The Erwin Schrödinger International Institute for Mathematical Physics

Boltzmanngasse 9/2
A-1090 Vienna, Austria

ESI NEWS

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Contents

Walter Thirring and the Erwin Schrödinger Institute

In August of 1990 Alexander Vinogradov sent a letter to Peter Michor in Vienna with the suggestion to set up a research institute devoted to mathematics and physics in Vienna. Setting up such an institution was seen as a potentially valuable contribution at this time of crisis for the Eastern European scientific community: based on the cultural and scientific tradition in Vienna, especially in the field of mathematical physics, a new institute based in Vienna could provide a focal point for both Eastern and Western science and an international platform for research in the field of mathematical physics.

This initiative was warmly welcomed by Walter Thirring. In a letter to the Minister of Science and Research, Erhard Busek, dated October 18, 1990, Thirring proposed to establish the 'Erwin Schrödinger Institute for Mathematical Physics' in Vienna. Thirring's proposal immediately won the support of eminent scientists all over the world, and Busek responded favourably in December 1990.

The society (*'Verein'*) 'Internationales Erwin Schrödinger Institut für Mathematische Physik' was officially founded in April 1992, and on May 27 the constitutional general assembly of this society elected Thirring as its president and took the formal decision to set up a research institute under the legal framework of the society.

The *Erwin Schrödinger International Institute for Mathematical Physics* (ESI) started its operation in January 1993 under the presidency and scientific directorship of Walter Thirring with Peter Michor as executive director, and was opened officially on April 20, 1993, under the auspices of Vice Chancellor and Minister for Science and Research, Erhard Busek.

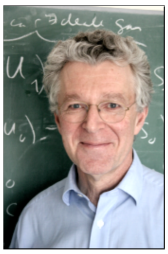
In 1998 Walter Thirring retired as President of the ESI and became its Honorary President. This did not in any way diminish his immensely valuable contributions: even now, at the age of 80, Walter Thirring continues to suggest new initiatives and directions for the Institute and its scientific programme.

This special issue of ESI NEWS devoted to Walter Thirring on the occasion of his 80th birthday is a small token of our gratitude for almost 17 years of work, support and advice for the Institute, and for many years of personal friendship and scientific stimulation.

Klaus Schmidt, Joachim Schwermer and Jakob Yngvason

Following Walter

Jakob Yngvason



My first encounter with Walter was in 1976. This was not a personal meeting though, I was simply in the auditorium of the Winter School in Schladming where he lectured on Stability of Matter. Little did I know then that 20 years later I would be in Vienna as his successor.

Of course, it is not possible to succeed Walter except in the trivial sense of a temporal order in a list of professors of theoretical physics at the University of Vienna. Walter's style, both in his profession and his life in general is so unique that it would be preposterous even to try to imitate it. But his papers, books and lectures continue to be a standard of reference for the assessment of any work in mathematical physics and have been a constant stimulus for my own work.

Coming back to the Schladming lectures of 1976 I recall how I liked the way Walter presented the case for Stability of Matter. He did not jump right into the mathematics but explained first the importance of the question and then started off with the 'private room' argument for the electrons and did the 'back of an envelope' estimate of the kinetic and Coulomb energies, comparing the result also with the analogous calculations for Bosons and also for gravitating particles to bring out the differences. He then commented on the justly famous work of Dyson and Lenard from the year 1967 where stability of matter was proved for the first time but with an absurdly large constant of the order 10^{14} in the lower bound to the energy. As Walter remarked, the large constant was solely due to the tour de force method of proof that consisted of a long sequence of estimates, about 45 in all, loosing a factor of 2 on average each time. If you do this 45 times, Walter explained, then you are close to 10^{14}. After these explanations he proceeded with his and Elliott Lieb's new and beautiful proof based on Thomas-Fermi theory that goes right to the heart of the matter and produces a reasonable constant. This work is a true masterpiece of 20th century mathematical physics and its extensions and refinements continue to be active research topics.

Walter's textbooks on Mathematical Physics are marvellous works and the denseness of information and insights they provide is truly remarkable. One reason for this is Walter's awesome skill in distilling a complicated mathematical argument into a few lines. Sometimes the result has the character of 'spiritus concentratus' that must be diluted again to bring it back to drinking strength; this is in fact what I have done on several occasion when lecturing to students. Such elaborations have pedagogical merits but I sometimes wonder if I am not depriving the students of the experience of being exposed directly to the hard stuff. Also, Walter's method takes less time and I have never in my courses managed to cover a comparable amount of material as Walter has apparently done in his courses. But this is one of the differences in style I mentioned before.

There is one aspect of Walter's working style that I sometimes wish I could imitate: his extreme discipline and organization. I understand that when he had administrative duties at the University he managed them very effectively and kept them in time slots well separated from other things. In this way he had plenty of time for the things he likes most: doing physics, playing the organ and composing music.

Characteristic for Walter's scientific work is the breath of topics he has been interested in and worked on. In this respect he is for me the model case of a mathematical physicist. One often distinguishes between 'method oriented' and 'problem oriented' approaches in science and sometimes mathematical physics is regarded as being as a discipline solely defined by its methods. I regard this is a misunderstanding. In any field there are practitioners that have acquired some skills in a specific method and spend most of their professional life in searching for and working on problems to which these methods can be applied. In disciplines that rely heavily on expensive experimental equipment this strategy may even be perfectly justified for a limited amount of time. But in physics the true aim of the game is to understand how nature ticks at its fundamental level and for this task the methods have to be chosen to fit the questions attacked.

Walter has worked in Quantum Field Theory, Elementary Particle Physics, Statistical Physics and General Relativity and made seminal contributions to all these fields. He is one of the pioneers of modern mathematical physics, to which I would also count people like Rudolf Haag, Arthur Wightman, Hans Borchers, Ludwig Faddeev, Elliott Lieb, Joel Lebowitz and David Ruelle, among others, who in the 1960's realized that there are deep questions in physics that require deep mathematics for their proper understanding and were prepared to learn what it takes whenever necessary. Carrying on with this tradition is the legacy for Walter's followers.

Walter Thirring – A Short Biographical Sketch

Wolfgang L. Reiter

'I never wanted to write an autobiography. How despicable those nostalgic war stories of old men appeared to me proving what hell-raisers they had been in their youth'.[1] With these words Walter Thirring opens a biographical sketch of his early days overshadowed by the invasion of Nazi barbarism. He was born on April 29th, 1927, in Vienna into a Protestant family originating from Thuringia (the family name Thüringer finally became Thirring). His ancestors came to Austria in 1623 as religious refugees during the times of the Thirty Years' War and later settled in the small Hungarian town of Sopron. Hungary was then and later a safe haven for Protestants, since the Transleithanian part of the Habsburg empire did not submit to the *cuius regio eius religio* rule and the Counter Reformation. His grandfather Ludwig Julius Thirring (1858-1941) moved to Vienna in 1878 to study mathematics and physics.

While being of Protestant denomination Walter attended the progressive, non-elitist and socially oriented Catholic 'Neulandschule'. The adolescent boy was soon confronted with a radical change of society *'which can be arbitrarily deformed in the hands of a demagogue'*.[2] A few days after the occupation of Austria by Nazi Germany in March 1938 his father Hans Thirring (1888-1976) was dismissed from his academic post. The Gestapo was since then a frequent visitor of the Thirring's flat.

'My brother three years older in age had always been considered a great genius while I was just the small appendage'.[3] In a letter to Walter written from the Russian front his brother Harald (1924-1945?) expressed no hope of surviving the war. He charged his younger brother *to keep the scientific tradition of the family*.[4] Without being able to attend school classes, because in the meantime he had already been conscripted as a 'Flakhelfer' (there he exercised his mathematical abilities by misdirecting the guns until noticing that this was to no avail) he was provided by his father with the then standard textbook for theoretical physics, the 'Joos', *a superb choice* as Thirring remarks.[5] The 17 year old boy started off to follow the family's orders issued by his brother. Following his grandfather Ludwig Julius who studied with Charles Hermite (1822-1901) in Paris and his father Hans, student of Boltzmann's pupil Fritz Hasenöhrl (1874-1916) and since 1927 full professor of theoretical physics at the University of Vienna, it was now Walter's turn to close the gap in the family's chain of physicists. Harald did not return from the Russian battle field.

Walter had had in mind a quite different professional career: he wanted to dedicate his life to music, playing the piano and composing. But the brother's letter from the eastern front overruled Walter's love for music. Responsibility for the family and its tradition, 'Verantwortungsethik' according to Max Weber's analysis, was the driving force — or was it the father's order unconsciously executed by the brother (here we are in genuine Freudian waters) setting the stage for Walter becoming a physicist?

During the last days of World War II the Thirrings left Vienna for their second home at Kitzbühel not far from Innsbruck, where Walter began his studies with the theoretical physicist Arthur March (1891-1957) lacking a proper final exam (Matura). In 1949 he got his PhD from the University of Vienna with distinction. The topic of his thesis was the Dirac equation ('Zur kräftefreien Bewegung nach der Dirac-Gleichung'). Paul Urban (1905-1995), assistant at the Institute for Theoretical Physics at the University of Vienna, raised Thirrings interests in meson theory, at that time the talk of the town among physicists, by engaging him in boring perturbation theory calculations of various cross sections. When he asked if there were any experimental data available to check and verify his calculations he had to be disappointed by his tutor.

After having received his PhD Thirring was granted a scholarship at the Dublin Institute of Advanced Studies, where he met Erwin Schrödinger (1887-1961), a close friend of his father from the times of their studies with Fritz Hasenöhrl. The following year he spent as a Fellow at Glasgow University and during the academic year 1950/51 he held an assistantship at the Max-Planck-Institut in Göttingen, where he worked with Werner Heisenberg (1901-1976). Until his first position as a professor of theoretical physics at the University of Bern he was 'on the road': 1951/52 as UNESCO Fellow at the ETH Zürich, where he met the caustic Austrian Wolfgang Pauli (1900-1958), 1952/53 assistant at the University of Bern. In 1953/54 he worked at the Institute for Advanced Study in Princeton and met Albert Einstein (1879-1955). In 1952 Thirring married Helga Georgiades. The couple has two sons, Klaus and Peter, and four grandchildren have since enlarged the family.

In 1954 he returned to Europe as a lecturer at the University of Bern. The following years 1956/57 he was Visiting Professor at MIT and 1957/58 at the University of Washington in Seattle. During this time of academic tramping Thirring was intensively engaged with problems in quantum field theory, becoming one of its pioneers. Among his co-authors at that time were Stanley Deser, Marwin L. Goldberger and Murray Gell-Mann. Several papers on renormalization and dispersion relation in particle physics were the result of these fruitful co-operations.[6] During this ten year period of work at different research centres Thirring made a pivotal experience, transnationality of science, an insight he brought back to his, at that time provincial, native Austria. After one year as full professor at the University of Bern in 1959 he accepted a call of the University of Vienna. Until his retirement in 1995 he was professor at the Institute of Theoretical Physics in Vienna.

From an early stage onward, Thirring was engaged with the first steps of Austria's membership of CERN which was finalized in 1959, opening doors for experimental and theoretical research on a truly international basis. Since the 1950's Thirring concentrated his research mainly on theoretical elementary particle physics with some excursions into other fields of physics. When Marietta Blau (1894-1970), who pioneered the photographic method in particle physics, came back to Austria in 1960 after having left the country as a Hitler refugee in 1938 and years spent in Mexico and later in the US (Columbia University, Brookhaven, Florida), she and Thirring formed a group of young student researchers to analyse bubble chamber photographs from CERN. This initiative was a very first bridge between experimentalists at CERN and physicists in Vienna and thus the starting point of high energy physics in Austria. Subsequently, the Institute of High Energy Physics of the Austrian Academy of Sciences was founded in 1966 and became the home base for Austria's activities at CERN, actively supported by Thirring as long term chairman of the board of the Institute. In 1966 Thirring was made corresponding and in 1967 full member of the Austrian Academy of Sciences.

