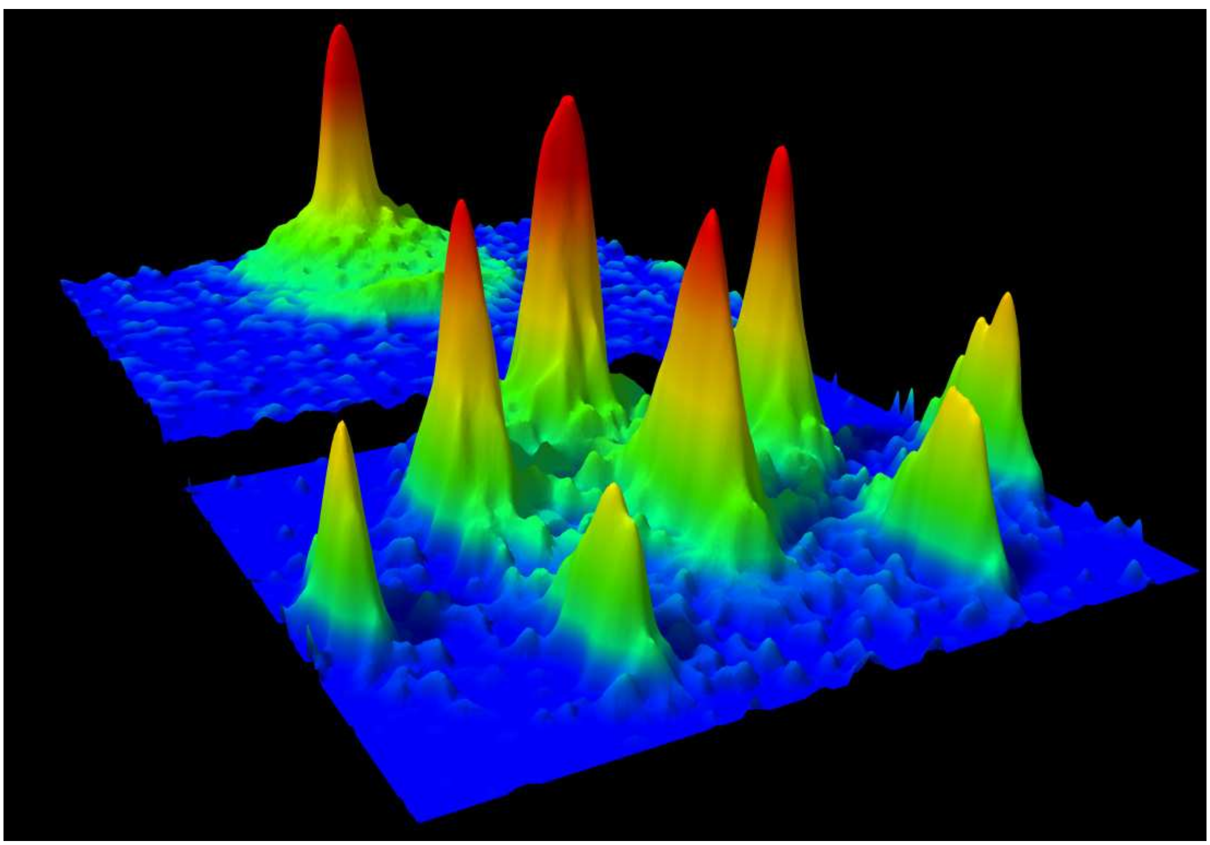
International Association of Mathematical Physics

News Bulletin

July 2010



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Cover photo: Observation of superradiant Rayleigh scattering. An elongated (cigar-shaped) condensate is illuminated with a single off-resonant laser beam. Collective scattering leads to photons scattered predominantly along the axial direction and recoiling atoms at 45°. (W.Ketterle group (1999))

Looking back to the IAMP roots

by Pavel Exner (IAMP President)

Our Association is not old in terms of generations but it is not new either; a third of a century is a solid period of time and our path from the origin in the seventies of the last century has been long and full of events.

