5 \cdot 10^{-13}$ and a half-life of about 10^{-7} s.

[10] On Fe^{57}, which is generated from cobalt 57 by β decay, see Pound and Rebka [1959b], 555: 'our experimental width at half-height is approximately 10^{-12} times the velocity of light and represents a 100-fold reduction compared to the example reported by Mössbauer and others'. Schiffer and Marshall [1959], DePasquali *et al.* [1960], Hanna *et al.* [1960a]; compare also Mössbauer [1961], 151f. and Frauenfelder [1962], 36ff., Barloutaud *et al.* [1960] on other isotopes which can be used for Mössbauer measurements, among them the 92 keV transition in Zn^{67} and the 23·8 keV transition in Sn^{119}.

[11] Holland *et al.* [1960], 181, and Schiffer and Marshall [1959], 556 estimate 63% γ quanta emitted recoillessly; compare both, as well as Hay *et al.* [1960], 165 or Cranshaw *et al.* [1960], 163, for descriptions of the preparation of the sources of radiation.

[12] The first mention of the term 'Mössbauer effect' might have been made by his doctoral adviser Heinz Maier-Leibnitz (1958); compare also Hanna *et al.* [1960], Cranshaw *et al.* [1960], and Lipkin [1960], etc.

[13] Frauenfelder [1962], especially the bibliography, 86–96, and the Conference Proceedings mentioned ibid., 96; compare also Mössbauer [1961], 151: 'Die Bedeutung der neuen Methode lag sofort, wenn auch nicht in allen Konsequenzen auf der Hand', and Edingshaus [1986], 114ff. For early reviews of possible applications see, for example, Barit *et al.* [1960], Cotton [1960], Basov *et al.* [1962], Maier-Leibnitz [1964], 288ff., and Wegener [1966].

Table 1. Phases on the discovery of the Mössbauer effect, remarks by Lipkin to Frauenfelder (Frauenfelder [1962], 13).

Period	Date	Remarks
Prehistoric	before 1958	Might have been discovered, but was not
Early Iridium age	1958	Discovered, but not noticed
Medium Iridium age	1958–59	Noticed, but not believed
Late Iridium age	1959	Believed, but not interesting
Iron age	1959–60	Wow!

2.2. *Application to GRS in the Earth's gravitational field*

The Mössbauer effect also stimulated the study of GRS. Before 1958, GRS could only be studied in the spectrum of the sun or other stars with high gravitational potential $|\phi| \sim M/R$ such as the white dwarf Sirius B, since the conventional optical wavelength resolution (at best $\Delta\nu/\nu \simeq 10^{-8}$) did not allow discrimination of the effect in systems of lower gravitational potential. For the earth's gravitational field, for instance, a difference in height of 10 m corresponded only to a GRS of $\Delta\nu/\nu = \Delta\phi/c^2 = g\cdot\Delta h/c^2 \simeq 1{\cdot}09\cdot10^{-15}$. On the other hand, it was a different matter for the 14·4 keV γ transition in Fe^{57}: $\Delta E/E \sim \Delta\nu/\nu$ was of the order of $5\cdot10^{-13}$, thus measurement of GRS became feasible if a relative accuracy in the determination of the precise absorption maximum and its shift of better than 10^{-4} could be achieved.

On 1 November 1959 the Harvard professor of physics Robert Vivian Pound (b. 1919) advanced a preliminary note in the *Physical Review Letters* on a projected experiment to measure the GRS in the Earth's gravitational field. He had become aware of the Mössbauer effect from two brief follow-up reports in the *Physical Review Letters* of 1 September 1959, and 'immediately foresaw that the effect might be developed to examples, like ^{57}Fe and ^{67}Zn to look for the GRS'.[14] Together with his doctoral student Glen A. Rebka he searched for appropriate strong γ transitions in radioactive isotopes which would make an earthbound test of the GRS feasible. Apparently at first they even considered height differences of the order of kilometres.[15] Only one month later, the two physicists declared in the same journal: 'We are now confident that we can perform the gravitational experiment inside the laboratory using this γ-ray from Fe^{57}'.[16] On the very same day the editors of the *Letters* branch of *Physical Review* also received a contribution from John Paul Schiffer (b. 1930) and his collaborator Walter Charles Marshall (b. 1932) at the Atomic Energy Research Establishment (AERE) in Harwell (England), who also announced their plans to use the 14·4 keV transition in Fe^{57} to measure the GRS, which they further specified quantitatively as follows:

> At a height of 20 meters this would produce a shift in the energy of the gamma rays which would be 1% of the width. With a one-curie source of Co^{57}, this effect would be measured with 10% accuracy, in 20 hours of counting. An

[14] Personal communication to the author, 8 September 1995. See Craig *et al.* [1959], and Lee *et al.* [1959].

[15] Pound and Rebka [1959a], 439: 'the experiment could be performed between a mountain and a valley, in a mineshaft, or in a borehole'; compare ibid., 440: 'we are undertaking to examine these...isotopes with the aim of selecting an isotope suitable for a gravitational experiment' (submitted 15 October 1959).

[16] Pound and Rebka [1959b], 555f., submitted 23 November 1959, published 15 December 1959.

experiment to measure this is also underway in collaboration with Dr. Cranshaw and awaits the successful preparation of a source of sufficient intensity.[17]

In the last footnote of their contribution they mention not only the fairly general note by Pound and Rebka (1959a), but also other teams whose independent development of the idea of using the Mössbauer effect to test GRS they had only heard about after they had already started their own work.[18] Actually, a third Russian team in Dubna had also begun work with special focus on the zinc isotype Zn^{67} that in principle had the sharpest resonance line,[19] and other teams in Princeton and Tokyo were in the running as well.[20] This shows quite clearly how charged the air had become at that point at the laboratories endowed with adequate instrumentation. In Pound's opinion, his former experience with nuclear magnetic resonance and with perturbed directional correlations of short-lived isomeric nuclei 'which involve much of the same physics' had put him into a special disposition toward the new Mössbauer technology:

> I was probably better able to believe in the resonance properties and to appreciate the limitations than most. For example, Bob [Robert] Dicke was naively pursuing an example of many seconds lifetime which I knew couldn't succeed because other interactions we study in NMR would set limits on the sharpness of the resonance. No resonance narrower than that of the ^{67}Zn isotope we cited in our first letter has been found in the 36 years since then. Neither the Manchester group nor the Dubna, USSR group realized that the strength of the absorption in the iron example was so important. It was so much easier to employ than the zinc example which had only a relatively short lived gallium isotope as a parent source and which required liquid helium temperatures even to be detectable. Boyle and the Russians put all their efforts into zinc.[21]

The news even drifted into the daily press.[22]

Why did not more laboratories enter the race? My explanation is that to reduce systematic errors sufficiently it was indispensable to have strong and chemically pure sources of γ radiation: in the case of Schiffer and his team it involved exposing iron plates in the Birmingham cyclotron and subsequent chemical treatment by the Radiochemical Centre in Amersham. Pound and Rebka were provided with similar services by the Oak Ridge National Laboratory, the Nuclear Science and Engineering Corporation and by Nuclear Metals Inc.[23] This also explains why, in the early years

[17] Schiffer and Marshall [1959], 556f., submitted on 23 November 1959, published on 15 December 1959, directly after the contribution of Pound and Rebka [1959b].

[18] Ibid., 557: the two groups mentioned are D. H. Wilkinson's team on the one hand, and A. Boyle and S. Devons's team on the other. Both later published experimental work on the Mössbauer effect, but not directly related to the testing of GRS (Boyle *et al.* [1960]), perhaps because they had to cede to the priority and time advantage of Pound and Rebka and Cranshaw and Schiffer.

[19] Compare Barit *et al.* [1960].

[20] Compare, for example, Pound [1979], 134 and notes 9–14 where he mentions personal communications by S. Devons (17 November 1959), R. H. Dicke (12 November 1959), and Koichi Shimoda (14 December 1959).

[21] Pound, in personal communication to the author, 8 September 1995.

[22] See the *New York Times* article on Pound and Rebka and GRS, 13 December 1959, or lead article in *The Observer*, early February 1960, on Schiffer and Marshall.

[23] Cranshaw *et al.* [1960], 164; Pound and Rebka [1960b], 337.