Thirring's close ties to high energy physics found a new stage when he became member of the Directorate of CERN as head of the Theoretical Department for the period 1968 – 1971, a time of important decisions to be made. The main tasks at that time were to get the intersecting storage ring ISR, based on a concept of the Austrian-born physicist Bruno Touschek (1921-1978), operational and to negotiate the siting of the next generation accelerator, the 300 GeV SPS, which had already been decided on in 1964 under the directorship of another Austrian-born physicist, V. F. Weisskopf (1908-2002). Thanks to the interventions of the then Director General of CERN, Bernard Grégory, the member states finally took the wise decision to build the SPS at Geneva, against strong lobbying of several countries to build the machine on a different site somewhere in Europe. Austria took part in this competition and made a site proposal for Göpfritz in the Waldviertel, a place surrounded by lovely woods and of perfect geological conditions some hundred kilometres north of Vienna!

Interestingly enough Thirring has written only occasionally on science policy or exposed his opinion on matters beyond pure science in public. On the occasion of the opening of the 10th Schladming Winter School for Nuclear Physics in 1971 Thirring addressed the audience with an analysis of the situation of high energy physics in Europe stressing the importance of fundamental science for the scientific development in Europe to counteract the influence of the leading powers, USA and the Soviet Union. Big science had become the characteristic of research in fields like particle physics, and Thirring stressed the fact that it is most important for small countries to participate in this game. Thirring went one step further and called for new instruments for research in Europe: *'The real problem of science in Europe seems to me to be how we shall be able to cope with these challenging fields which require large-scale collaboration. The individual countries are too small to deal effectively with these branches of science and it will be difficult to co-ordinate the intentions and desires of various countries and various scientific disciplines. Without something like a* European Research Council [*my emphasis*] *which plans and supervises science as well as directs the spending of the research money on a European level, I can hardly see that we will get anywhere'.*[7] It took quite some time until, in 2006, Thirring's proposal became reality (albeit in a different form) with the foundation of the European Research Council within the framework of the European Community. The incisiveness of his intellect had seen a pressing need long before, but the realisation of his proposal needed a further step of unification and collaboration in Europe transgressing the political model of CERN.

Thirring started his academic career by and large as a theoretical elementary particle physicist. Interestingly enough by the mid 1960's he shifted his style from traditional theoretical physics to a stronger mathematically oriented mode and this shift goes together with a shift of his research interests. As soon as 1967 he published in *Communications in Mathematical Physics*.[8] His first encounter with Elliott Lieb happened during their stay at the ICTP in Trieste in 1968, and subsequently when Thirring invited Lieb to give a talk on joint work with J.L. Lebowitz on the existence and convexity properties of the thermodynamical functions of Coulomb systems.[9] Thirring *found that this result was so much part of the general physics culture that even particle physicists ought to know about it.*[10]

A period of intense collaboration on the improvement of the Dyson-Lenard proof of the stability of matter between Thirring and Lieb started in 1974 when Lieb was Schrödinger Guest Professor at Thirrings institute in Vienna. In the following summer Lieb again came to Vienna and this time they succeeded in directly proving the stability of matter.[11] During the following years this topic and other problems of the Thomas-Fermi-Theory was further investigated by both, together with other collaborators.

During the years 1976 – 1978 Thirring served as first president of the International Association of Mathematical Physics

(IAMP) and succeeded in developing this organisation into an international platform.

Naturally, opinions are split which of Thirring's achievements should be counted as the most important. He himself leans towards the work on the stability of matter. In trying to do justice to the enormous breath of his oeuvre an assessment is confronted with a wide range of subjects. Among his early work we find a paper in the field of applied statical mechanics, solving a problem in dentistry,[12] next in his publication list to his important paper on a quantum field theoretical model of a self-interacting Dirac field which bears his name as a toy model stimulating research to this day.[13] Work on gravitational theory is followed by a precursor of the quark model.[14] The next paper I want to mention here briefly because Thirring frequently refers to it not only in his popular writings: in 1970 the scientific community received with considerable scepticism his claim, that under certain circumstances a system, e.g. our sun or a supernova, will exhibit negative specific heat, i.e. the seemingly paradoxical fact that a system when cooled gets hotter and cooler when heated.[15] The later period of his work is marked by dealing with questions on the ergodicity of quantum systems and quantum chaos. An excellent overview of what he has achieved can be found in Thirring's 'Selecta' with contributions to Mathematical Physics, Statistical Physics, Quantum Field Theory, General Relativity, and Elementary Particle Physics.[16]

As an author of textbooks his influence extended far beyond the inner circle of students attending his lecture courses. Thirring published advanced introductions to quantum electrodynamics[17] and quantum field theory.[18] His four volume textbook on mathematical physics, resulting from a four term lecture course during the 1970's, is considered as the standard text in the field, known at that time to his pupils as typed and mimeographed lecture notes, the *green booklets* ('grüne Hefte').[19] Thirring's argument for the selection of the material presented in the textbooks is clear and brief, a trade mark of his somewhat terse style: *'I decided to cover only those subjects in which one can work from the basic laws to derive physically relevant results with full mathematical rigour'*.[20] It follows, Thirring tells us succinctly, that relativistic quantum theory and other important branches of physics are not taken up because they *'have not yet matured from the stage of rules for calculations to mathematically well understood disciplines'*.[21] This statement, in a sense, sounds very much like an *ex negativo* formulation of Thirring's research programme as a mathematical physicist.

Thirring's autonomy of choice of his research topics is one of the most astonishing observations to make regarding his scientific oeuvre. Till today he is working on the forefront of mathematical physics and at the same time he is a very independent observer of the scene. His view on physics is void of fashions. Is there — one may ask — a specific 'philosophy' guiding Thirring's physical insight, his intuition and choice of subject? He never expressed himself on this delicate point of motivation of his scientific work, as far as I am aware. I will try to formulate it my way: the central epistemological and methodological motor of his style of doing physics is based on a deep intuition concerning the physical relevance of a specific problem, combined with the aim of uncompromising mathematical rigour.

Communication between scientists from both fields, physics and mathematics, and the use of the most recent mathematical tools are integral parts of this mode of operation and has manifested itself on an international scale at the Erwin Schrödinger Institute under Thirring's initial directorship. The credo of Thirring — and of the ESI — is the following: the ultimate criterium for the validity of a physical argument lies in a rigorous mathematical proof based on clearly stated premises. This does not mean that a good deal of physical inspiration and a strong feeling for the right way to formulate a physical problem are not necessary conditions to find one's feet in physics.

This edition of the ESI NEWS would not have appeared and we would not be able to congratulate Walter Thirring on these pages had he not lent all his support, enthusiasm and last, but not least, his reputation as a scholar to the initiative by Austrian and Russian physicists and mathematicians to found an institute dedicated to mathematical physics in Vienna. Without Thirring as an outstanding leader and scientist this dream would not have become reality. Since 1993 his spirit of weaving together the threads of physics and mathematics into one texture is manifesting itself in the Erwin Schrödinger Institute, perhaps Thirring's most visible achievement as a scientific manager. There is a further aspect of the ESI which goes beyond its scientific work: as a meeting place for researchers not only from East and West of Europe but on the global scale it reflects aspirations and hopes of Walter's father Hans Thirring to promote peace in this world.[22]

Let me conclude by citing an observation Thirring made on Schrödinger: *'For him [Schrödinger] the so-called exact sciences are just a little stone in the greater edifice of a philosophical world view'*.[23] Considering Thirring's dedication and love for music, playing the organ and composing, together with his religion-based insight into the great cosmic fabric we may be well justified to take Thirring's words on Schrödinger as a sort of testimony of his own world view.[24]

Notes

[1] *'Ich wollte nie eine Autobiographie schreiben ... Wie jämmerlich schienen mir doch die nostalgischen Kriegsgeschichten alter Herren, mit denen sie beweisen wollten, was sie in ihrer Jugend für Teufelskerle gewesen wären'*. Walter Thirring, Biographical & Bibliographical Series of Classics of World Science. S.S. Moskaliuk, ed., Kyiv 1997, p. 12.

[2] *'... in der Hand eines Demagogen ... beliebig deformiert werden kann'*, Ibid., p. 57.

[3] *'Mein Bruder war drei Jahre älter als ich und galt schon immer als das große Genie, während ich nur das kleine Anhängsel war'*, Ibid., p. 47.

[4] *'die wissenschaftliche Tradition unserer Familie weiterzupflegen'*, Ibid., p. 47.

[5] *'... eine treffliche Wahl'*, Ibid., p. 47. Georg Joos, *Lehrbuch der theoretischen Physik*. Leipzig: Akademische Verlagsgesellschaft 1932.

[6] Cf. e.g. W. Thirring, M. Gell-Mann and M. Goldberger, *Use of Causality Conditions in Quantum Theory*, Phys. Rev. 95 (1954) 1612-1627.

[7] W. Thirring, High Energy Physics and Big Science, Acta Phys. Austr., Suppl. VIII (1971) 12-20.

[8] W. Thirring, A. Wehrl, *On the Mathematical Structure of the BCS-Model*. Comm. Math. Phys, 4 (1967), 181-189.

[9] J.L. Lebowitz, E.H. Lieb, Phys. Rev. Lett. 22 (1969) 631-634.

[10] Walter Thirring, Biographical & Bibliographical Series of Classics of World Science. S.S. Moskaliuk, ed., Kyiv 1997, p. 221.

[11] E.H. Lieb, W. Thirring, *Bound for the Kinetic Energy of Fermions Which Proves the Stability of Matter*. Phys. Rev. Lett. 35 (1975) 687-689. *Errata* 35 (1975) 1116.
W. Thirring, *Stabilität der Materie*. Naturwissenschaften 73 (1986) 705.

[12] W. Thirring, T. Hromatka, *Über die Statik federnd abgestützter Freisättel*. Deutsche Zahnärztliche Zeitschr. 7 (1952) 209.

[13] Walter E. Thirring, *A Soluble Relativistic Field Theory*. Annals of Physics 3 (1958) 91-112.

[14] W. Thirring, *Three-Field Theory of Strong Interactions*. Nuclear Physics 14 (1959/60) 565-577.

[15] Walter Thirring, *Systems with Negative Specific Heat*, Z. f. Phys. 235 (1970) 339-352.

[16] Walter E. Thirring, *Selected Papers of Walter E. Thirring*. Providence, Rhode Island: Amer. Math. Soc., 1998.

[17] W. Thirring, *Einführung in die Quantenelektrodynamik*. Wien: Deuticke 1955. *Principles of Quantum Electrodynamics*. New York: Academic Press 1958, 2nd ed. 1962. Russian Edition.

[18] Ernest M. Henley und W. Thirring, *Elementary Quantum Field Theory*. New York: MacGraw-Hill, 1962. [German transl.: Manfred Breitenencker], *Elementare Quantenfeldtheorie*. Mannheim, Wien, Zürich: Bibliographisches Institut 1975. Russian und Japanese editions.

[19] W. Thirring, *Lehrbuch der Mathematischen Physik*. Wien, New York: Springer Verlag 1977

– 1980. Band 1: *Klassische Dynamische Systeme*. Band 2: *Klassische Feldtheorie*. Band 3: *Quantenmechanik von Atomen und Molekülen*. Band 4: *Quantenmechanik großer Systeme*.

[20] W. Thirring, *Lehrbuch der Mathematischen Physik*. Wien, New York: Springer Verlag 1977 – 1980. Band 1: *A Course in Mathematical Physics. Classical Dynamical Systems*. New York, Wien: Springer-Verlag, 1978, p. xi.

[21] Ibid.

[22] Brigitte Zimmel, Gabriele Kerber (Hrsg.), Hans Thirring. *Ein Leben für Physik und Frieden*. Wien, Köln, Weimar: Böhlau 1992.

[23] *'Für ihn waren die sogenannten exakten Naturwissenschaften nur ein Steinchen in dem größeren Gebäude einer philosophischen Weltsicht'*. W. Thirring, *Einleitung* in Erwin Schrödinger, *Gesammelte Abhandlungen*, Band 4, *Allgemeine wissenschaftliche und populäre Aufsätze*. Wien: Verlag der Österr. Akad. d. Wiss. 1984, S. xii.