It is an old truism that a future of a community is full of uncertainties if it does not remember its past. Keeping records is usually not the first concern in the foundation years but as the time passes you start noticing that it is useful to look back sometimes — be it seeking a reference point in previous decisions, an encouragement in the way we were able to face troubles in the past, or simply a sense of identity of a group of like-minded people.

History comes to us in many different forms. There are great deeds worth chanting in a *Song of Roland* and there are lesser things, down to an archaeology in everyday activity records. A correct historical picture cannot exist without all of these components. This applies not only to big entities but by the same right to smaller ones such as our Association.

In the interview published in the previous issue Professor Araki recalled some stories of the early days, and I hope very much that they would encourage the others who were there at that time to put their memories into writing. There are certainly stories to tell, in particular, because the birth of the IAMP witnessed competing concepts giving the process a certain dramatic quality which would be worth recording.

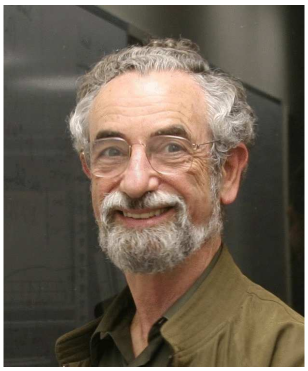
We tried to work recently on the "archeological" end of the Association history by compiling and digitizing a collection of IAMP bulletins from its prehistory in 1976 up to 1997 when, under the Presidency of Elliott Lieb, the *News Bulletins* switched from the mimeographed sheets or booklets to an electronic form. A few weeks ago the collection was posted at our web page, cf.

http://www.iamp.org/page.php?page=page_veryoldbulletin.

The material was dug out from the bottom of personal drawers and does not aspire for completeness — additions from oldtime members are welcome — but its more than nine hundred pages gives you a colorful picture of mathematical-physics activities in the first two decades of the IAMP existence. The documentary value of the old bulletins lies in part in the fact that they collect information which since the nineties we learned to store and retrieve electronically, on preprints, books, conferences, etc. Looking through the pages you will appreciate the amount of the work done by our community in a generation. Enjoy browsing the old pages.

Some early history of the mathematical physics of the Bose gas

by ELLIOTT H. LIEB (Princeton, USA)



One of the founder of the contemporary Mathematical Physics, Elliott H. Lieb has been a professor at Princeton since 1975, following a leave from his professorship at MIT. He has been awarded several prizes in mathematics and physics, including the Henri Poincaré Prize of the IAMP (2003) for his life achievements in quantum mechanics, statistical mechanics and analysis. Professor Lieb has twice served (1982-1984 and 1997-1999) as the President of the International Association of Mathematical Physics.

Valentin Zagrebnov, the current editor of the IAMP News Bulletin, has raised its level in a major way by including survey articles of general interest to the members of IAMP. One topic he thought should be reviewed is the history of the mathematical physics of the Bose gas, a subject to which he has also contributed.

In response to his request I decided that while several people could do a better job than I can about relating developments after 1998, as well as summarizing the current status of the field, one thing I could contribute was to relate some of the long ago events in which I participated. The year 1998 is significant for me because it is the year in which Jakob Yngvason and I solved a problem that arose four decades earlier, and which gave rise to the work by several authors on the ground states of low density Bose gases with pairwise interactions. (Some of this work is summarized in a book†, and by Robert Seiringer in this News Letter.) This largely personal history could be useful to young people today who might be discouraged about the possibility of achieving apparently unreachable goals, such as a proof of Bose-Einstein condensation (BEC). Some of the theorems we now take for granted seemed, at one time, also to be unreachable.