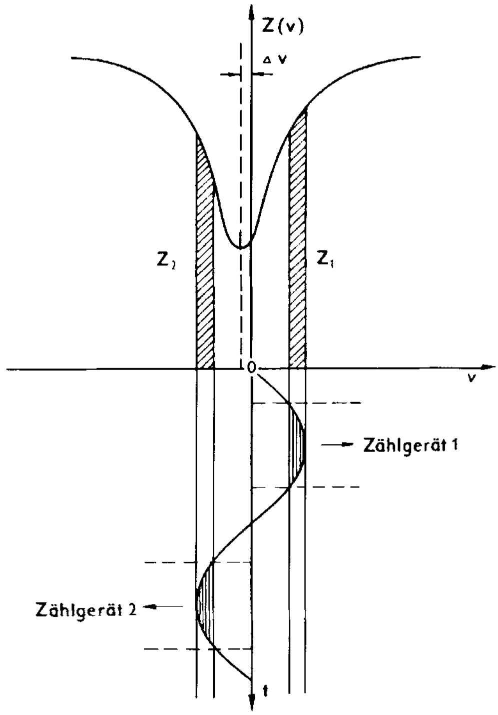

Figure 1. Asymmetry in the counting rates due to a shift $\Delta\nu$ in the frequency of the absorption line. The source is moved sinusoidially with an amplitude correlating with the total line width. *Source*: Wegener (1966), 213.

of the Mössbauer wave, mainly laboratories in the USA, England, France, and the Soviet Union were performing most experimental studies, since it was precisely in these countries that the know-how and the industrial proliferation of products such as purified metal isotopes were fully developed. For the preparation of strong sources the presence of a cyclotron or of a nuclear reactor was most convenient. Heidelberg, where Mössbauer had made his experiments, was one of the few well-situated places in Germany because the physics branch of the Max-Planck Institute for Medical Research had a cyclotron and the concomitant know-how since 1943.[24]

On 15 February 1960, Schiffer and his team in Harwell (England) were the first to present to the readers of the *Physical Review Letters* their results on the GRS induced in the 14·4 keV transition of Fe^{57} by a drop of a γ quant at a height difference of 12·5 m.[25] According to the above rule of thumb: $\Delta\nu/\nu \simeq 1{\cdot}36 \cdot 10^{-15}$. Their radiation source was a Co^{57} source of 30 mCurie activity mounted on an iron disc and heated in a preparation process of several hours' duration at 700 °C hydrogen atmosphere, then in an ultra-high vacuum. This source of intense γ radiation was moved to and fro sinusoidally 50 times per second with the aid of a loudspeaker system, so that a similar periodicity of the absorption rate was observed in the detector that helped to discriminate the signal from the background. The optical path of the latter was evacuated along its entire length to eliminate disturbing influences of the air. The

[24] On the slow introduction of cyclotrons in Europe, which were first developed mainly in Berkeley, California, by Lawrence and his team, see Heilbron [1986].

[25] For the following, see Cranshaw *et al.* [1960], 163f.

detector consisted of a proportional counter embedded in a crypton atmosphere of a 1/4 atm which absorbed the larger part of the low-energy γ radiation well below 14·4 keV, but not the γ quanta from the nuclear resonance.

The counter rates for the oscillation phases of the relative motion of the radiation source *towards and away* from the absorber were counted separately (cf. Figure 1). In the case of absence of the GRS they ought to be identical. For relative rest between emitter and absorber, they should reach their maximum, and symmetrically decrease with to-and-fro motions of the source. If GRS existed in the order of magnitude predicted by GTR, it ought to cause slight differences in the two counter rates.

Because of the sharp definition of the 14·4 keV transition, these differences of the counter rates only amounted to $3{\cdot}9 \cdot 10^{-4}$. After 250 hours' measuring time, the factual asymmetry of the two counter rates $(3{\cdot}75 \pm 1{\cdot}76) \cdot 10^{-4}$ was established, i.e. the relativistic prediction was experimentally confirmed to 96%, but a comparatively high error of $\pm 47\%$ remained. Although no clear-cut confirmation of the full value of the relativistic GRS was reached, at least there was only a 1·7% probability that GRS did not exist at all. A comparison measurement with the same instrumentation at a difference in height of 3 m yielded $0{\cdot}9 \cdot 10^{-4}$, which showed that a difference in height of 10 m was a kind of minimum requirement, given the technological constraints at that time.

Pound and Rebka's rival team got their results into print in the same journal only 6 weeks later on 1 April 1960.[26] They also measured the change in counter rate caused by the periodic up and down movements of the emitter relative to the resting absorber and detector, and their experimental set-up was similar in many other respects, but it differed in three important aspects:

(1) Their path length was longer than Cranshaw and Schiffer's: 22 m (the total usable height of the Jefferson Physical Laboratory tower erected on the Harvard University campus in 1884).[27]
(2) Their radiation source was stronger by a factor of 12 when compared with Cranshaw and Schiffer's Co^{57}: it had an activity of 0·4 Curie. The absorber they used was a specially prepared iron film that included 31·9% Fe^{57}; independent tests had shown that about one-third of the resonance γ quanta was absorbed recoillessly. Owing to its enormous length the optical path was not evacuated but just filled with a cylindrically constricted helium atmosphere.
(3) They had inverted the positioning of emitter and absorber to symmetrize the experimental set-up and thereby excluded two important systematic errors (see below).

After a 10-day run Pound and Rebka also registered a slight asymmetry in the count rates for up and down movement from which they arrived at $\Delta\nu_{\mathrm{exp}}/\Delta\nu_{\mathrm{ART}} = 1{\cdot}05 \pm 0{\cdot}10$. This experiment also confirmed the prediction of GTR within the 10% limit of their error analysis. Gamma quanta thus do experience a slight increase in frequency due to the drop in the Earth's gravitational field. In view of the basic agreement of the results of both experiments, Pound and Rebka's sharp criticism of Cranshaw and Schiffer's rival experiment which was so similar to their own in many respects is rather surprising: 'Our experience shows that no conclusion can be drawn

[26] On the following, see Pound and Rebka [1960b], 337ff., Pound [1961], Krause and Lüders [1961], 35, and Frauenfelder [1962], 61.

[27] See Pound [1961], 877, and [1979], 135, as well as Pound and Snider [1965], B792 for a sketch of the experimental setup.

from the experiment of Cranshaw *et al.*'[28] In their opinion, their British colleagues had neglected the possible influence of temperature differences as well as of chemical differences between emitter and absorber on the counter rates. As they had shown previously in a small note that had appeared 14 days earlier in the *Physical Review Letters*, the temperature dependency of nuclear resonance was so strong that a mere difference of one degree Celsius between emitter and absorber would be sufficient to cause a frequency shift equal to GRS at a height difference of 22 m.[29] Since Cranshaw and Schiffer's set-up had only a 12·5 m height difference, even a mere 0·6 °C temperature difference would have been sufficient to simulate the presence of GRS. While in Harwell no measures had been taken to ensure the temperature equality of emitter and absorber during measurement, the two Harvard physicists had taken these two possible sources of systematic errors into account by (1) installing a thermoelement that automatically recorded the temperature differences between emitter and absorber which were later used to extract this effect from the raw data before the final analysis, and (2) by exchanging the position of emitter and absorber at regular intervals.

In Pound's first review article on this issue published at the end of 1960 in Russian and in 1961 also in English, he could already evaluate a total of six independent test runs conducted at Harvard, each time with slightly more optimized instrumentation and an error margin of about 10% each. Taken together, however, they allowed the conclusion that the observed relative frequency shifts coincided with the GRS predicted by GTR of $4{\cdot}67 \cdot 10^{-15}$ to $(98 \pm 4)\%$. 'The result demonstrates that the predicted "red-shift" exists and is quantitatively in agreement with expectation at least within about 4 percent. In effect the effective mass and weight of radiation have the same proportionality as do those measures of material bodies.'[30]

As far as I can see, Cranshaw and Schiffer only responded to Pound's strong criticism in a paper that appeared in 1964 in which they published their own measurements of GRS using a similar method for the same γ transition, but with a much more involved error analysis. According to this new measurement, the observed frequency shift was $0{\cdot}859 \pm 0{\cdot}085$ times the prediction of relativity theory which thus lay *outside* the error margins of their data. In this same paper they also defended their earlier result with the argument that the temperature effect suspected by Pound and Rebka would in all probability not cause any trouble since it would act on both emitter and absorber, thus effectively cancelling itself out in the statistical average of several days' measuring period. In retrospect they were willing only to increase the error margin of their earlier result:

> ... a measurement of temperature differences over a period of several days in the building where the experiment was conducted indicated that, although some differences existed, the diurnal variations were such as to cause almost complete cancellation of the temperature difference between the two points. Thus the published results would have required no appreciable correction. Since the conditions inside and outside the building in which the experiment had been

[28] Pound and Rebka [1960b], 340.