[24] Walter Thirring, *Kosmische Impressionen. Gottes Spuren in den Naturgesetzen*. Wien: Molden 2004.

Walter Thirring's Arrival in Vienna

Herbert Pietschmann

In 1959 a number of graduate students at the Institute of Theoretical Physics of the University of Vienna was eagerly waiting for the arrival of Walter Thirring as new head of the Institute. Erwin Schrödinger had retired the year before and the only other (associate) professor of theoretical physics, Theodor Sexl, refused to teach Quantum Mechanics for personal reasons, although he was expert in Nuclear Physics. We graduate students had to organize private Seminars in order to learn Quantum Mechanics and Field Theory from textbooks. (For Quantum Mechanics we had chosen Dirac's book — not exactly the easiest way into this chapter! Field Theory we learned mostly from Walter's first book on Quantum Electrodynamics.)

When Walter Thirring arrived, many things changed spontaneously — the Institute was transformed from a historic place to a modern research establishment. Needless to say, that he taught all the modern subjects in his course on theoretical physics; but at that time it was almost equally important (as well as surprising) that the Institute now had its own secretary! With research grants from the United States, Walter could open an experimental appendix to the Institute of Theoretical Physics: a group evaluating bubble chamber photographs from CERN which later grew into the Institute for High Energy Physics of the Austrian Academy of Science. (After returning from CERN, my first job with Walter was to support this group with theoretical advice.)

Many prominent guests came to Vienna to lecture and stimulate our research. Some of them came for a longer period and created an international atmosphere which also attracted young postdocs. (One of them was A.P. Balachandran from Madras, with whom I published the first papers on Current Algebra.)

Due to Walter's US grants, we were also able to report our research results at international conferences or — for example — travel to the United States to lecture about our work. (In this way, I could report our results on Current Algebra in 1963 at six American Institutes.)

We younger physicists greatly admired Walter's physical intuition and ability to do calculations in the most simple and straightforward manner. Indeed, when he wrote his textbook with Henley, Ranninger and I had to check the equations. At one point, we were unable to get from one line to the next in spite of prolonged and great effort. Of course we were quite ashamed when we came to Walter to confess. He was not the slightest annoyed but simply went to the blackboard, wrote down an Ansatz from general symmetry arguments, did the remaining algebra in his head and said with satisfaction: 'Don't worry, its OK!'. I have to add that we were relieved when a few days later a letter came from Jacobson and Henley asking Walter to add one line for they also had to work for some time before they found the right way to check this equation.

In the early sixtieth there was a fierce fight among particle theorists as to what was the future theoretical basis for the description of elementary particles. Double dispersion relations were the fad of a large group of theorists and many believed, that Lagrangian field theory was old-fashioned and had to be overcome. Indeed, some respectable Universities cancelled all conventional field theory courses and replaced them by 'S-matrix theory'. A culmination point in this fight was the publication of a paper by Zachariason in which he claimed 'we shall construct a model field theory which is ... not defined in terms of a Lagrangian'. If this were possible, it would be quite a victory for the believers in 'S-matrix theory'.

Walter's intuition told him that this 'was not the true Jacob' (to use a formulation of Einstein). For Walter it was quite obvious that Lagrangian field theory could not be abandoned! Soon after the appearance of Zachariasons paper, he published a paper called 'Lagrangian Formulation of the Zachariason Model'. Today we all know that he was right, for the Standard Model of Elementary Particles is of course based on a huge Lagrangian! But in those days it was a minority opinion and not at all obvious. (My own Habilitation was based on the 'Zachariason-Thirring-Model')

In 1966 I worked with Walter on the quark model of elementary particles. During these happy days I learned a lot and again I could admire Walter's physical intuition. For the benefit of younger colleagues let me just tell one story about the old days: We had to work out a three-particle phase space integral, but in those days there were no computers! So we shared the work: I drew the integral on a then often used graph paper and counted the squares. Walter had a slide-rule of one meter length and did a rough computation. At the end, we used an electric calculator which was able to perform the four basic algebraic manipulations. Walter, working on his slide-rule, said the numbers aloud and I typed them into the calculator. In this way — after an hour of work — we did the integral numerically. And this was just about 40 years ago! Times have changed rapidly.

Since 1968 I had my own chair in Theoretical Physics, so I was now Walter's colleague. I am very grateful to Walter for the fact, that we always had a warm and personal climate at the Institute; the usual rivalries and quarrels among colleagues were totally absent during all these decades to the very day! To me, this is one of the finest aspects of Walter's personality, for I could not have lived in an atmosphere of tension, not to mention fight. (Our common interest in music may have been helpful.)

Around this time, Walter became director of the theory division at CERN for three years. It is coincidental, that just then he changed his field of interest from particle physics to mathematical physics. One may deplore the loss of such an intuitive physicist for particle physics, on the other hand, one may applaud the gain for mathematical physics of a person with such a great physical intuition. Without it, mathematical physics may too easily become esoteric.

Walter Thirring and Austria's Accession to CERN

Wolfgang Kummer

In the 1950's the physics institutes in Austria in general and especially at the University and the Technische Hochschule Vienna were still in an extremely poor shape: laboratories still showed the marks of bombings, obsolete equipment — if available at all — from pre-war times, among the students many former soldiers of the German army with large deficits in secondary schooling and a very inhomogeneous body of teaching personal, because only in rare cases well qualified professors had somehow survived the nazi regime. Also the motivation for senior Austrian physicists abroad to return into such an environment (moreover at ridiculously low salaries) was not pronounced for understandable reasons. Under these circumstances it appeared somewhat of a miracle that it became possible to recruit Walter Thirring as a successor to his father Hans for the professorship of theoretical physics at the University Vienna. A fortuitous combination of supports from quite different (yes, also opposite political) camps created the necessary boundary conditions.

How Walter immediately made his institute a focus of international importance in the fields of quantum field theory and mathematical physics will not be described here. Instead due credit must be given to his successful activities regarding Austria's accession to the European laboratory for High Energy Physics CERN. This organization had been created in 1952 as an exemplary European effort to collaborate in the field of fundamental physics at the subnuclear level. For Walter is was inconceivable to enter the map of top level physics research without being a member in the new and — as it turned out soon — extremely successful enterprise.

Especially in small countries like Austria the decision to join CERN to this day usually faces considerable resistance from different opponents: finance ministers hesitate to pay large membership fees to international organizations, most of whom do not exhibit a glamarous reputation for effectiveness. Even more importantly, the dear collegues from other fields of physics and science in general deplore the amount of money which, in their opinion, could be invested much more fruitful in their own fields. Powerful arguments in favour, however, usually come from the political side ('European solidarity'), but especially from the lobbying by industry which always found CERN and its top requirements for technological products an important competitive meeting place. Clearly the last point did not apply to Austria at that time. As a young nobody I, of course, never knew any details about the mechanisms of the political interventions of Walter and his congenial partner Fritz Regler, an experimental physicist from the Technische Hochschule, who politically covered the 'right' wing. After some ups and downs the fortunate consequence was the formal accession of Austria to CERN in 1959. However, for Walter Thirring is was evident that this membership without proper activity in Austria was useless. Therefore, with the help of financial support from a US foundation he created the 'Plattengruppe', an initially small group of doctoral students, who evaluated the tracks of elementary particles in photographic plates which had been exposed to the new high energy accelerator at CERN. This group represented the core of the Institute for High Energy Physics at the Austrian Academy of Sciences which started its work officially in 1966 — again thanks to the joint efforts of Thirring and Regler. The rest ist history . . .

Walter Thirring's Path into Mathematical Physics

Heide Narnhofer

When looking at Walter Thirring's publication list in order to determine a point of transition from elementary particle physics to mathematical physics one soon realizes that there was no turning point: there was a continuous evolution in the sense that he always wanted to make rigorous statements. Only the mathematical tools he used got sharpened in the process. This process reflected a general tendency in the second half of the last century: physicists realized that understanding some parts of physics could benefit enormously from applying more advanced mathematical tools.

I remember that Walter told me a joke: 'In the first part of the last century it was said that the only mathematical tools necessary for physics were a good knowledge of the Greek alphabet.' But already in 1950 Walter was giving a seminar on the scattering matrix in elementary particle physics and Bruno Touschek, a physicist with strong interests in experimental physics and less in the mathematical beauty of the theory, raised the question whether Walter could control the error in the perturbative calculation of the scattering matrix. As a consequence Walter tried to control this error and had to observe the divergence of perturbation theory in quantum field theory (1952).

Walter also in daily life wants things to be clear and straight and is disgusted by any escapes into ambiguity. Therefore it was clear that he was not willing to ignore this fact of divergence and so he was looking for statements where he was not forced to trust arguments without a profound basis. In quantum electrodynamics renormalization seems to offer such a basis. The Thirring model (1958) allowed an explicit solution without referring to any perturbation expansion. The search for the group structure in the family of observations in particle physics (1958) could also be formulated in a profound mathematical language. But already here Walter realized that it is worthwhile to deepen the mathematical background.

An important ingredient of this deepening of his mathematical understanding was the good contact with the mathematical department. With Leopold Schmetterer he started to have joint seminars with the purpose not so much to teach students but out of the desire that he himself wanted to learn the mathematical concepts. Schmetterer and Thirring studied Pontryagin's book on continuous groups. I remember a seminar on ergodic theory based on a course by Konrad Jacobs. In addition Walter followed an advanced course on the theory of manifolds given by Edmund Hlawka. I remember him sitting among the students two rows in front of me. He would have enjoyed a joint research project with Hlawka, clarifying how general relativity is based on the covariance of general manifolds while maintaining the evidence of a Riemannian metric. But for some reason or other this cooperation did not work out.

In the meantime Walter covered all of theoretical physics in a course lasting seven semesters. This required going into details in various disciplines, and as a consequence he got attracted to various subjects of theoretical physics that were close to his taste of combining rigour with intuition. But he was also attracted by the fact that some problems were asking for more delicate mathematics.

We can compare Walter's approach to more mathematically minded problems with the evolution of mathematical physics as a new discipline. In 1960 the Journal of Mathematical Physics was founded, where the term 'Mathematical Physics' indicated a shift of emphasis towards a more mathematical outlook on 'Theoretical Physics'. In 1965 the Communication in Mathematical Physics with the chief editor Rudolf Haag followed. Both journals started with articles on quantum field theory and statistical mechanics. In the first volume of CMP you can find the description what we should understand as mathematical physics, unfortunately this part disappeared in later volumes:

It is one of the goals of this journal to generate among mathematicians an increased awareness and appreciation of current problems in physics, just as we are hoping to acquaint a growing number of physicists with methods and results of modern mathematics. This should not be interpreted as an encouragement of physicists to confine themselves to rigorous mathematical argumentation. Conjectures, intuitive judgement and plausibility arguments indeed have their place, so long as it is made clear that they are not regarded as proofs.

Evidently Walter was attracted by this spirit. In 1967 he published for the first time in CMP, together with Alfred Wehrl, a paper on the BCS-model where they had to combine ideas of convergence with analytic considerations.

But as proposed in CMP, he also enjoyed picking up new methods. I remember my thesis on various conductivity problems. The task was to control in different settings the time evolution and this was possible by calculating the integral kernel and extracting from this kernel the desired information. However, in a joint publication Walter added an appendix that was not really necessary but offered the chance to study in an explicit example the difference between self-adjoint and hermitian operators, a fact that nowadays is standard for more mathematically minded physicists, but in those days had just become apparent.

In 1968 Walter moved to CERN in Geneva, the European center to do elementary particle physics. But Walter, though always very interested in this field, concentrated on other things. He started to study the book of J. Dieudonné on Functional Analysis. He was so much attracted by this first volume that he offered to Dieudonné to read the corrections of the following volumes with the motivation that this would force him to study all the theorems carefully and in detail.