My interest in the subject started in 1958 when I went to Cornell University to be a postdoc with Hans Bethe. In 1957 T-D. Lee, K. Huang and C-N. Yang had found the ground state energy and elementary excitations of the dilute, homogeneous gas of N interacting bosons. The leading term in the ground state energy was $E = \frac{\hbar^2}{2m} 4\pi N\rho a$, where a is the scattering length of the 2-body, short-range interaction potential and ρ is the particle density. This formula was already understood by N.N. Bogolubov in his famous 1947 paper, and even earlier by W. Lenz in 1935. LHY found several ways to get this formula; the most important from the point of view of modern physicists was the use of the pseudopotential, invented by Fermi to understand nuclear forces. It replaces a hard core interaction of radius a, for example, by a delta-function of strength a. Every mathematical physicist knows that a simple delta-function in 3D does nothing (as a potential in a Schrödinger equation), but it does work effectively and efficiently in perturbation theory to imitate the effect of the hard core.

Despite the seeming correctness of this formula for the energy, a rigorous proof was lacking. A low density Bose gas is, in several ways, more difficult to treat rigorously than a Fermi gas. Dyson tried to find a rigorous proof and succeeded in constructing a correct upper bound, which was far from as trivial as this might sound, but only managed to get a lower bound that was 14 times too small.

LHY were also able to 'derive' the next term in the Bogolubov expansion of the energy, $\frac{\hbar^2}{2m}\frac{512}{15}N\rho a\sqrt{\pi\rho a^3}$, which measures correlations. It shows that such a dilute gas has, surprisingly, 3 length scales, $a \ll \rho^{-1/3} \ll 1/\sqrt{\rho a}$. Fermi gases, in contrast, are more intuitive and do not have the third scale, whose presence makes it impossible to think of the bosons as particles localized with respect to each other. To this day, this second term has not been fully proved, although significant progress has been made recently by A. Giuliani and R. Seiringer.

Bethe posed the problem to me of independently verifying these formulas, especially the $4\pi N\rho a$, without using the unconventional mathematics that led, nevertheless, to a formula that everyone believed to be true. This says a lot for Bethe because while he was a physicist's physicist, and perhaps the best calculator in the field, he was nevertheless interested in knowing whether there was a solid mathematical foundation for the understanding of basic physical phenomena. Not very many physicists then or now have this point of view.

Why consider a hard core? Because at that time, and for many years afterward, the only interesting Bose fluid known was liquid helium, where the density is high and the atomic interaction is mostly hard core. Naturally, the low density theory could not be expected to be accurate for liquid helium, but that didn't bother most people. In a famous 1956 paper O. Penrose and L. Onsager tried to estimate the condensate fraction for helium and arrived at 10%, but while they never claimed that this figure was anything more than a guess, it was widely accepted at the time as the truth. Such was the state of physics in those days.

Although I was not able to prove the $4\pi N\rho a$ formula rigorouly, I did manage to 'rederive' it in 1963 by a different argument that operates in real space rather than in momentum space as the Bogolubov, LHY and other derivations did. This, nevertheless, suggested that real space was the more useful perspective, and this was born out when Jakob Yngvason and I finally proved it in 1998. The key was to find a useful way to 'localize' the many body-wave functions.

Another bosonic problem that eventually succumbed to real-space localization was the ground state energy for *long*-range Coulomb potentials. In 1961 L. Foldy used Bogolubov's method to find the leading term in the ground state energy for high density 'jellium' (positive particles in a *fixed*, negative background), and Dyson showed rigorously in 1967 that a 2-component mixture of positive and negative bosons would violate 'stability of matter' and have an energy $-CN^{7/5}$. In 1988 J. Conlon, H-T. Yau and I managed, finally, to verify both results, but with non-sharp constants, and finally, J. P. Solovej and I managed to prove the conjectured sharp constants in 2001 and 2004, respectively. The new feature here was a kind of continuous localization called ‘sliding’. The point, again, is that the buildup of ideas over a long period can, despite initial skepticism, eventually lead to rigorous solutions of problems.

In this early period I was also involved with the one-dimensional Bose gas with repulsive delta-function pairwise interaction. That the ground state energy and low-lying excitations could be calculated using an ansatz that goes back to Bethe’s early days in 1931 was something I realized while trying to teach applied mathematics in the then peaceful Sierra Leone of 1961. In 1960, M. Girardeau had shown that the spectrum of the infinite delta-function (hard-core) gas was the same as that of the fermi gas. I was employed by IBM at the time and W. Liniger, a numerical analyst in the Yorktown lab, and I calculated some of the properties of this gas. No one, at the time, even remotely thought that a 1D gas would ever be seen, but it is now something verifiable in the laboratory. Indeed, almost all ‘real’ physicists at the time thought that one-dimensional models were a waste of time but, as it turned out, they were very wrong.