[29] Pound and Rebka [1960a], 275, submitted on 17 February, and published on 15 March 1960. For a theoretical derivation of this temperature dependency which follows from the relativistic time dilation that increases for higher temperatures, that is higher speeds of its constituents, see, for example, Josephson [1960]; compare also Boyle *et al.* [1960] for the experimental verification of this thermal redshift that soon followed, and Pound [1979], 134–6, as well as Preston and Gonser, in Gonser (ed.) [1981], 167f., for later papers on this effect.

[30] Pound [1961], 880.

> conducted were approximately the same during the temperature measurements as they were during the course of the experiment, a probability can be assigned to the validity of the assumption that a serious temperature difference did not exist. It was estimated that the uncertainty in the temperature difference would increase the errors of the measurement by approximately 20%. Thus, instead of quoting the earlier result as $0{\cdot}96 \pm 0{\cdot}45$, it should be $0{\cdot}96 \pm 0{\cdot}54$.[31]

According to Pound, this hand-waving argument was not sufficient to dispel doubts about their argument that their former data were not systematically disturbed by the existence of isomeric shifts induced by slight differences in the chemical composition of emitter and absorber. The physical reason for this shift is that the effective charge radii of ground level and isomeric levels of the nucleus are slightly different, thus leading to different electrostatic interactions with the electronic charge for the two states. The more compact the charge distribution, the lower the energy level.[32] Already in April 1960, a team at Brookhaven National Laboratory had warned that with chemically different emitters and absorbers 'any recoil-free absorption at zero velocity is accidental. In general, a search for this effect...will not yield significant results unless source and absorber are identical chemically'.[33] According to Cranshaw and Schiffer's own measurements of 1964, this isomeric shift amounted to $4{\cdot}72 \cdot 10^{-3}$ mm/s, that is to a relative shift of $1{\cdot}6 \cdot 10^{-14}$ and thus ten times the redshift measured in their first experiment.[34] Therefore, Pound thinks that the chemical shift led to a 'complete destruction of their result previously reported'.[35]

Anyway, one year later in 1965, all remaining doubt as to the existence of GRS in the earth's gravitational field vanished with the publication of a new value by Pound and his new collaborator Joseph Lyons Snider (b. 1923) who had previously work on short-lived nuclei in atomic beams generated in a cyclotron at Princeton University. Again they had taken the 14·4 keV transition with an even stronger Co^{57} source of 1·25 Curie activity, a larger absorber and of course with even more sophistication in their measurement and data analysis: 'The result found was $(0{\cdot}9990 \pm 0{\cdot}0076)$ times the value $4{\cdot}905 \cdot 10^{-15}$ of $2gh/c^2$ predicted from the principle of equivalence.'[36] With this measurement that confirmed the relativistic prediction with a remaining error of less than 1%, the long-disputed existence of redshift in the gravitational field was thus finally resolved.[37]

It should also be mentioned that in 1960, Cranshaw and Schiffer, together with co-workers, also confirmed Einstein's principle of equivalency according to which a gravitational field should be locally indistinguishable from an accelerated system.[38]

[31] Cranshaw and Schiffer [1964], 246.

[32] This interpretation was given in Walker [1961], 98; compare also DeBenedetti *et al.* [1961], 61, for a table of chemical shifts δ and quadrupole splittings ΔE (as they occur for instance with chemical binding of Fe^{57} in Fe_2O_3) for different types of iron compounds as absorbers.

[33] Kistner and Sunyar [1960], 414.

[34] Cranshaw and Schiffer [1964], 253; compare Pound's letter to the author, dated 10 October 1995: 'It has always puzzled me how they could have ignored the significance of that measured, finally, chemical shift as an influence on their original experimental result, and tried to recover respect for it by accommodating a small increase in the uncertainty arising from thermal fluctuations'.

[35] Personal communication to the author.

[36] Pound and Snider [1965], B788; compare ibid. for their description of the improvements compared with the earlier experiments.

[37] Compare, for example, the following examples for references to the work of Pound, Rebka, and Snider as 'the most accurate verification' of the GRS: Adam [1962], 302; Bertotti, in Witten (ed.) [1962], 30; Tonnelat [1964], 163f., Sciama [1969], 51; Dehnen [1969], 401; Nordtvedt [1972], 1158; Weinberg [1972], 82f.; Misner *et al.* [1973], 1056ff.; Will [1981], 6, 33f.; [1986], 3, 13, 51f.

[38] On the following, see Hay *et al.* [1960], and Wegener [1966], 214f.

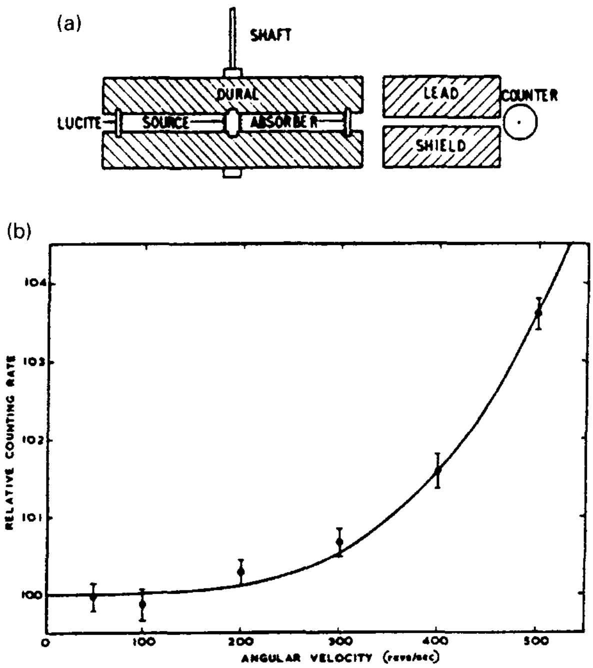

Figure 2. (a) Schematic diagram of the experiment by Hay *et al.* (1960) and (b) comparison of their data with the prediction from the principle of equivalency. *Source*: Hay *et al.* (1960).

To do this with the aid of Mössbauer technology, they mounted a Co^{57} source into the centre of a 0·8 cm diameter iron cylinder. Around it they also mounted a concentric absorber consisting of a cylindrical shell of Lucite with a diameter of 13·28 cm, covered with an iron foil enriched with 50 % of Fe^{57} (cf. Figure 2, left part). This assembly was mounted on a shaft that was rotated at angular velocities up to 500 cycles per second, and the γ-ray absorption rate in xenon-filled proportional counter was measured as a function of angular velocity.

According to classical mechanics, a potential difference $\Delta\phi$ exists between the emitter at the distance $r_E = 0{\cdot}4$ cm and the absorber at radius $r_A = 6{\cdot}64$ cm accelerated at angular velocity $\omega \sim 2\pi(\mathrm{d}\varphi/\mathrm{d}t)$:

$$\Delta\phi = \left(\frac{\mathrm{d}\varphi}{\mathrm{d}t}\right)^2 \cdot (r_A^2 - r_E^2)/2, \tag{1}$$

and according to the equivalency principle, this implies a frequency shift $\delta\omega$ for the absorber A relative to the emitter E according to $2\pi\Delta\nu \sim \Delta\phi/c^2$, that is:

$$2\pi\Delta\nu = \left(\frac{\mathrm{d}\varphi}{\mathrm{d}t}\right)^2 \cdot (r_A^2 - r_E^2)/(2c^2). \tag{2}$$

For the relative shift in the energy of the γ-rays, Hay *et al.* derived a numerical estimate of $2{\cdot}44 \cdot 10^{-20} \cdot \omega^2$. It is noteworthy that this equation can also be derived by looking at the effect as a second-order Doppler effect so that it is not absolutely unavoidable to use the relativistic principle of equivalency in the derivation.[39] At any rate, the experiment showed conclusively that the observed effect is in reasonable agreement with expectations: the relative counter rate increased with the square of the

[39] See on this point in particular Wegener [1966], 215, and Hönl and Bennewitz [1966].

angular velocity as predicted by equation 2: 'The size of the shift of the gamma-ray energy in the effective gravitational field of a rotating system is in agreement with that due to terrestrial gravitational field, within the accuracy of the experiment'.[40]

2.3. *Another perspective on those experiments*

In recapitulating this group of experiments, let us have a look at the rather critical perspective on this cut-throat race by a professor of physics at the University of California at Berkeley, Frederick Reif:

> A few years ago Mössbauer, a young German physicist, discovered that the radiation emitted by certain atomic nuclei in solids is characterized by an exceedingly well defined frequency. This observation suggested to several people, in particular to two scientists, X and Y,[41] that such nuclei might be used as extremely accurate clocks well suited for checking a consequence of Einstein's general theory of relativity. This theory predicts that the rates of two identical clocks should be minutely different if they are located at different heights in a gravitational field. Both Z and Y undertook to check this prediction experimentally. Scientist X, however, first published a 'Letter' outlining his proposal for the experiment, long before he was ready to obtain actual data.[42] A few weeks later, again before either Z or Y had published any preliminary results in the scientific literature, the front page of the *New York Times* carried a picture of scientist X, together with an article describing the experiment he was undertaking.[43] When X discussed his experiment at a scientific meeting 6 weeks later he reported reluctantly that, despite hard work at great speed, he had not yet been able to reach any conclusions.[44] At the same meeting Y announced that he had successfully carried out the experiment and obtained results in agreement with the theory;[45] shortly thereafter Y published his findings.[46] It was not until some 2 months later that X, in a 'Letter', was able to report his own experiment, which also confirmed the theoretical expectation.[47] He pointed out, however, the necessity of controlling the temperature of the experiment quite carefully to avoid introducing large extraneous effects; indeed, since Y had not taken such precautions, his findings lacked significance. In this instance an important experiment was performed in a short time and ultimately in a reliable way. But the example shows vividly the actual circumstances under which the experiment was carried out—the announcement of an experiment before it was undertaken, the newspaper publicity, the hurried activity of two scientists working under pressure to be the first to publish—and the lack of sufficiently careful work which may result from these conditions.[48]

[40] Hay *et al.* [1960], 215; Kündig [1963] found agreement within 1·1 %.

[41] In a footnote Reif identifies these scientists X and Y as Pound and Schiffer.

[42] This alludes to Pound and Rebka [1959a], published on 1 November 1959.

[43] This alludes to Schmeck's *New York Times* article of 13 December 1959.

[44] This alludes to the meeting of the American Physical Society in New York in January 1960 where Pound gave an invited talk on his first measuring results that still had much fluctuation (due to uncontrolled temperature checks, as it turned out later).

[45] According to a personal communication by Pound, Cranshaw, and Schiffer appeared only as 'post deadline' speakers not listed in the programme of the conference as printed in: *Bulletin of the American Physical Society*, (2), 5 (1960), 72; see also Cranshaw and Schiffer [1960].

[46] He alludes to Cranshaw *et al.* [1960], received by the journal on 27 January, published on 15 February 1960.

[47] He means Pound and Rebka [1960a], submitted on 9 March, published on 1 April 1960.

[48] Reif [1961], 1961–2.

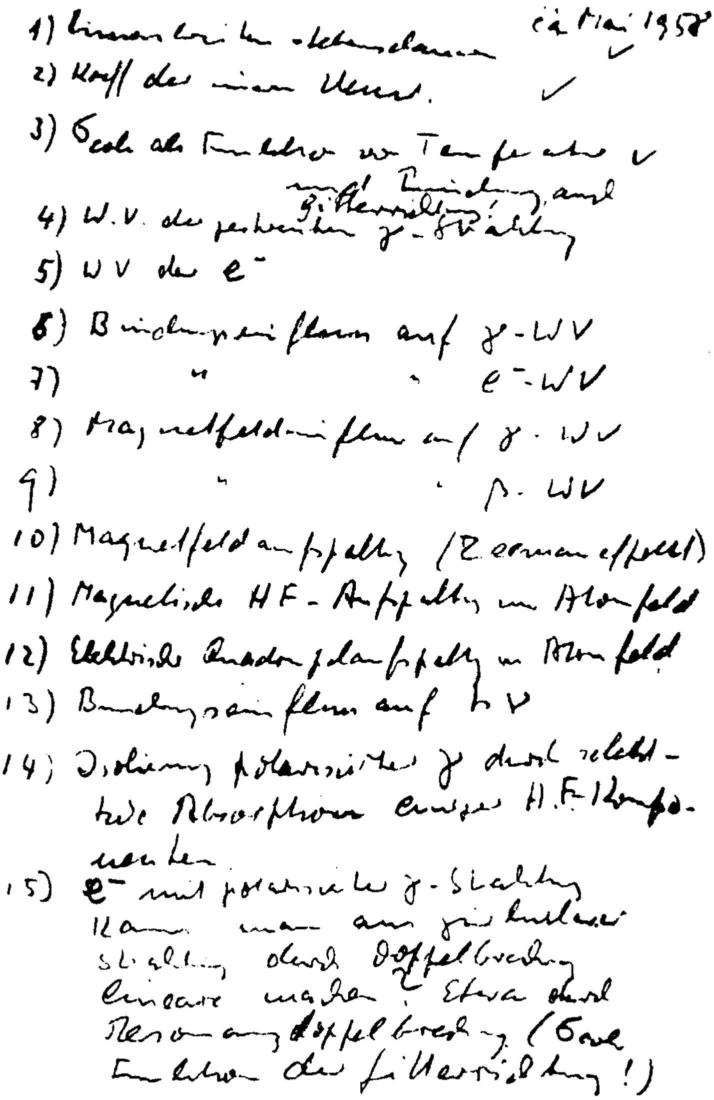

Figure 3. Maier-Leibnitz's handwritten list of 15 feasible applications of the Mössbauer effect, dated [later?] May 1958 by Maier-Leibnitz. *Source*: Kienle (1991), 83.

So why is it that after a 30-year span of silence, suddenly two teams rushed simultaneously to do this experiment? This was because a new avenue of experimentation had been opened by the discovery of the Mössbauer effect in 1958. Heinz Maier-Leibnitz, Mössbauer's doctoral advisor, told me that the possibility of applying the Mössbauer effect to measurements of gravitational redshift was obvious to every experimentalist who at that time considered these issues at all. However, as the handwritten list of possible applications (see Figure 3, compiled by Maier-Leibnitz soon after Mössbauer's discovery) shows, gravitational redshift is not among the applications he himself had listed. When I asked him about this point, he replied: 'Well it was just too trivial to us for serious consideration.'

Be this as it may, it is true that several other teams had also thought about performing this type of experiment apart from Pound and Rebka and Cranshaw, Schiffer and Whitehead, but had been deterred by the early publications of these two teams that signalled their great advantage in the race (cf. note 18). What we observe here is thus one of the most important patterns in the interaction of theory, experiment and instrumentation: namely the triggering of a new type of experiment owing to the development of a new measuring technique.[49]

3. Precision measurements of solar GRS

Since the experiments by Pound and Rebka and Cranshaw and Schiffer in the early 1960s, the existence of a redshift of approximately the order predicted in Einstein's GTR had been proven more or less. However, it was only with Snider's repetition of these experiments in the mid-1960s that the precise determination of its magnitude was achieved. Moreover, all of these experiments were done with γ-rays in the Earth's gravitational field, so methodologically they were far removed from the earlier efforts to find GRS in the gravitational field of the sun. But in the early 1960s there was also a new interest in experiments on solar GRS.

3.1. *Photoelectric spectrometers with wavelength modulation technique*

In May 1962, Robert H. Dicke's student at the department of physics at Princeton University, James William Brault (b. 1932),[50] submitted his PhD thesis on GRS in the solar spectrum, discussing a new approach to this problem with instruments he had designed specially for the purpose. Like many of his predecessors, Brault also started by asking himself what might have caused the failure of earlier efforts to confirm Einstein's prediction of GRS quantitatively.[51] Though Dicke had published theoretical alternatives to the GTR which also implied a non-standard gravitational redshift, Brault started with the working assumption that Einstein's value for the GRS was correct.[52] Since all acceptable models of the solar atmosphere at the time included some kind of assumption on convective zones, it was clear that it had to be assumed that part of the observable redshifts, as well as their variability over the solar surface, were caused by Doppler shifts related to radial currents. Furthermore, in all the models these convective currents were layer-dependent. In the concrete model Brault chose,[53] for optical depths[54] greater than 0·05 increasing gas pressure was another possible cause for larger redshifts at deeper layers.[55] As all of these factors influencing the resulting redshift were layer-dependent, Brault could safely assume that *disregarding the details of the solar models* the majority of the Fraunhofer lines ought to have asymmetrical line profiles. This is the case because the observable

[49] This pattern recurs repeatedly in the historical study on redshift experiments and observations between 1890 and 1960 covered in Hentschel [1995].

[50] During Brault's Princeton years (1955–64) he was also employed at the company Princeton Applied Research where he optimized electronic circuits and developed a highly sensitive JB4 amplifier: compare R. H. Dicke's interview with P. Forman and J. L. Bromberg on 2 May 1983, transcript at AIP, 16, and personal communication to the author.