But there followed another experience where the power of mathematics was important. Whereas in disproving the convergence of perturbation theory in quantum field theory he had to realize how mathematics can falsify intuition, now he experienced how mathematics justifies good intuition even if this intuition is against common knowledge. He had to argue heavily with the referee that his result on negative specific heat was correct, but even more, this struggle convinced him that this result was not only correct but also relevant: research on the effect of gravitation in stars followed. Finally he was happy that the dynamical part of the theory could also be justified by computer simulations.

In the meantime important progress in mathematical rigour in quantum field theory was made as well. Back in Vienna he picked up this project in a seminar. But differently to previous seminars, these studies did not inspire him to do his own research. I can only imagine what was the reason: this was mathematics that was against his attitude. Mathematically rigorous perturbation theory in quantum field is a hard job. It is as if you had to find your way in a jungle with the vague hope that after heavy struggles some clearing might appear where you would find some insight into what is important and what not and, at last, some intuition. We now know that this hope was not quite in vain and that some new mathematical structures really turned out to be relevant. But this is not the way how Walter enjoys research. He wants to start with intuition and then to hunt for the appropriate rigorous arguments that would justify his intuition. The more tricks are necessary the more fun.

Somehow typical for this attitude is a result on the continuity of entropy that was needed for the definition of dynamical entropy. I offered a characterization with the necessary dependence on the dimension, based on the idea that first you convince yourself that the largest variation occurs in the abelian case and then you estimate this abelian situation. I was too lazy to calculate the explicit numbers on which Walter insisted. On the other hand, he considered my arguments too boring. Instead he used some integral equations, some resolvent equations, some trace estimates and with the appropriate mixture of these arguments he got explicit numbers. Of course these numbers were not optimal. The optimal estimate was carried through by Mark Fannes on the basis of my idea and has since found its way into the literature.

As I mentioned earlier, Walter's research happened at the time when more advanced mathematics entered into physics. The necessity to control convergence was of course always present. Now it became apparent that one has to be careful in choosing the right topology. Puzzled by scattering theory, Walter realized that the norm topology is useful if you concentrate on the invariance of the group (here time translation) but one has to revert to the strong topology to understand the time limit and to the weak topology to incorporate the existence of bound states. When I was a student, in the textbooks you just found an explanation on the basis of differential equations. In his course Walter taught me how to switch between the viewpoints and apply both ideas of differential equations and of operator algebras and, here especially, the power of different topologies.

Another viewpoint close to Walter's heart is the inspiration coming from classical mechanics. What can be inherited and what becomes different? Here the formulation of classical mechanics in phase space is tailor-made, and quantum theory enters by adding correction terms and by controlling them by estimates in weighted Sobolev spaces. It is still a problem that puzzles Walter and where he has not given up, that inequalities so far do not give the numerical bound on stability of matter that his intuition from classical phase space offered. Here we observe another element in his attitude to mathematical physics: existence proofs are of course important, but you can only be sure to understand the essential facts if you can produce numbers that are close to the numbers observed in experiment.

Stability of matter is a result that he obtained jointly with Elliott Lieb. This collaboration was extremely fruitful and inspiring. Walter and Elliott argue on the same footing and are interested in the same kind of problems. In their joint work it is impossible to assign the various ideas to one or the other. This also becomes evident in a fictitious dialogue that Walter wrote on the occasion of Elliott's sixtieth birthday. It is not complementarity but harmony that inspires and is the source of success.

What were the important achievements

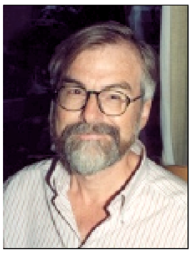
of mathematics that inspired physics in this half century? There was the role played by different topologies. Another example was the realization that for thermodynamics and quantum field theory one needs the initially unfamiliar type III algebras. I remember that as usual I was reading the handwritten manuscript of his book before it went to Franzi Wagner to be typed. There I found the sentence: 'Unfortunately in the thermodynamic limit the algebras turn out to be type-III.' This raised a discussion that made Walter remove the word 'unfortunately', though he did not go as far as to write 'fortunately' as I had suggested: this is really the source that enables us to find qualitatively different behaviour in the microscopic and macroscopic world without the necessity to draw a sharp border.

The Tomita-Takesaki theory, discovered more or less independently by mathematicians and physicists, makes it possible to make statements on ergodic properties in the thermodynamic limit also in the quantum setting, quite in the spirit of Boltzmann. Mathematics developed further in this direction. Alain Connes inspired the connection between various disciplines of mathematics and founded the concept of noncommutative manifolds. Together with topology this opened the possibility to find connections between the microscopic and macroscopic world. Walter was always interested in these developments; however, they did not really inspire his intuition. He is not a mathematician who is looking for beautiful structures, but he uses physical models as inspiration. Walter is a physicist with all his heart whose imagination is very much influenced by what can be observed. The macroscopic world is the world with which we are familiar, and from this macroscopic world he looks for his inspiration for the quantum world that he wants to build on exact mathematical statements.

This note presents of course a rather personal view, resulting from many discussions and anecdotes that Walter told me, and observations in our collaboration. Yet memory can be deceptive and I apologize for possible errors and misinterpretations. In any case I do want to thank Walter for the profound education and guide to relevant problems, and even more for a long collaboration in which I explored the beauty and the fun in guessing, substantiating the wishful thinking and finally hunting for the justification. This is one of the ways to attack physical problems and Walter has always been a master in it.

Unmathematical and Mathematical Physics

Bernhard Baumgartner



My first impression of Walter Thirring was at a talk on the thermodynamics of stars. He was at that time director of the theory division at CERN, visiting his home university in Vienna. I was a student of physics at the time. Having fallen in love with mathematics at the beginnings of my studies, but being mainly interested in physics, I was happy to hear about the existence of 'Mathematical Physics'. And this branch of physics was led in Vienna by Walter Thirring, well known in the physics community (and also beyond). Now I saw this man of high reputation, short and sturdy, talking about such a big object — a star! – without much ado, but with certitude: *'The star gives away energy, contracts, and becomes hotter.'* For me as a student, this was breath-taking — wasn't this contrary to text-book physics? A 'Negative Specific Heat'! But Walter Thirring together with Peter Hertel had proved this with mathematical rigour,[1] followed by investigations including more details.[2] So I got acquainted with Mathematical Physics, appreciating its exactness.

Only in later years I realized that this was just one of the extremal points of science. Another one was actually the beginning of any investigation: it is the art of model building, using intuition and 'Unmathematical Physics'. Each model of a system is thought to represent some specific aspects of nature. To be able to concentrate on those elementary details of natural laws which are essential to what is going on, in order to highlight the principles, one has to discard the other details. In the model for stars mentioned above one disregards relativity, the multitude of elements, and — hard to swallow — the nuclear reactions. On the other hand one has to invent some unnatural hypotheses: containers for the stars and a limit of particle numbers going to infinity, coupled with a limit of the volume going to zero, keeping the proper relations between mass, size, energy and temperature. This is a kind of Thermodynamic Limit adapted to Thomas-Fermi Theory.

The fact that such a procedure is sound, not crazy, is realized by making observations in the realm of Unmathematical Physics, also designated by Walter Thirring as *Größenordnungsphysik* (physics of orders of magnitude). In the case of a star with 10^{57} electrons and positrons in a volume with diameter $1.4 \cdot 10^9 m$ it tells us that the system behaves not really differently from its description by Thomas Fermi theory. Only to reach the goal of indisputable mathematical exactness one has to consider the particles as confined and has to enact the limiting procedures.

For the readers coming from other fields I would like to point out some of the ideas: in the model which has to represent the thermodynamic properties of stars, one discusses a gas of Fermions. In reality each star radiates away energy and mass, no container exists. But in mathematical investigations of statistical mechanics one has to incorporate a vessel in order to prevent evaporation. Considering the orders of magnitude one finds that this evaporation is 'slow' in comparison with both the movements of particles and the approach to local thermal equilibrium, the error in discarding it for the moment is small. Then comes an important mathematical ingredient: the Virial Theorem. It implies that lowering the total energy leads to contraction, to the rise of kinetic energy and to the rise of temperature. These changes appear in mathematical investigations as functions of the theoretical input, whereas in reality they happen in the course of the birth of a star, followed by the ignition of nuclear fusion. Again these changes of the constituents happen much more slowly than the local processes establishing the thermodynamic equilibrium. Of course it would now be interesting to discuss the history of the theory of stars: how Lord Kelvin estimated that chemical reactions, as for example the burning of coal, give less energy than gravitational contraction; that this is still not enough, enabling a shining of the sun for only some millions of years; also how Houtermans and Gamow found out that nuclear fusion was the main source of energy. It was also of great importance that the mathematically exact discussions of the model describe an unusual kind of a phase transition, related to the collapse of a star after the nuclear fuel has been consumed.

Use of 'Größenordnungsphysik' farsightedly reveals that throughout history all the 'giants of science' (on whose shoulders we are standing) had the right intuition to create models, followed by rigorous investigations only afterwards. However, the final presentation is often the other way round: first comes the law, then the unmathematical popular explanation. For example Newton's Third Axiom, *'To any action there is always an equal and opposite reaction'* is followed by *'If anyone presses a stone with a finger, the finger is also pressed by the stone'*.[3] For the 'average' scientist, however, the danger of making fundamental errors in following misconceptions is not to be ignored. So, up to the middle of the twentieth century, little Unmathematical Physics can be found in textbooks. But nowadays the enormous amount of detail in modern physics necessitates its use in order to get a general picture. Walter Thirring gave a course on such themes, perhaps relaxing somehow after having finished the first draft of his enormous four-volume work on mathematical physics. At the start he stated its motto *'Es wird nie eine Gleichung aus der anderen folgen* (*'Never will one equation follow from the other one'*).

Here is the list of contents of Thirring's course on Unmathematical Physics:

- Gravitation (astronomical data in orders of magnitude for earth, moon, sun, Jupiter, milky way and the universe; the virial theorem and its applications; clouds of gas);
- Electromagnetic phenomena in atoms in a semiclassical way à la Bohr-Sommerfeld theory and its relativistic corrections;
- Compressibility of atoms and its use in finding the changeover from planets to stars;
- Structures of atoms;
- The role of the Pauli principle in atoms and stars;
- Chemical binding;
- The Periodic System of the elements;
- The question concerning existence of negative ions (*'H^{--} zerbricht beim Anschauen'* (*'H^{--} breaks when looked at'*). and *'There are three wrong proofs for its non-existence'*);
- Oscillations and rotations of molecules;
- Aspects of solid state physics;
- Black body radiation, its use to determine the temperature of the solar surface by 'Daumenpeilung' (taking a bearing by rule of thumb), and some aspects of a light bulb;
- Spectral lines (semiclassical aspects of their formation and line width);
- Entropy (orders of magnitude, Gibbs paradox, photons, phonons, Fermions)

(My thanks go to Helmuth Urbantke for keeping the records.)

The spirit of this course provided me with a firm foundation for my lecturing on such diverse themes as solid state physics (together with Gero Vogl), physics for prospective teachers and *Principles of Modern Physics* (a lecture course initiated by Herbert Pietschmann). Walter Thirring is still a good source of ideas in this spirit.[4] His influence has been and will always be of immeasurable value.

Notes

[1] W. Thirring, *Systems with Negative Specific Heat*, Z. Phys. 235 (1970), 339–352.

P. Hertel, W. Thirring, *A soluble Model for a System with Negative Specific Heat*, Ann. Phys. 63 (1971), 520–533.

P. Hertel, W. Thirring, *Free Energy of Gravitating Fermions*, Commun. Math Phys. 24 (1971), 22–36.

P. Hertel, W. Thirring, *Thermodynamic instability of a system of gravitating Fermions*, in Drr H.P. (Ed.), *Quanten und Felder*. Braunschweig: Vieweg, 1971.

[2] P. Hertel, H. Narnhofer, W. Thirring, *Thermodynamic Functions for Fermions with Gravostatic and Electrostatic Interactions*, Commun. Math Phys. 28 (1972) 159–176.

B. Baumgartner, *Thermodynamic Limit of Correlation Functions in a System of Gravitating Fermions*, Commun. Math Phys. 48 (1976), 207–213.