[51] On the following, see Brault [1962], 1f.

[52] In Brault's own words (personal communication): 'I was scared to hell it might not come out right'. Compare, for example, Dicke [1957a, b].

[53] Brault used the model assumptions in Minnaert [1953]: see Brault [1962], 12a, Table I, for the numerical data on the layer-dependency of temperature, gas pressure and electron pressure, density, and other parameters; compare also ibid., 79, on Schröter's two-current model.

[54] Brault defines optical depth as the absorption coefficient per unit length in the continuum at 5000Å integrated from infinity to the point in question.

[55] For $\tau_{5000} = 0{\cdot}129$ the gas pressure was 4·0 dynes/cm^2, for $\tau = 0{\cdot}672$ already 10·0 and for $\tau = 6{\cdot}09$ even 20 dynes/cm^2.

absorption lines are the superposition of absorption in all these different layers of the solar atmosphere. And it was precisely this asymmetry of Fraunhofer line profiles which lay at the heart of the earlier failures to find the full GRS.

> In the presence of line asymmetry, the results obtained with conventional spectrographic techniques become unreliable; the measured wavelengths will depend on such factors as the measuring habits of the observer, the degree of exposure and quality of the photographic plate, and even the type of instrument used.... The inadequacy of conventional photographic plates for dealing with... asymmetries, as well as the relatively low degree of precision attainable with such techniques when broad lines are being measured, probably accounts for the rather poor agreement between different observers.[56]

In other words, since 'present knowledge of solar structure is inadequate to produce a quantitative description of these effects' as well as their implications for the resulting line asymmetries,[57] the solar observer had to confine himself to the drastically limited subset of data he could safely assume to be free of these disturbing influences. 'On the whole, a strong case can be made for choosing only the best free-standing lines possible.'[58] He thus actually followed a strategy also used much earlier by Grebe and Bachem around 1920 in their effort to confirm GRS in the solar spectrum. They also had selected less than 10 lines from a very much larger sample with the aid of photometric analysis of line profiles.[59] But instead of confining himself to the use of passive registering instruments such as a Koch or Hartmann photometer (as his Bonn colleagues had done 40 years earlier), Brault went further and developed his own instrument specially adapted for high-precision measurement of Fraunhofer line profiles and peaks. The key idea behind his photoelectrical spectrometer was a wavelength modulation technique.[60] The spectrometer's output slit was moved up and down electromechanically with variable modulation amplitude, and the spectrometer's photoelectrically recorded signal was synchronously recorded electronically. The dependency of the photoelectrical signal on the positioning of the slit (cf. Figure 4) was then used in feedback to reposition the slit in such a way that the slit always lay at the real maximum of the chosen spectral lines. This was possible because the oscillations of the output signal were minimal if the slit was precisely at the maximum of the absorption line while it oscillated with (or against) the phase of oscillation of the slit at either of the line's two flanks.

The complicated details of the instrument's design and the polished theory of the photoelectrical spectrometer's response cannot be dwelt on here.[61] It must be emphasized, though, that with this 'null detection method' Brault could increase the accuracy of wavelength measurements by a factor of 10–30 compared with visual measurements, and by a factor of 5 in comparison with all other contemporaneous methods.[62] Furthermore, his method was also well suited to detect deviances of a line

[56] Brault [1962], 1f.
[57] Ibid., 82.
[58] Ibid., 19.
[59] See in this respect Grebe and Bachem [1919, 1920], and Hentschel [1992a, 1995 (§8.8)]. According to a personal communication to the author, Brault did not know about Grebe and Bachem's experiments. This strategy of rigorous data selection is also to be found in papers by Schwarzschild and St John, who in 1916 and 1917 carefully selected the lines they could safely assume not to be subject to pressure shifts.
[60] On the following, see Brault [1962], 2, 25ff.
[61] Brault [1962], chapter 4. Brault confirmed that his earlier work in the plasma physics group as well as on electronics and amplifiers for the Princeton Applied Research Company had considerable impact on the set-up of his solar redshift experiment (personal communication).
[62] Brault [1962], 28.

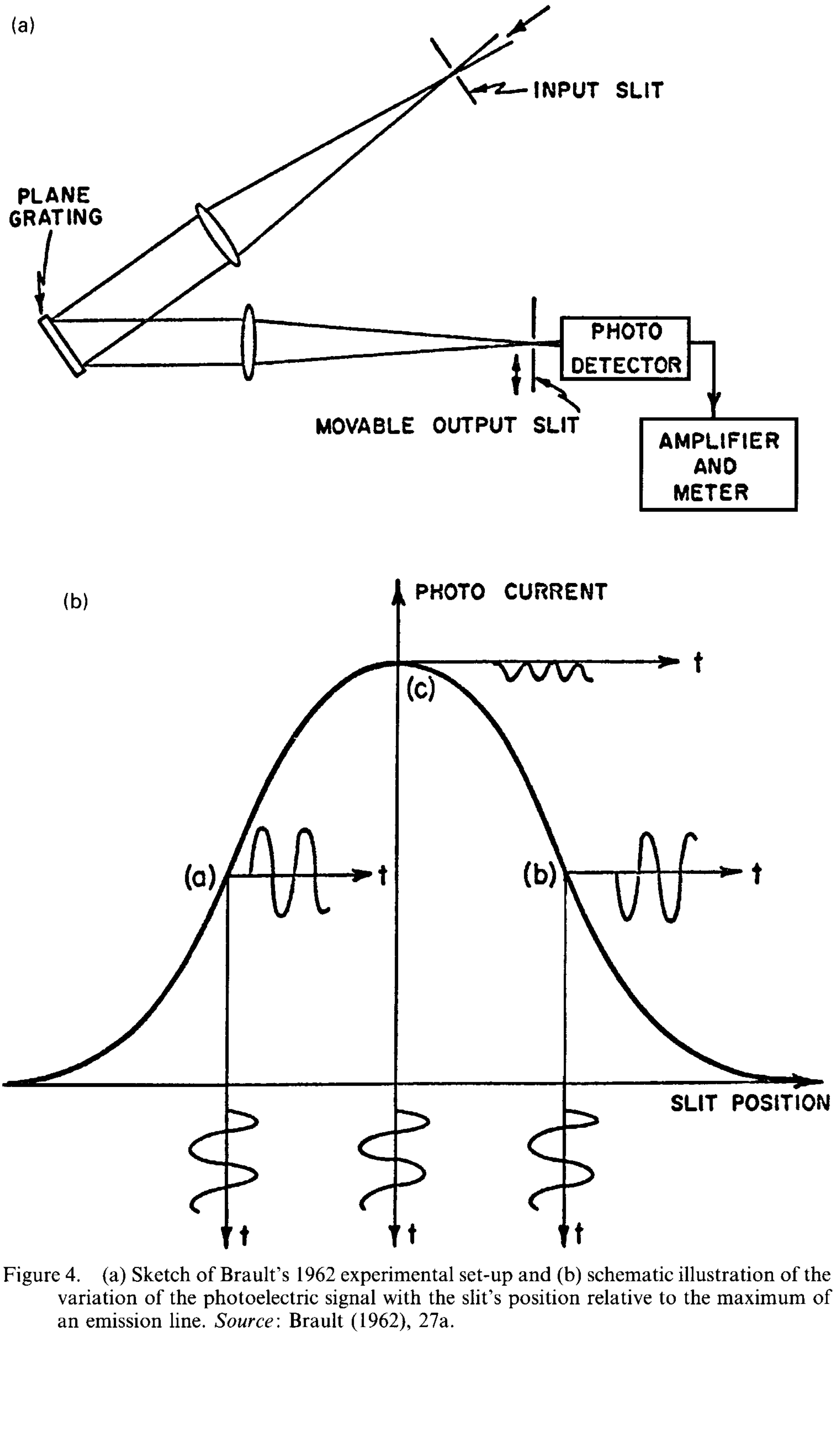

Figure 4. (a) Sketch of Brault's 1962 experimental set-up and (b) schematic illustration of the variation of the photoelectric signal with the slit's position relative to the maximum of an emission line. *Source*: Brault (1962), 27a.

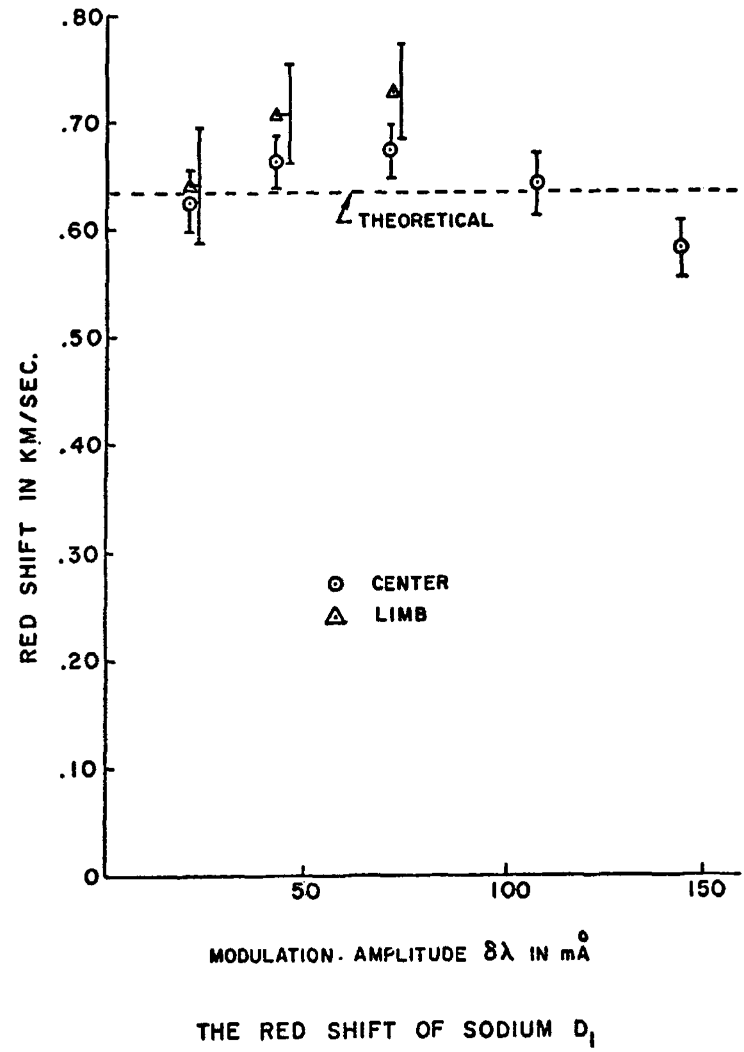

Figure 5. Brault's data for the solar redshift in the centre of the Sun (circled points) and at the solar limb (triangular points) as a function of the modulation amplitude of his photoelectrical spectrometer. *Source*: Brault (1962), 73a.

profile from perfect symmetry because the amplitudes of the output oscillations were no longer symmetrical.[63] The error margin of his data lay in order of 0·1 mÅ resp. 0·005 km/s Doppler equivalent velocity—amounts related to irregular solar convection currents also known from other studies of the solar atmosphere.[64]

Using this instrument Brault searched for Fraunhofer lines 'most free from disturbing effects' of Doppler shift, centre–limb shift and pressure shift.[65] As the latter

[63] Brault [1962], 67f.; compare also ibid., 26f., on the sensitivity of his instrument which Brault thought was able to determine the precise position of the maximum or minimum of a line to 1/1000 of its width.

[64] See Brault [1962], 67ff., on his error analysis; compare also H. H. Plaskett [1952–59], and Hart [1954, 1956] on large-scale convection in the solar atmosphere.

[65] Brault [1962], 3; notice the comparative formulation; compare ibid., 54ff., for his description of a typical run to generate his data.

always led to asymmetric line profiles (see above), to Brault freedom from these disturbing influences was synonymous with a maximum symmetry of the chosen lines. *A priori* only those lines known to be formed in upper layers of the solar atmosphere came into consideration, because the above-mentioned disturbing influences only increased in deeper layers. Even the strongest iron lines, such as 5324 Å with Rowland intensity 7, showed clear indications of convective Doppler shifts. After a critical elimination of most candidate lines, only three remained that were sufficiently intense, undisturbed by blends (neighbouring lines) and unhampered by centre–limb shifts:[66] Two of the three lines from the Mg triplet at 5170 Å and the sodium D_1 line stood out as the most symmetric. Brault's further check of these three lines eventually singled out the latter as the most suitable for precision measurement of GRS in the solar spectrum. His own checks with varying modulation amplitude in the interval between 21·6 and 72 mÅ confirmed that this line was perfectly symmetrical (at least in the immediate neighbourhood of the maximum): 'It was found to show little or no centre-limb shift, and to be essentially symmetric'.[67] Further test series in which the Na D_1 wavelength centre–limb shift was checked for 13 intervals of the solar diameter also did not reveal any considerable centre-limb shift,[68] although Brault's values for the solar limb all lay above those of the centre by a few percent.

As Figure 5 shows, Brault's data for redshift as a function of modulation amplitude first rose slightly and then fell again with a slightly stronger decline for amplitudes higher than about 75 mÅ. Brault argued that only the increasing violet shift for very high modulation amplitudes was a real effect. An increase in the modulation amplitude meant enlarging the interval between which the slit was moved back and forth. This meant taking into account the wider lower part of a spectral line that was formed predominantly in the lower sections of the atmosphere at higher pressures and higher temperatures. On the contrary, the move from the solar centre to the solar limb implied the selection of progressively higher levels of the solar atmosphere, because at the solar limb one cannot see as deeply into the solar atmosphere as at the solar centre (as is already clear from the discussion of solar limb darkening). But in his experiments, both an increase in modulation amplitude and movement from the solar centre to its limb were coupled with increased redshift, so one of these shifts had to be an artefact: 'the trend is to the red in both cases, which is improbable'.[69]

On the basis of this argumentation, Brault omitted on the one hand data gained at modulation amplitudes above 72 mÅ, but on the other hand admitted *all* data at lower modulation amplitudes (up to 72 mÅ, that is, the left part of Figure 5) including those on the centre–limb shift. For the remaining six data points he thus simply gave a numerical average resulting in

$$\left(\frac{\Delta\lambda}{\lambda}\right)_{\mathrm{obs}} = (1{\cdot}05 \pm 0{\cdot}05)\cdot\left(\frac{\Delta\lambda}{\lambda}\right)_{\mathrm{GTR}}, \tag{3}$$

thus an empirical confirmation of GTR's prediction of GRS in the solar spectrum within about 5 %. (As shown in the diagram, even the inclusion of the two data points

[66] Brault [1962], 24, 64, mentions in this connection especially the studies by Evershed [1931], 260f., and [1937] as well as the Utrecht photometric atlas by Minnaert *et al.* [1940], that led to the exclusion of the Na D_2 line because this line was possibly disturbed by strong blends.

[67] Brault [1962], 3; compare ibid., 18f., on weak blends of many other lines. On the sodium D profiles in the centre and at the limb of the sun, see also Shane [1941].

[68] Brault [1962], 65f.

[69] Ibid., 74.

at higher modulation amplitudes would have given a very good coincidence with GRS, since the point at 100 mÅ is by chance virtually coincident with the GRS of the GTR. Because of this astonishing degree of coincidence with the relativistic prediction (at least when compared with all former attempts at verification) Brault's work is quoted in many textbooks and review articles on GTR as 'the' confirmation of GRS in the solar spectrum.[70] However, Brault's great achievement never gained wider popularity, perhaps because apart from a 10-line summary, no detailed account of his doctoral thesis was every published.[71]

3.2. *Resonance scattering in atomic beams*

3.2.1. *Blamont and Roddier, 1961–65.* Independently of Brault's endeavours, a French team had also attacked the problem of GRS in the solar spectrum in the early 1960s. Between 1961 and 1965 four papers appeared in English and French astrophysics journals, authored by Jacques E. Blamont and François Roddier[72] at the Observatoire de Meudon. They used a completely different method to determine the line profiles and thereby also the precise location of the absorption maximum of a Fraunhofer line (in their case of strontium). The key idea of their experimental set-up (Figure 6) was to use the chosen spectral line of a certain chemical element to stimulate vapour at about 600 °C of the same element in the form of a well-defined atomic beam to emit radiation of the same wavelength.[73] The intensity of this resonance emission of light was then determined at a right angle to the incoming light with the aid of a photo-multiplier. The crux of the set-up was that the resonance frequency of the vapour could be shifted by $\Delta\lambda$ in a very controlled way by an additional magnetic field H transversal to the direction of the atomic beam, since the line chosen was subject to the normal Zeeman effect with $\Delta\lambda \sim H$. The order of magnitude of the Zeeman slope was 10–20 mÅ per kG. Unlike Brault's experiment, where only one line passed the critical scrutiny of the experimenter, the preconditions for this experiment were much laxer. Blamont and Roddier only had to make sure that they chose spectral lines that were (1) resonance lines, that is: transitions into the ground state, (2) sharply defined, (3) subject to the normal Zeeman effect, and (4) not subject to any hyperfine structure effects.[74] Thus the variation of the intensity of the re-emitted light of the atomic beam as a function of the magnetic field correlated with the line profile of the stimulating light from the sun. This very elegant method did not fuss with scaling the solar spectrum with any terrestrial comparison spectrum but used the oscillatory behaviour of light-scattering atoms directly as its clock, omitting

[70] For example, Born [1920, 4th edn 1964], 305; Dicke [1964], 25f.; Dehnen [1969], 401; Weinberg [1972], 81; Misner *et al.* [1973], 1058f.