B. Baumgartner, H. Narnhofer, W. Thirring, *Thomas-Fermi Limit of Bose Jellium*, Ann. Phys. 150 (1983), 373–391.

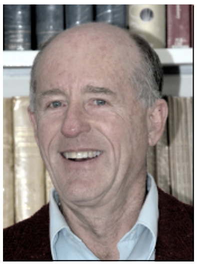
[3] Isaac Newton, *The Principia*, A new Translation by I. Bernard Cohen and Anne Whitman. Berkeley, Los Angeles, London: University of California Press, 1999.

[4] W. Thirring, *Kosmische Impressionen. Gottes Spuren in den Naturgesetzen*, Wien: Molden Verlag, 2004.

C. Faustmann, W. Thirring, *Einstein entformelt*, Wien: Seifert Verlag, 2007.

Gravitational Matters

Helmuth Urbantke



In 1959 W. Thirring was appointed as full professor at the University of Vienna and immediately started his 7-semester lecture course on Theoretical Physics. This lecture course was accompanied by a set of mimeographed notes *Guidelines for Students of Theoretical Physics* which are reproduced on p. 10.

In his Classical Electrodynamics course he included, as an appendix, his alternative special-relativistic approach to General Relativity (GR), based on two of his papers.[1] This is how many of us were introduced to the subject. In particular, Roman U. Sexl took up this approach and used it to classify rival theories of GR in their linear approximations.[2]

The year 1965 constituted a turning point in Thirring's interests, directing them towards mathematical physics — perhaps also triggered by a seminar, organized jointly with the mathematician Leopold Schmetterer, on I.E. Segal's (then) recent book *Mathematical Problems of Relativistic Physics*.[3] On visiting the London conference on GR that year, Thirring became impressed by the power of the geometric arguments of Roger Penrose in fighting his battle with the Landau-Lifshitz-Khalatnikov school concerning singularities in GR. While the buildup of a group working on GR in the following years was left largely to R.U. Sexl, Thirrings main contribution to gravitational physics is nonrelativistic: his quantum statistical analysis of stellar equilibrium from first principles.[4] In the relativistic regime, i.e. in GR, a contribution was to point out advantages of the use of differential forms, in particular in the context of the variational formulation, conservation laws and energy 'pseudo-tensors' (which were formerly based only on coordinate bases). This got published in the second volume[5] of his four-volume textbook on Mathematical Physics.

This textbook, referred to as 'monumental' by Nobel Laureate S. Chandrasekhar upon a visit to Vienna, first appeared in the form of mimeographed lecture notes of a four-semester course on mathematical physics and was subsequently published by Springer Verlag (several editions, from 1977 on). The basic idea was to include only subjects where it is now possible to go from fundamental laws to physically relevant results without mathematical gaps, thus eliminating what W. Pauli called 'wishful mathematics' and W. Thirring calls 'Darwinian physics'. In compensation, each of these topics is introduced by heuristic order-of-magnitude remarks. Actually, many of these, and many other heuristic considerations (in part due to V. Weisskopf) were presented by Thirring in a one-semester course entitled 'Unmathematical Physics' (cf. p. 8). It is a great pity — not only for students! — that this course was given only once and has never been written up. In contrast, courses along the lines laid down in the four volumes were given by Thirring regularly till the nineties, and many members of the Vienna Institute of Theoretical Physics had the opportunity to give lecture courses based on (various parts of) Thirring's course at the then recently founded SISSA (Scuola Internazionale di Studi Avanzati) in Trieste during the 1980's. In fact, this set of courses was included in the SISSA curriculum.

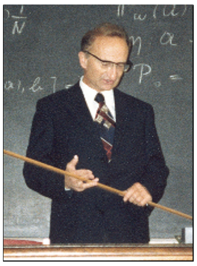
Returning to Gravity, it is perhaps needless to say that there is a continuing interest on his part in 'Machian' matters, in particular in the experimental results concerning the (H.) Thirring-Lense effect. Machian questions were touched upon in the dissertation of F. Embacher[6] under Thirring's supervision. An early question, dating from the fifties, was the discussion of quantum fields in external gravitational fields; actually, the question came too early in the sense that the relevant global aspects of gravitation, which lie beyond the domain of the linear approximation, were unknown or unnoticed at the time (like J. Synge's 1950 analysis[7] of the global structure of Schwarzschild spacetime). Thirring once told us that he had even discussed particle creation by deep gravitational potential wells with A. Einstein — who characteristically did not like the idea.

Notes

[1] W. Thirring, Fortschr. Phys. 7 (1959), 79. W.Thirring, Ann. Phys. (N.Y.) 16 (1961), 96.
[2] R. Sexl, Fortschr. Phys. 15 (1967), 269.
[3] I.E. Segal, *Mathematical Problems of Relativistic Physics*, Providence, R.I.: Amer. Math. Soc., 1963
[4] P. Hertel, H. Narnhofer, W. Thirring, Commun. Math. Phys. 28 (1972), 159.
[5] W. Thirring, *Classical Field Theory*. A course in Mathematical Physics, vol. 2, second edition. New York, Vienna: Springer Verlag, 1978.
[6] F. Embacher, *Machian Effects and Methods of Solution for Einsteins Field Equations in the Case of Cylindrically Symmetric Stationarily Rotating Matter*, University of Vienna, 1980 [in German].
[7] J.L. Synge, Proc. R. Irish Acad. A 53 (1950), 83.

Guidelines for Students of Theoretical Physics

Walter Thirring



The following notes by Walter Thirring were handed to students of his lecture course on Theoretical Physics in 1959. The English translation is due to Helmuth Urbantke.

Theoretical Physics has become an extremely broad field whose mastery presents steadily increasing difficulties. It is, therefore, essential to take the most economic path to avoid unnecessary waste of time. For this reason, in the following I shall give some guidelines to help students find a suitable path through the jungle of modern Theoretical Physics.

1. Tools and Aids

For the understanding of Theoretical Physics, some aids from other parts of science are needed. Since the material is presented in a mathematically deductive manner, knowledge of mathematical language is indispensable; but even knowledge does not suffice — it is mastery of mathematical techniques that enables independent work. Mainly the following branches of mathematics are needed:

a) Linear algebra (vectors and tensors)
b) Differential and integral calculus
c) Differential and integral equations
c) Complex analysis

This material has to be acquired by the student from lectures and suitable textbooks.

Furthermore, in Theoretical Physics education in experimental and general physics is required. This will be, in the first place, knowledge of phenomena and of related terminology. There are lecture courses on these as well as technical literature. Since a large part of the latter is written in English, knowledge of the latter is likewise needed urgently. To people who want to engage in Theoretical Physics as a profession, active collaboration in experimental work is strongly recommended. All this knowledge is taken for granted in Theoretical Physics, and in lectures one cannot go into details about it.

2. General Advice

While studying Theoretical Physics, everybody will go through a few growing pains which are not to be taken too seriously; but it will be well to be informed about them. In the following, we will give a few words of advices in this respect. To the beginner, combination of mathematical formalism and physical ideas is alien, and it will be important to be clear about significance and order of magnitude of the symbols at each step of calculation. One should believe to have understood a result only if one is able to reproduce from memory all the steps involved in its deduction. It is not sufficient to be able to follow these steps by reading them. To counter these difficulties, the following are useful:

a) Discussions

An essential means to convince oneself of having reached understanding is discussing it with others. It is only if one has an overview of objections and suggestions of different kinds that one may judge the soundness of one's own approach. In discussions, all shyness and vanity must be overcome, and one must not be content before the objective state of affairs has been honestly ascertained. . . . It is also strongly recommended to physics students to attend seminars, because there will be more room for discussions and questions than during the lecture courses. In particular, PhD students of Theoretical Physics should, besides their work on their theses, participate in those seminars on principle.

b) Exercises

The execution of typical exercises and problems is as important for the physicist as is exercising for the pianist. This is why lectures are accompanied by problem sessions. It is not sufficient to calculate and calculate until some result is produced: a major part of the labour has to be invested into ascertaining the correctness of the result and discussing its significance. On the one hand, one should deduce the result by different methods, pondering about the most direct one. On the other, one must inquire about the physical significance and order of magnitude of the result; by plausibility considerations, one should see the approximate validity of the result. A frequent mistake is to write down a result with many decimal places without making sure that the very first decimal place is correct.

c) Lectures

One recommended way — but not the only one — is to attend lectures; one should be careful, however, not to attend too many of them, since it is impossible to digest fully more than two or three per day. Nevertheless, it is recommendable to work through a subject following a lecture course even if one knows it already. In general, one gets an overview of a subject only if light is shed upon it from different angles, i.e., if one has encountered it both in lectures and textbooks. Just taking notes from a lecture course is not sufficient; one must be able to reproduce the material from memory.

d) Literature

For most parts of physics, there are excellent textbooks that enable one to work through the material. However, these books cannot be read like newspapers to be useful, but only with paper and pen, so that one may perform all calculations oneself. There are many books whose reading is dangerous for beginners. In part, they are out of age, so that notation, presentation and content do not any more correspond to contemporary knowledge. Also, many books by famous scientists contain suggestions which later turned out to be useless: studying them means waste of time without much gain . . .

The notes end with a list of recommended and not recommended textbooks. Among the textbooks which were not recommended was Arthur Stanley Eddington's 'Fundamental Theory' and Arnold Sommerfeld's 'Atombau und Spektrallinien' (out of date) and, generally, all textbooks without new editions after 1930.

Walter Thirring: Probing for Stability

Harald A. Posch

Almost 40 years ago, Walter Thirring, then member of the board of directors at CERN in Geneva, faced a paradoxical problem: astronomers had known for almost 100 years that extracting energy from a star, for example by radiation, would make it collapse and heat it up. V.A. Antonov even showed in 1962 that no equilibrium state with a global maximum of the Boltzmann entropy exists for a sufficiently large system of point particles interacting with a $1/r$ potential. This means that the particles form a cluster and continue to collapse for ever with their temperature diverging to infinity, even if the total energy is constant. The British astronomer D. Lynden-Bell realized that an explanation of this so-called *gravothermal catastrophe* in thermodynamic terms requires the specific heat (or more correctly the heat capacity) to become *negative*. The paradox was that physicists "knew" that the specific heat of ordinary matter could be expressed in terms of the *square* of the energy fluctuations and, as a consequence, had to be *positive*. Walter, at about the same time and independent of Lynden-Bell, realized the significance of the negative specific heat and "risked his reputation" (in the words of the editor), when he published an article in 1970[1] in which he explained the difference and resolved the paradox: physicists were used to specifying the state of matter by the temperature in contact with a heat bath and, hence, within the framework of the canonical ensemble. There, the specific heat is indeed always positive. Stars and star clusters, however, may be considered as isolated objects with constant total energy, for which the microcanonical ensemble provides the proper setting. And there the specific heat may become negative, depending on the particle interaction and the energy. This provided the first significant example in statistical mechanics for the results of different ensembles to disagree.

The problem of negative heat capacity has continued to interest Walter ever since. Most notably, he showed that negative specific heat is related to other notions of stability, namely i) *extensivity* (stability against implosion), ii) *subadditivity* (stability against explosion), and iii) *convexity of* $E(S)$ (thermodynamic stability). According to a theorem by Walter Thirring, the three stability conditions are intimately connected: each pair of conditions implies the third.[2]

The dynamics of thermally unstable systems has also provided the background for my collaboration with Walter up to the present day. Earlier than most theoretical physicists, he accepted computer simulations of complex dynamical events as a legitimate tool to check and guide the intuition and to study systems still outside of present-day theory. In 1989, in collaboration with H. Narnhofer, we conducted a series of computer simulations with a simplified model of 400 particles in a box interacting with a short-ranged attractive potential.[3] We established the unusual thermal properties of negative specific heat in a certain range of energies and studied the particle-number dependence. As an example, in Figure 1 a typical particle cluster is shown, which forms after the simulation has been initiated with a homogeneous gas.