[71] See, for example, Dicke's unpublished scientific autobiography (AIP typescript, p. 7), in which he refers to Brault's experiment as the first 'meaningful' confirmation of GRS in the solar spectrum and continues: 'This excellent experiment did not receive the full publication that it deserved. The good results were due to the design of a special instrument and the use of a sodium D line (developed high in the photosphere where convective motion disappears)'. Brault reported that he continued to work on GRS for two years (still on the Na D_1 line), but never found the time to write down his results after moving to the Kitt Peak Observatory in 1964 (personal communication).

[72] Between 1959 and 1964, Roddier and Blamont were employed at the 'Service d'Aéronomie' of the CNRS at the Observatoire de Meudon. In December 1964 Roddier submitted his two-part summary of his work (on which I report in the following) as 'thèse principale' at the Faculté des Sciences de l'Université de Paris: Roddier [1965], 463 (footnote).

[73] On the details of the preparation of this atomic beam, see especially Roddier [1965], 465–76.

[74] Practically, this latter condition meant that the line had to be emitted by an isotope with nuclear spin 0.

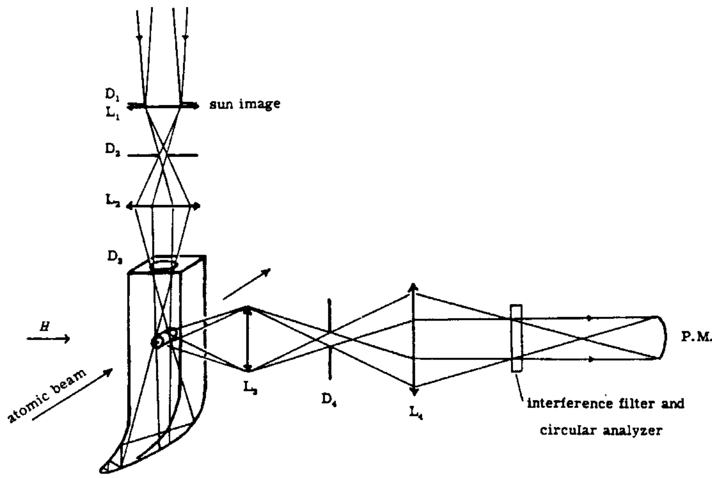

Figure 6. Set-up for the measurement of light resonance scattering with atomic beams. D_1 is a blend for selecting a suitable part of the solar disc, D_2 to D_4 are blends against stray light and L_1 and L_4 are convex lenses; the magnetic field H is applied orthogonally to the axis of the atomic beam. *Source*: Blamont and Roddier (1964a), 438.

thereby all potential disturbing influences such as pressure or pole effect. Blamont had already used this method in 1953 for the determination of terrestrial emission spectrum line profiles. As the width of the chosen, very sharp resonance line of strontium at 4607·3 Å was a mere 1–2 mÅ, the obtainable wavelength resolution $\lambda/\Delta\lambda$ lay between 10^6 and 10^7, thus clearly above the resolution obtainable by diffraction gratings.

This absolute measuring method allowed a direct comparison of the wavelength of Fraunhofer lines with the spectral lines of similar atoms in the atomic beams. In this way Blamont and Roddier determined the minimum of the strontium line 4607·3 Å in the solar centre and at 1′ distance from the solar limb (Figure 7), later (by 1965) in even more detail as a function of heliographical width.[75] While the theoretical prediction of GRS for this line was 9·76 mÅ, the empirical values were 8 mÅ for the solar centre and about 12 mÅ for the solar limb.

To explain the increase of redshift at the solar limb, Blamont und Roddier took recourse to the Lindholm effect,[76] since its pressure-induced redshift would add to the GRS at the solar limb, thus leading to a higher value of 12·16 mÅ at the solar limb which fits nicely with the observed value. However, this reasoning led to a problem with the value at the solar centre, since there too one should find the superposition of GRS *and* the Lindholm effect, but the observed value was lower. While in their first paper of 1961 Blamont and Roddier could do no better than to assume rising convective currents with associated violet shift in the solar atmosphere as the reason for this discrepancy,[77] they later corroborated this suspicion in three follow-up papers. Detailed comparisons were made of observed line profiles with calculated profiles based on certain models of the solar atmosphere such as de Jager's and also

[75] On the latter, see Roddier [1965], 18ff.

[76] Compare Hentschel [1995], § 10.3; Blamont and Roddier [1964a], 438, assumed T = 5700 °C and 10^{17} hydrogen atoms per cm^3 and argued that for the ${}^1P_1 - {}^1S_0$ resonance transition of the chosen strontium isotope, Lindholm's approximation of the interatomic forces by van der Waals potentials was sufficient. On the Lindholm effect, see Lindholm [1942], and Hentschel [1995], § 10.3, and sources mentioned there.

[77] Blamont and Roddier [1964a], 439; compare also Sciama, in Adam [1962], 304.

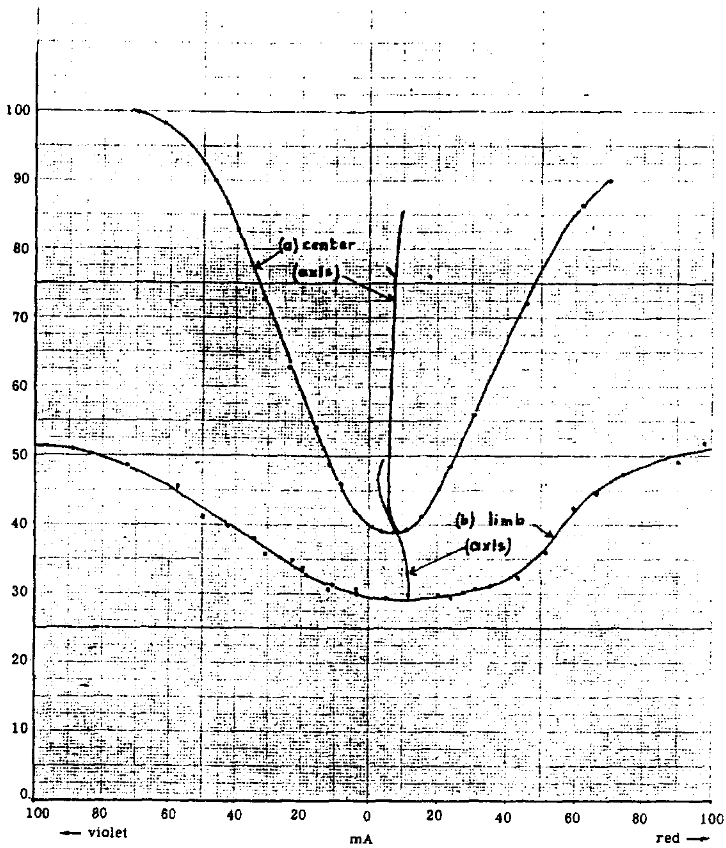

Figure 7. Line profiles of the strontium line 4607.3Å in the solar centre and at the limb, as well as their symmetry axes. *Source*: Blamont and Roddier (1964a), 439.

Schröter's two-current model which assumed the existence of such vertical tubes of rising gas in the solar atmosphere related to solar granulation.[78] Although it was not possible to fit the observed line profiles to the contemporary models of the solar atmosphere if only one constant microturbulence of 1·8 km/s was assumed, the agreement between theory and experiment improved with the assumption of a smaller velocity of just about 1 km/s in the convection zone ($\tau > 0{\cdot}07$), but a much greater velocity of circa 3·5 km/s in the layers above ($\tau < 0{\cdot}07$).[79] 'L'analyse de ces résultats nous a conduit à formuler l'hypothèse d'un accroissement rapide de la microturbulence avec la hauteur dans l'atmosphère solaire. Cet accroissement aurait lieu exactement à la limite de la zone convective.'[80]

3.2.2. *Snider 1969*. As mentioned earlier, the technique of magnetic *scanning* on the basis of resonance scattering used by Blamont and Roddier for their confirmation of GRS worked only for a very limited number of Fraunhofer lines. A systematic check in the solar spectrum made by Joseph Lyons Snider (see above) at Harvard University's Jefferson Physical Laboratory showed that, apart from the strontium

[78] Compare, for example, Bray and Loughhead [1967] or Hentschel [1995], §10.4.5 for further literature.