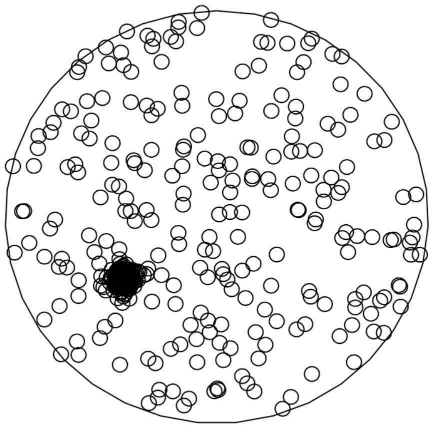


FIGURE 1: 400 ATTRACTING PARTICLES IN BOX. CONFIGURATION AT A TIME $t_0 = 3000$. THE PARTICLES ARE REPRESENTED BY CIRCLES WITH DIAMETER EQUAL TO THE INFLECTION POINT OF THE NEGATIVE GAUSS POTENTIAL.

By coupling to a heat bath, we also explicitly demonstrated that the negative heat capacity disappears and is replaced by a first-order clustering phase transition when the simulations are carried out within the canonical ensemble.[4]

The next logical step with such an unfamiliar matter was to check for the Second Law. We performed a computer experiment of an adiabatic cyclic process, where the volume of the box was periodically expanded and contracted, reducing and raising the energy in the process.[5] During the expansion the cluster formed and disappeared again during compression. The net effect always was an increase of the system's energy, which excludes the possibility of a perpetuum mobile of the second kind. The coarse grained entropy increased both during the expansion and compression, but only when it was of the Boltzmann type. Other entropies with coarse graining solely in the momentum or configuration space failed the test. Using the concept of passivity, Walter also demonstrated, for an ensemble of harmonic oscillators with periodically-switched force constants, that active states, which violate the Second Law, are located on a fractal set in phase space with a measure which vanishes in the many-particle limit.

But a star is threatened not only by the gravothermal effect, which would cause it to implode and heat up in the process. It is also subjected to a second gigantic instability, the thermonuclear heating in it's core. The later which would cause it to explode if unchecked. Both instabilities, the gravothermal effect and thermonuclear energy input, are intimately linked together and neutralize each other for billions of years. The negative heat capacity changes the bifurcation point due to exponential nuclear energy input and radiative energy losses into a fixed point. But beware, if eventually one of the gigantic instabilities gets weaker than the other! Recently, we exhibited this delicate balance with two simple examples.[6] The first is the many-body system of Figure 1. If energy is *added* to the equilibrated particles in the main cluster for times $t > 3000$, the temperature of the cluster particles goes down as depicted by curve B in Figure 2.

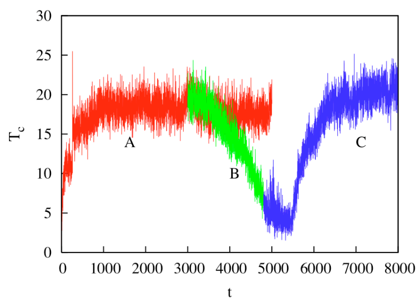


FIGURE 2: TIME EVOLUTION FOR THE TEMPERATURE T_c OF THE LARGEST CLUSTER. LINE A BELONGS TO THE EQUILIBRATION STEP, LINE B TO THE HEATING STEP, AND LINE C TO THE FINAL HEATING/COOLING STEP.

If, in addition to heating the cluster particles, the system is also subjected to a cooling process at the boundary supposed to mimic radiation loss off the star, the temperature goes up again until it reaches a stationary state. This is shown by curve C in Figure 2. This provides a beautiful example for the mutual stabilizing effects of the two otherwise destructive phenomena.

The second model emphasizes another interesting observation:[7] negative specific heat means that the volume of the energy shell expands more than exponentially with energy. In gravity-dominated systems the formation of clusters always causes heating, that is expansion in momentum space. If, however, more than exponential expansion is provided in configuration space, the system may even cool down and still show negative specific heat. The model Walter proposed is shown in Figure 3.

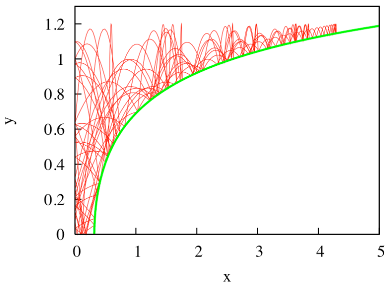


FIGURE 3: JUMPING BOARD MODEL AND SHORT CHAOTIC TRAJECTORY FOR A PARTICLE. THE GRAVITATIONAL FORCE POINTS INTO THE NEGATIVE y DIRECTION. AT THE BOTTOM, THE PARTICLE IS ELASTICALLY REFLECTED. TO AVOID NEGATIVE x, THE y AXIS ACTS AS AN ELASTIC MIRROR.

A particle, or an ensemble of weakly interacting particles, is subjected to a gravitational field parallel to the negative y axis and confined to a valley, which opens up very quickly such that the accessible configuration space increases faster than exponentially with the energy (respective the maximum height the particle may jump to). The farther the particle gets to the right in this way, the more potential energy is used up and the colder the particle gets, a clear sign of negative specific heat. The non-equilibrium version of this model (the particles farthest to the right are allowed to fall out of the potential well) may also be looked at as the most-efficient form of evaporation cooling, a process extensively studied to generate ultra-cold gases in connection with Bose-Einstein condensation. This simple model is also the first mechanical device with negative specific heat, which could be built in the laboratory, at least in principle.

The gravitational collapse of a few particles in a box due to the gravothermal effect may also be prevented, if the particles are subjected to external constraints, which reduces the allowed phase space such that the system asymptotically remains stable. We studied, for example, what effect momentum and/or angular momentum conservation has on such a system. Three particles in a square box in two dimensions will collapse for ever, in a circular box the collapse comes to an end in a finite time due to angular momentum conservation present only in this case.[8]

Recently, the problem of negative heat capacity was front-page news again, when laboratory experiments on nuclear fragmentation and atomic clusters hinted to the existence of this phenomenon also for ordinary Coulombic matter, for which gravitation could not be the culprit. This seemed to contradict a general result by J. L. Lebowitz and E. H. Lieb according to which a quantum-mechanical Coulomb system consisting of electrons and nuclei always has positive specific heat, even in the microcanonical ensemble. Of course, this theorem presupposes ergodicity and the thermodynamic limit. We could demonstrate by computer simulations and simple model calculations that the failure of any of the two assumptions may be sufficient to render the specific heat negative in some restricted range of energy.[9]

I had the good fortune to study many other problems with Walter. I would like to mention the problem of ergodicity and the Lyapunov instability of one-dimensional gravitational systems,[10] the unpredictability of symmetry breaking in phase transitions,[11] and some aspects of the classical three-body problem.[12] The work on these problems always brought to light the deep intuitive insight Walter has of the physical world, which is rivalled only by his extraordinary mathematical skills he brings up when solving a problem. As befits a great physicist, most recently Walter Thirring has turned also to another outstanding problem, the study of evolutionary processes leading to the formation of structures, to the selection of species. Again, the computer turns out to be an invaluable tool and provides the opportunity for me to participate in this adventure.[13] For this, and for his generosity in all scientific and private matters I am forever grateful.

Notes

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[2] W. Thirring, *Quantum Mathematical Physics*, 2nd edition, Springer Verlag: Berlin, 2001.
[3] H.A. Posch, H. Narnhofer, and W. Thirring, Phys. Rev. A 42 (1990), 1880.
H.A. Posch, H. Narnhofer, and W. Thirring, *Microscopic Simulations of Complex Systems*, M. Mareschal ed., New York: Plenum Press, 1990, p. 241.
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[6] H.A. Posch and W. Thirring, Phys. Rev. Lett. 95 (2005), 251101.
[7] H.A. Posch and W. Thirring, Phys. Rev. Lett. 95 (2005), 251101.
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[8] Lj. Milanović, H. A. Posch, and W. Thirring, J. Stat. Phys. 124 (2006), 843.
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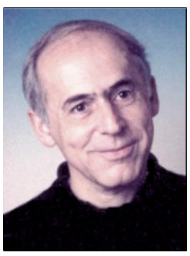
[13] Ch. Marx, H.A. Posch and W. Thirring, Phys. Rev. E, in press (2007).

Almost 50 Years of the Thirring Model

Harald Grosse



The formulation of renormalized perturbative quantum field theory by Dyson, Feynman, Schwinger and Tomonaga predicted observable corrections due to quantum fluctuations, which are confirmed by experiments to an extremely high precision. After the construction of a solvable field theory by Lee many people tried to obtain a similar relativistic model too. The well known Bethe Ansatz, successfully applied in the thirties to the Heisenberg chain model, served as a guide.

Heisenberg proposed in the fifties a Four-Fermi Interaction Model ('Urformel'), which, as was noticed immediately by Pauli, led to difficulties, since it is not renormalizable.

The analogous two-dimensional model, formulated already by Tomonaga in the early fifties, was studied by Walter Thirring and published 1958. With the help of the Bethe-Ansatz he was able to solve the appropriate many particle system in a Hilbert Space of fixed particle number. The next step to change the representation for which the spectral condition is fulfilled needs renormalization and is tricky. Within the physical correct representation Thirring was able to calculate the two particle correlation function with one particle on shell, which is called the form factor.

Soon after this work Glaser proposed an operator solution to the model and obtained surprisingly for the form factor an answer which differed from Thirring's answer by a change of coupling constant.

This puzzling situation was analyzed by a number of people like Johnson, Klaiber, Wightman and others from various points of view. Nowadays, of course, it is easy to state that only renormalized quantities make sense and the step from bare to renormalized ones depends on the renormalization scheme. Klaiber wrote down a generalized solution of the massless model.

In 1962 Schwinger extended the set of solvable models. Two-dimensional QED shows mass generation and appearance of a theta vacuum. It was a common effort of many physicists and mathematicians, especially Australians around Carey, to establish Wightman axioms for these models. Two-dimensional conformal field theory ideas can be tested again on the massless model and allow for many generalizations.

Of course, nowadays, an infinite number of solvable, respectively almost solvable integrable models are known and studied in detail. Especially the algebraic structure behind the Yang-Baxter equation led to the invention of quantum groups which generalize the Lie algebra structure.

Despite many attempts (also by myself), the massive Thirring model resists a complete solution. Assuming factorization, unitarity, crossing and no particle creation it was possible to deduce a possible S-matrix. Again it was possible to obtain the form factor, but no higher correlation functions have been obtained in closed form. On the other hand from constructive methods the existence of these higher correlation functions has been derived.

Coleman's duality allows to map the massive model to the Sine-Gordon model with all its solitons, antisolitons and breathers. This generalized the Kramers-Wannier duality of the Ising model substantially, and was extended through work of Olive-Montonen and many others to the fascinating geometric Langlands duality concept, which became popular recently through work of Kapustin and Witten. Such unforeseen developments show the power of toy models which seem to be unphysical at first sight, but allow to study physical effects in a nutshell.

The completely independent developments (almost at the same time) in statistical physics are worth mentioning. The well-known Luttinger model represents a kind of lattice-regularized Thirring model, were renormalization effects can be studied easily. Its first correct solution is due to Mattis and Lieb. Its physics implications are nowadays well established: conductivity along one-dimensional quantum wires is well described by the Luttinger liquid.

It is interesting to note that the two main proponents of these developments in two different fields, in two-dimensional quantum field theory and two-dimensional statistical physics, joined and obtained a substantial improvement of the stability of matter proof of Dyson-Lenard. Although Thirring's calculation of the distribution of pressure of teeth might be of more practical importance, the first analysis of the two-dimensional spinor model gave a great impulse and led to many generalizations. The still puzzling situation of the four-dimensional local or even nonlocal quantum field theory models forces us even nowadays to study toy models in lower dimensions.

During the almost 50 years which have passed since the first study of the massless Thirring model, it has served as a toy model and a testing ground for general ideas and phenomena. A similar four-dimensional model is still to be discovered; the best candidates are $N = 4$ or even $N = 8$ supersymmetric models.