[79] On this, see Roddier [1965], 25–9.

[80] Ibid., 34.

line chosen by Blamont and Roddier, only two others could be taken: the D_1 line of 5896 Å of the element sodium already chosen by Brault in 1962 as well as a line of potassium at 7699 Å, which (according to the contemporary photometric atlases of the solar spectrum) was nearly perfectly symmetrical (quite unlike the strontium line) and also fulfilled the other four criteria listed above.[81] Therefore Snider decided to use the resonance method on this line to check Blamont's and Roddier's result for the GRS. Because of the similarity of the line profiles of the Na D_1 line and the K line, a confirmation of their result would also imply an increase in the reliability of the magnetic *scanning* technique as compared with Brault's wavelength modulation technique.

Using instrumentation and data generation roughly similar to Blamont and Roddier's, Snider collected data on 22 different days between January and May 1969 but, surprisingly, it did *not* lead to the expected quantitative confirmation of GRS but only to an average of

$$\Delta\lambda = (10 \pm 1)\,\text{mÅ} = (0{\cdot}61 \pm 0{\cdot}06)\,\Delta\lambda_{\text{ART}}, \tag{4}$$

i.e. only 60% of Einstein's prediction which lay well beyond Snider's error margin. As the GRS prediction had already been confirmed by too many other observers by that time, Snider tried to locate the error in his experiment rather than in those of the others. At the time of publication of his results in 1970 he suspected unknown time-dependent disturbing influences in the solar atmosphere as the reason for this failure:

> All we can at present say is that, contrary to our hopes, the solar potassium line is affected differently than the sodium line by the dynamics of the photosphere. The 'additional unambiguous evidence for the solar gravitational redshift' mentioned in the Introduction has not been found.[82]

Despite his intensive search for possible errors, the actual cause of these deviances only surfaced several years later after Snider, who had moved in the meantime to Oberlin College in Ohio, conducted new measurements of GRS in 1971. It was the deviations from the linear dependency of magnetic field strength and line shifts which gave Snider the idea that unwanted circularly polarized components of the incoming light ray may have altered the line profile.[83] These superimposed components happened to be strongest in those cases where GRS and the Doppler shifts induced by the earth's motion happened to work in the same direction as had been the case in his first measuring period of 1969. Contrary to this, the second measuring period (from December 1970 to November 1971) took a whole year so that the superimposed effect of the earth's motion equalled out in the average, which meant that his data were essentially free from superimposed effects of circularly polarized components. The data collected during this long measuring period led to a result of $(16{\cdot}4 \pm 1)$ mÅ compared with the GTR prediction of 16·3 mÅ for this line. This result $(1{\cdot}006 \pm 0{\cdot}061)$ was another clear confirmation within an error margin of merely 6%. Since the accuracy of this result was quite comparable with the one that Brault had achieved (5% error margin), but no longer needed the intricate auxiliary assumptions

[81] Snider [1970], 353; compare Minnaert *et al.* [1940] or Moore *et al.* [1966]. In Snider [1972], 856, it is also shown that this line as well as Na D_1 does not show any significant centre–limb shift. Snider had worked on atomic beam technology already in Princeton and thus transferred some of this know-how to the Harvard research group.

[82] Snider [1970], p. 368.

[83] See Snider [1972], 855, where he specifies the amount of unwanted light components to 12%; compare also Snider [1974], 233f.

introduced by Blamont and Roddier concerning convection currents and the like, Snider himself spoke emphatically of 'the second clear-cut experiment on the solar red shift'.[84] Perhaps of more importance than the agreement with relativity theory was the re-establishment of mutual agreement with the result of Blamont and Roddier obtained by similar means, since this kind of stability was the precondition for the reliability of the magnetic scanning method of resonance scattering in atomic beams.[85]

4. Conclusion

The dampened oscillations with which in the 1960s and early 1970s the three different measurement techniques (Mössbauer effect, photoelectric wavelength modulation, and resonance scattering) each came closer and closer to the value predicted by Einstein's theory of relativity are summarized in Table 2.

The following systematic conclusions can be drawn from the previous case studies on experimental work on GRS between 1959 and 1972:

(1) The successful confirmation of Einstein's prediction of GRS in the solar spectrum as well as in the Earth's gravitational field in the 1950s and early 1960s only became possible with the aid of new technologies (Mössbauer effect, wavelength modulation, fast photoelectric spectrometer) that were not available in the decades before.

(2) The experiments soon kept pace with these technologies; in fact, in the case of the application of the Mössbauer effect there was even a cut-throat race to be the first to apply it to the GRS problem. Therefore, the Mössbauer effect was itself a new experimental discovery at first, but soon evolved into a measuring technology for other experiments.[86]

(3) This clustering of similar experiments with (roughly) similar outcomes actually furthered the acceptance of each of them, but also (even more so) the acceptance of the new technologies on which they were based.[87]

(4) Methodologically there are striking parallels between the approach of experimenters around 1960 with those three or four decades earlier (in particular, if we compare Brault with Bachem and Grebe):
    strict selection criteria as to suitable data; and
    effective combination of insights from different branches of science.

Granted that the last conclusion is true, it also supports my approach of searching out patterns running through cases related to the same *problematique*, but in different periods and situated in different scientific communities (physicists, spectroscopists, solar astrophysicists). Similar to the case of St John,[88] the successful strategy was to combine systematically isolated insights into a densely packed network of mutually consistent data, model assumptions and further hypotheses (such as, for instance, Brault's assumption of an abrupt break in the order of magnitude of the micro-

[84] Snider [1972], 853.

[85] On this calibration of scientific instruments by mutual coherence of measurements, see, for example, Galison [1987], 267ff., Andy Pickering and Allan Franklin, in Gooding *et al.* (eds) [1989], 275–97, 437–59, especially 279ff., 438f. respectively, and, in particular, Franklin [1994], 472–87.

[86] It became 'transparent' in the sense of Schaffer and Gooding, in Gooding *et al.* (eds) [1989], 14f., 70, 91ff., 216ff.

[87] On Brault's reception, see note 70; on Pound, Rebka, and Snider, see note 37; in many of these references the coincidence in the empirical values for GRS between these two sets of measurements was pointed out explicitly.

[88] Compare Earman and Glymour [1980], and Hentschel [1993a], and references given there.

Table 2. Results of GRS measurements, 1959–71; the last column lists the resulting values for $\Delta\nu_{emp}$ as compared with $\Delta\nu_{GTR}$.

Name	Date of publication	Technique	$\Delta\nu_{emp}/\Delta\nu_{GTR}$
Cranshaw and Schiffer	15 February 1960	Mössbauer effect of Co^{57} radiation at 10 m tower	$0{\cdot}96 \pm 0{\cdot}45$
Pound and Rebka	April 1960	Mössbauer effect of 14·4 keV transition in strong Co^{57} radiation, 22 m tower	$1{\cdot}05 \pm 0{\cdot}10$
Pound	End of 1960	22m Tower	$0{\cdot}98 \pm 0{\cdot}04$
Brault	1961/62	Wavelength modulation in photoelectric spectrometer	$1{\cdot}05 \pm 0{\cdot}05$
Blamont and Roddier	1961–65	Resonance scattering in atomic beams for $\odot$ centre and $\odot$ limb respectively	0·82 and 1·5
Cranshaw and Schiffer	1964	Reassessment of 1960 measurements with temperature inhomogeneities included in the error analysis	$0{\cdot}96 \pm 0{\cdot}54$
Pound and Snider	1965	Mössbauer effect (as above)	$0{\cdot}9990 \pm 0{\cdot}0076$
Snider	1969	Resonance scattering of 7699Å line of K (without unwanted contributions from circularly polarized γ radiation)	$0{\cdot}61 \pm 0{\cdot}06$
Snider	1972	Resonance scattering of 7699Å line of K (without unwanted contributions from circularly polarized γ radiation)	$1{\cdot}006 \pm 0{\cdot}061$

turbulences at the border zone between convection zone and photosphere). Owing to the progress of science, the acceptable assumptions were even more constrained for Brault in 1960 than they had been for Bachem and Grebe 40 years earlier; but on the other hand many model parameters open to Brault were simply beyond the reach of his predecessors, so that, all in all, in both periods considerable interpretive leeway remained.

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