A Crossword Puzzle Without Clues[1]

Can one find a 'Theory of Everything' by pure thinking?

Peter C. Aichelburg

I dedicate this article to my teacher Walter Thirring who not only introduced me to modern theoretical physics, but also guided my scientific interest towards Relativity.

Modern theoretical physics is confronted with a dilemma. The standard model of particle physics is in excellent agreement with experiments and Einstein's theory of gravitation is confirmed every day when applied to the satellites of the global position system. However, the decisive step towards a complete theory describing our physical world is missing: the unification of *quantum theory* and *gravitation*. The dilemma is that nature does not seem to give us any clue how to achieve this.

It is fair to say that, over the past decades, the brightest scientists in the field have devoted their efforts to this question. A number of challenging ideas are on the scientific market, especially modern *string theory* which tries to unify all fundamental forces. But string theory has recently come under severe criticism, even from experts in the field. The problem is experimental verification: although most of these proposed theories may be falsified in principle in the sense of Popper, any of their expected consequences are far too small for today's methods of detection.

Revolutions in physics were almost always triggered by observations which were either in conflict with, or could not be explained by, current theories. For example, the emission of electromagnetic waves with definite frequencies by atoms was in contradiction with classical electromagnetic theory and led to the formulation of quantum theory, forcing physics to abandon a deterministic description of the world.

Today, however, the situation is different. Since there are no observations which are in contradiction with the established theories, an essential guiding principle is absent. The problem may be compared to a crossword puzzle without clues, where it is difficult, but not impossible to solve the puzzle. The only guideline is internal consistency i.e. that the letters at the intersections of words must agree. Moreover, the puzzle should admit only a single correct solution.

If this argument were to be applied to the physical description of the word we could do away with the sophisticated experiments in particle physics and costly large telescopes for astronomers. All that would be needed are mental efforts of theoreticians.

In fact, never before have so many scientists dedicated themselves to the construction of theories which are far beyond any observational verification, the only criterion being internal consistency and convergence to existing theories when taking suitable limits. However, in pursuing the analogy with a crossword puzzle a little further, an essential feature of the problem is missing. Even if the solution is unique it can only be found if the number of 'squares' in the puzzle is finite. An infinite puzzle can not only not be solved completely, but unfortunately also not partially. It may happen that one has found a consistent solution of, say, the upper left corner, but that one runs into contradictions at a later stage. This forces one to go back and abandon what has already been obtained. If the puzzle is unbounded, this may happen again and again at every stage.

Without no generally accepted theory at hand it is not too astonishing that sometimes the fantasies of scientists go astray. Every now and then the public is confronted with new and surprising ideas about the origin of the universe. For example, that our cosmos is the result of a vacuum fluctuation and that these processes take place continuously and our world is therefore just one in an infinite 'multiverse'. Or that we live on a kind of membrane embedded in a higher–dimensional space, where the extra dimensions manifest themselves purely through the force of gravity. If one follows the well known physicist Lee Smolin in his book *The Life of the Cosmos*[2] one learns that for the universe a cosmological *natural selection* acts similarly to biological evolution.

What should we think of such ideas? Without doubt, there is today not a single observation which supports these speculations. In contrast, the Nobel Price 2006 in Physics was awarded to the American scientists John Mather and George Smoot for the detection of the structure and exact form of the cosmic microwave background. These observations give strong support to the big bang theory. They show a detailed picture of the universe of more than 13 billions year ago and made cosmology a precise science.

How do these observations compare to claims made about the emergence of our cosmos? We are confronted with a questionable embellishment of observation–supported knowledge on the one side, and with imaginative speculations on the other. Such speculations reach the public through popular and semi-popular books and are apparently well received. In our secularized world, where an increasing number of people find it difficult to unite 'rationality' and 'religious faith', such concepts take the place that traditionally was provided by religious genesis. Religious faith is replaced by scientific faith. But only few are aware that the scientific basis of these statements is rather meagre.

Not trying to make a case for intelligent design, it is fair to say that up to now there does not exist a theory which would allow us to model the beginning of the universe. Einstein's theory of General Relativity gives a dynamical description of the cosmos as a whole, but shows at the same time its limits of applicability. The big bang, where the density of the substrate that fills the cosmos increases unboundedly, thereby driving the geometry into a singularity, lies outside the scope of the theory. The theory cannot make statements about the big bang itself.

To overcome this problem one would need a theory which unites Einstein's gravity with quantum theory, the theory that governs interactions at the microscopic level. Such a 'theory of everything' is the dream of many physicists. Although it might be possible to find a unique consistent theory of nature by pure thought, it is very unlikely. Knowledge about our universe has to be based and oriented on observation.

What then is observation–based knowledge in modern cosmology? The main assertion is that the universe undergoes a continuous change and that it has emerged from a dense state about 14 billion years ago. This big bang theory is supported by three key observations:

(i) The shift in frequency of light coming from distant galaxies which indicates that the cosmos as a whole is subjected to a general expansion.

(ii) The cosmic microwave background radiation confirms that the early universe was dense and hot.

(iii) Physicists were able to calculate the formation of the first generation of light atomic nuclei in the universe. These calculations are based on laws for subatomic matter tested in experiments at accelerators and reactors. The theory for the primordial nuclear synthesis turned out to be in excellent agreement with observations.

On the other hand there is the serious and so far unsolved problem of *dark matter*. As the name says, dark matter can not be seen directly and its presence is solely noticed through its gravitational effects, e.g. the bending of light rays which reach us from distant objects. Dark matter must have completely different properties from ordinary matter, otherwise it would have influenced the primordial distribution of light nuclei. Scientists expect to obtain clues about the strange properties of dark matter when the Large Hadron Collider (LHC) at CERN in Geneva goes into operation this year. Still more mysterious is what is called the *dark energy*: observations of exploding stars in distant galaxies indicate that the cosmic expansion is accelerating, while ordinary matter should slow down the expansion rate. These findings are also supported by the observed fine structure in the microwave background. Investigations indicate the existence of a yet unknown substrate.

It may well be that the search for this dark energy could give a decisive clue to a connection between the quantum world of the microcosmos and the largest dimension of the universe dominated by gravity. This could bring the long–lasting search for a theory of quantum gravity back into the state of an observation–based science.

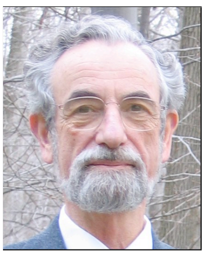
Notes

[1] A German version of this article was published in the Austrian newspaper 'Die Presse' on February 16, 2007.

[2] Lee Smolin, *The Life of the Cosmos*, Oxford University Press USA, 1998.

Doing Physics with Walter

Elliott Lieb



My scientific life has natural dividing lines, like new chapters in a book, the most important of which is the day I started to work with Walter. Walter says we met in 1968, and that is undoubtedly true, but my memory goes back to a turning point in the early seventies when we were sitting around a lunch counter somewhere and Walter asked me if I ever thought about the Dyson-Lenard proof of the stability of matter. No, I hadn't really, but he had, and he realized that, while correct, it needed some new mathematical insight to make it physically understandable as well as mathematically correct.

Walter invited me to be a 'Schrödinger guest professor' at the University of Vienna in the summer of 1974, and while this visit led to lots of fruitful scientific discussion (including collaboration with Heide Narnhofer) nothing dramatic happened except that I lost a key to the Institute. Fortunately, and this must be recorded for posterity, the mainstay of Walter's group, apart, naturally, from his wife Helga, was his assistant Franziska (Franzi) Wagner. She could do everything; not only type up the stuff we generated but also figure out how to deal with a missing, priceless, irreplaceable key that was official government propoerty and must never, under any conditions be lost or duplicated.

But to return to the story, I was a ‘Schrödinger guest professor’ again in the summer of 1975 and this time we solved the problem. The main new idea was the realization that the kinetic energy of electrons (and other particles satisfying Pauli’s exclusion principle) is always greater than the integral over all space of the 5/3 power of the particle density. This, in turn, meant that the old theory of E. Fermi and L.H. Thomas gave a lower bound to the total energy of matter, and this bound was known (by earlier work with Simon) to satisfy the desired stability condition.

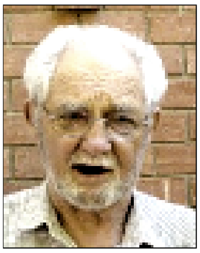
We had several collaborations after that, but our last published work together was the proof in 1986 that the attractive van der Waals force between pairs of atoms or molecules, which drops off like the negative sixth power of the distance between them (in the absence of electromagnetic propagation corrections), was a universal consequence of Schrödinger’s equation.

There were enjoyable visits before and after 1986. I would visit Walter in his hideaway in Zweiersdorf, where Helga was a superb hostess, and he would often visit my wife and me in Princeton. Walter is one of the most organized people I know. The visits followed a rigorous time schedule starting around 6 am and ending around 9 pm that dictated when we would wake up, take walks, play music, exercise and do science. Walter is an accomplished piano and organ player, and composer, while I was a fifth rate recorder (Blockflöte) player. Following an internal alarm clock Walter would announce that it was time to stop the science and play some baroque sonatas, and he would very patiently overlook my missed entrances and other mistakes. It was fun and I learned a lot, but I also realized that I would never be organized enough to do all the many things, in many fields, that Walter is capable of doing.

Walter continues his work in science with the energy of a newcomer. Not only is he interested in everything but he continues to do original research in his inimitable style – which is to do what you think is interesting and important without paying much attention to the fashion of the day. Welcome to the 80’s, Walter, and continue to inspire us youngsters by doing great science!

Encounters with Walter Thirring

Angas Hurst



My first contact with Walter was when I saw a letter in Physical Review in 1950 written by him from Dublin. It was probably his first publication and dealt with ‘Regularization as a Consequence of Higher Order Equations’. This was a neat connection between higher order wave equations and the Pauli-Villars regularisation. This was particularly helpful for me because shortly before I had heard Pauli deliver the Rouse Ball lecture to the mathematics faculty in Cambridge, and it was an example of how not to do things. Only someone with Pauli’s reputation could have got away with it. He introduced a method for controlling divergences in quantum field theory to a general audience of mathematicians who almost certainly knew little about quantum field theory and less about the problems of divergences. His delivery consisted of walking backwards and forwards in front of the audience, with his head sunk on his chest, accompanied by a regular squeak of a loose floorboard in the middle of his to and fro path. As a barely beginning research student I was completely lost. So Walter’s paper was a light in the darkness.

Later on we had a more personal encounter when he wrote to me about the content of my PhD thesis on the convergence of the quantum field theory perturbation expansion. Apparently he had been instructed by Pauli to check it out as it made some rather strong assertions. Not only did he check it out, but he rederived the results by much neater methods and completed the outstanding gap of including renormalisation effects. As a result the quantum field used was called, by Bogoliubov and Shirkov, the Hurst-Thirring field, and so our names have always since then been coupled together, although our paths have diverged widely.

Walter became one of the stalwarts of European physics, whilst I led a somewhat quieter life bringing mathematical physics to Australia, carrying on a tradition started in Adelaide University by William Bragg, and it was not until I went to a Summer Research Institute at Hercegnovi in 1961that we actually met. I went as someone who wanted to catch up after nearly a decade of isolation in Adelaide, and Walter was there as one of the lecturers. It was a very distinguished gathering, both for the standing of the lecturers and for the collection of students, many of whom became world famous. Apart from the lectures, Walter spent most of his time drifting around the lovely harbour using a snorkel, and I brought some order in the Proceedings by correcting much of the distorted European English provided by the lecturers. The final distinction of this meeting was that Walter invited me to come to Vienna, where I gave a seminar on work I had done with Green on the Ising model. It was also a marvellous opportunity to see Vienna, and to enjoy its wonderful music and art galleries.

Since then I have been to Vienna on numerous occasions and Walter and Helga came to Australia in 1987, staying with us in Adelaide for two weeks. During that time we took them to visit the property of a former mathematical physics student, which occupied 200 square miles, and was complete with shearing sheds and kangaroos. As a result they became great lovers of Australian television series.

Two areas in which our interests have overlapped have been in physical systems in which C^*-algebras have been a potent method.

The first was a paper with Heide Narnhofer on the construction of covariant QED without an indefinite metric. This thinking closely parallels work that I did in collaboration with Alan Carey, Janice Gaffney and Hendrik Grundling, which took off from an old paper I wrote in 1960. Our final conclusion was that for gauge field theories, or more generally theories with nonintegrable constraints, a very wide range of representations may be employed so long as the observable part is treated by a regular Hilbert space representation. What one does with the non-observable part does not matter, so indefinite metric or nonseparable representations or other pathologies can be employed without prejudice. For this C^*-algebraic methods are crucial, which Walter and Heide spelt out very thoroughly for their system.

The second area is in a very deep study of the description of entropy in operator algebras, and it touches on a question, which has bothered me for years, which is how to understand the way in which information is parcelled between physical theories and raw data. I am bothered by a remark I once saw Paul Davies makes about complexity of systems, and how theoretical science has been able to succeed because it has been possible to shunt the complexity into the data, whilst still having manageable theories. This seems to be a gift which Nature has made us, and it need not have been so.

I shall continue to admire the depth and diversity of Walter's work, and I am very privileged to have had so valuable a friendship.

Birthday Greetings to Walter Thirring

Ernest M. Henley

I have known Walter Thirring for over 50 years. Walter used to come to the University of Washington during summers, which is the nicest time of year here. We had Theoretical Summer Institutes, organized by a colleague (Boris Jacobsohn) and me, with the help of members of the Physics Department. These Institutes attracted quite a few well known physicists, as well as younger ones, to the University. During one of the summers in the late 1950's, Walter gave a series of lectures on quantum field theory and exactly soluble models in it. The lectures were excellent. Walter conceived the idea of publishing these lectures and asked me whether I would be willing to collaborate. I was eager to do so. After we wrote the lectures up, we asked students to find errors and offered monetary rewards. Still, after the book was published as *Introduction to Quantum Field Theory* by McGraw-Hill we continued to find errors which had eluded both the students and us. I noted all of these errors in my copy. When McGraw-Hill asked to borrow the annotated copy for a translation into Japanese, I agreed, with the proviso that the book would be returned to me. Unfortunately, I never saw the book again, and McGraw-Hill could not locate it! By then, the book was out of print. Since this was before Xerox was popular, I, unfortunately, had not copied the corrections.

The book is only one of many pleasant memories of the Thirrings' visits. We went on many hikes together. I particularly remember one notable one to the top of Mt.Dickerman. The mountain was enveloped in fog, but we thought that it would clear or that the top would be out of the fog. Instead, the fog got worse and worse the higher we went. Fortunately, there is a well known blueberry area about 3/4 of the way up, where we gorged ourselves and had lunch. On top, you could barely see your own feet. Nevertheless, I pointed out all the wonderful peaks that you could see (on a clear day): Glacier Peak, Sloan peak, Big Four, Vesper, and Monte Cristo, among others. It is such a beautiful sight that I was deeply sorry that Walter and Helga had missed out! We never managed to repeat this hike.

We have remained good friends over the years. Whenever I can get to Vienna I do so and always visit the Thirrings.

My best wishes to Walter on his 80th birthday. I wish I could attend the symposium and the celebration.

ESI News

The **ESI Junior Research Fellows Programme** (ESI-JRF) has been extended by a further three-year period (2007 – 2009).

Funded by the Austrian Ministry of Science and Research, the programme provides support for PhD students and young post-docs to participate in the scientific activities of the Institute and to collaborate with its visitors and members of the local scientific community for periods between 2 and 6 months.

During the initial three-year period (2004 – 2006) the ESI-JRF programme established itself as a highly successful component of the Institute's scientific activities. Both the number and quality of the applicants exceeded all expectations, and only 66 out of 282 applications could be funded during the years 2004 – 2006, although about 70 % of the applicants were judged to be of 'supportable' standard.

In 2004, the level of funding of the programme had been set at € 150.000/year. In 2005 funding was increased to € 200.000/year, with the additional € 50.000 earmarked to help increase the percentage of women engaged in mathematical research.

Although we have not been given precise figures yet, the funding level of the programme for 2007 – 2009 is expected to be at least € 200.000/year.

Currently there are 7 post-docs at the ESI, 6 of whom are funded by the Junior Research Fellows programme.

The most recent call for applications ended on April 30, 2007, and attracted 37 applications.

The deadline for the next round of applications for ESI Junior Research Fellowships will be November 10, 2007.

In the **ESI Senior Research Fellows Programme** the following courses will be offered during the Summer Term 2007 and the Winter Term 2007/08.

Summer Term 2007

- Vadim Kaimanovich, International University Bremen: *Boundaries of groups: geometric and probabilistic aspects.*
- Miroslav Englis, Academy of Sciences, Prague: *Analysis on complex symmetric spaces.*
- Thomas Mohaupt, University of Liverpool: *Black holes, supersymmetry and strings* (*Part II*).

Autumn/Winter Term 2007/08

- Christos Likos, Heinrich-Heine University, Düsseldorf: *Theory of soft matter.*
- Radoslav Rashkov, Sofia University: *Dualities between gauge theories and strings.*

For further information concerning the contents of these lecture courses and relevant literature we refer to the web page http://www.esi.ac.at/activities/courses.html.

The European Post-Doctoral Institute (EPDI): The European Post-Doctoral Institute for Mathematical Sciences was founded in October 1995 with the ambition of facilitating the mobility of young scientists within Europe. The Institut des Hautes Études Scientifiques (Paris), the Isaac Newton Institute for Mathematical Sciences (Cambridge) and the Max-Planck-Institut für Mathematik (Bonn) as founding members were later joined by several other institutes, among them the Erwin-Schrödinger Institute. This initiative turned out to be a very successful scientific enterprise which enabled young researchers to pursue their work at the participating institutions. This January, the 12th call came to a conclusion in a meeting of the Scientific Committee at the Mittag-Leffler Institute Stockholm, where the winners of the two-year research fellowships for the period 2007-2009 were nominated. It is worth noting the number of women elected is very high compared to the number within the scientific community.

In January 2007, **Klaus Schmidt** gave a *PIMS 10th Anniversary Distinguished Lecture* with the title *'On some of the differences between $\mathbb{Z}$ and $\mathbb{Z}^2$ in dynamics.'*

In December 2006, **Jakob Yngvason** held the *Erwin Schrödinger Lectures* at the University Colleges in Cork and Limerick and at Trinity College, Dublin, Ireland. This annual lecture series, supported by the Austrian Embassy in Dublin and the National Bank of Austria, commemorates the famous lectures *'What is life'* given by Erwin Schrödinger at Trinity College in 1943.

Jakob Yngvason also gave the *Andrejewski Lectures* at the University of Leipzig in January 2007.

Current and Future Activities of the ESI

Thematic Programmes

Amenability, February 26 – July 31, 2007.

Organizers: A. Erschler, V. Kaimanovich, K. Schmidt

Workshop on amenability beyond groups, February 26 – March 17, 2007

Workshop on algebraic aspects of amenability, June 18 – June 30, 2007

Workshop on geometric and probabilistic aspects of amenability, July 2 – July 14, 2007

Poisson Sigma Models, Lie Algebroids, Deformations and Higher Analogues, August 1 – September 30, 2007

Organizers: H. Bursztyn, H. Grosse, T. Strobl

Applications of the Renormalization group, October 15 – November 25, 2007

Organizers: G. Gentile, H. Grosse, G. Huisken, V. Mastropietro

Workshop on the Renormalization Group Flow and Ricci Flow, October 22 – 26, 2007

Workshop on Renormalization in Dynamical Systems, October 29 – November 3, 2007

Workshop on Renormalization in Quantum Field Theory, Statistical Mechanics and Condensed Matter, November 12 – 17, 2007
The programme will be followed by an **ESF-Workshop on Non-commutative Quantum Field Theory**, November 26 – 30, 2007. Introductory courses to this subject will be given by J. Barrett and R. Szabo in the week November 19 – 23, 2007.

Combinatorics and Statistical Physics, February 1 – June 15, 2008

Organisers: M. Bousquet-Melou, M. Drmota, C. Krattenthaler, B. Nienhuis

Workshop, May 25 – June 7, 2008

Summer School, July 7 – July 18, 2008

Metastability and Rare Events in Complex Systems, February 1 – April 30, 2008

Organizers: P.G. Bolhuis, C. Dellago, E. van den Eijnden

Workshop, February 17 – February 23, 2008

Hyberbolic Dynamical Systems, May 12 – July 5, 2008

Organisers: H. Posch, D. Szasz, L.-S. Young

Workshop, June 15 – June 29, 2008

Operator Algebras and Conformal Field Theory, August 25 – December 15, 2008

Organisers: Y. Kawahigashi, R. Longo, K.-H. Rehren, J. Yngvason

Other Scientific Activities

First European Young Scientists Conference on Quantum Information, August 27 - 31, 2007

Organizers: Simon Gröblacher and Robert Prevedel

Central European joint Programme of Doctoral Studies in Theoretical Physics, September 24 - 28, 2007

Organizer: Helmuth Hüffel

There will be two lecture courses of 15 lectures each:
Harald Grosse: *Noncommutative Quantum Field Theory*
Jakob Yngvason: *Local Quantum Physics*

Fourth Vienna Central European Seminar on Particle Physics and Quantum Field Theory, November 30 – December 2, 2007

Theme: Commutative and Noncommutative Quantum Field theory

Organizer: Helmuth Hüffel

Thirringfest

Tuesday, May 15, 2007

Lecture Hall of the Faculty of Physics, Strudlhofgasse 4, 1090 Vienna

10:00 – 10:15 **Welcome addresses**

GEORG WINCKLER, Rektor, University of Vienna
ANTON ZEILINGER, Dean, Faculty of Physics, University of Vienna
KLAUS SCHMIDT, President, ESI

10:15 – 11:15 **Wolfgang Rindler (Dallas)**: *Vienna, Hans Thirring, and Gravity Probe B*

11:15 – 11:45 Coffee Break

11:45 – 12:45 **Julius Wess (Munich)**: *Deformed Theory of Gravity*

14:30 – 15:30 **Elliott H. Lieb (Princeton)**: *Remarks on Density Functional Theory*

17:30 – 18:30 *Eine kleine Hausmusik: Chamber music composed by Walter Thirring*
Performed by friends at Pfarrsaal, Nussdorf, Pfarrplatz 3, 1190 Wien

19:00 – Heuriger – Maier am Pfarrplatz

Editors: Klaus Schmidt, Joachim Schwermer, Jakob Yngvason

Contributors:

Peter C. Aichelburg: peter.christian.aichelburg@univie.ac.at
Bernhard Baumgartner: bernhard.baumgartner@univie.ac.at
Harald Grosse: harald.grosse@univie.ac.at
Ernest M. Henley: henley@parity.phys.washington.edu
Angas Hurst: ahurst@physics.adelaide.edu.au
Wolfgang Kummer: wkummer@tph.tuwien.ac.at
Elliott Lieb: lieb@math.princeton.edu
Heide Narnhofer: heide.narnhofer@univie.ac.at
Herbert Pietschmann: herbert.pietschmann@univie.ac.at
Harald A. Posch: harald.posch@univie.ac.at
Wolfgang L. Reiter: wolfgang.reiter@univie.ac.at
Walter Thirring: walter.thirring@univie.ac.at
Helmuth Urbantke: helmuth.urbantke@univie.ac.at

ESI Contact List:

Administration

Isabella Miedl: secr@esi.ac.at

Scientific Directors

Joachim Schwermer: joachim.schwermer@univie.ac.at
Jakob Yngvason: jakob.yngvason@univie.ac.at

President

Klaus Schmidt: klaus.schmidt@univie.ac.at

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Redaktion und Sekretariat: Telefon: +43-1-4277-28282, Fax: +43-1-4277-28299, Email: secr@esi.ac.at
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