The exercise was meant to check Bogolubov’s theory in 1D; that theory worked very well, except for the fact that the model showed a second, unforeseen branch of the spectrum, which can now be seen experimentally! If there is a moral to this story it is that exactly soluble models and exact calculations can eventually be useful, and I would encourage more young mathematical physicists to think in this direction. It is, however, not enough to solve a model exactly; for the result to be useful one also has to dig out the physical consequences of the mathematical solution. Unfortunately, this second step is not always carried out.

Much remains to be done, such as a proof of Bose-Einstein condensation for the homogeneous gas in the thermodynamic limit (although this was shown by Seiringer and me for traps, in a different limit), and more insight into the time evolution of these gases, as started by L. Erdős, B. Schlein and H-T. Yau. It is time to end this thumbnail personal sketch, however. A proper historical account would include the work in this period of many other mathematical physicists, which has been important for our understanding of these highly quantum-mechanical objects. Despite what one might have thought naively, many of the properties of Bose gases and liquids near their ground states derive from quantum-mechanical dynamics that has no classical analogue. This is much more so than for fermions, and therein lies their fascination.

† E.H. Lieb, R. Seiringer, J.P. Solovej and J. Yngvason, *The Mathematics of the Bose Gas and its Condensation*, vol. **34**, Oberwolfach Seminars Series, Birkhäuser (2005).

Bose-Einstein condensation in cold atomic gases

by Robert Seiringer (Princeton, USA)

Robert Seiringer obtained his Ph.D. in 2000 from the Institute for Theoretical Physics, University of Vienna under supervision of Jakob Yngvason. Since 2003 he is Assistant Professor at Princeton University, Department of Physics. Robert Seiringer has been awarded the Henri Poincaré Prize of the IAMP (2009) for his major contributions to the mathematical analysis of low temperature condensed matter systems, in particular for his work on Bose condensation and the Gross-Pitaevskii equation.

Bose-Einstein condensation (BEC) in cold atomic gases was first achieved experimentally in 1995. [1, 2] After initial failed attempts with spin-polarized atomic hydrogen, the first successful demonstrations of this phenomenon used gases of rubidium and sodium atoms, respectively. Since then there has been a surge of activity in this field, with ingenious experiments putting forth more and more astonishing results about the behavior of matter at very cold temperatures. BEC has now been achieved by more than a dozen different research groups working with gases of different types of atoms. Literally thousands of scientific articles, concerning both theory and experiment, have been published in recent years.

The theoretical investigation of BEC goes back much further, and even predates the modern formulation of quantum mechanics. It was investigated in two papers by Einstein [4] in 1924 and 1925, respectively, following up on a work by Bose [5] on the derivation of Planck's radiation law. Einstein's result, in its modern formulation, can be found in any textbook on quantum statistical mechanics, and was concerned with ideal, i.e., non-interacting gases.

The understanding of BEC in the presence of interparticle interactions poses a formidable challenge to mathematical physics. Some progress was achieved in the last ten years or so, and the purpose of this letter is to briefly explain what has been achieved and how it is connected to the actual experiments on cold gases.

Much of the recent work in mathematical physics on dilute Bose gases takes as input a beautiful paper by Lieb and Yngvason from 1998. [6] They present an elegant and concise proof of a formula for the ground state energy that was conjectured to be true many decades earlier, as explained by Elliott Lieb in the preceding article. It says that for a gas with repulsive interactions at low density ρ, the leading term in the ground state energy per particle equals

$$4\pi a\rho \tag{1}$$

(in units where $\hbar = 2m = 1$, with m the mass of the particles), where a denotes the scattering length of the interaction potential. The method of proof, based on an idea of

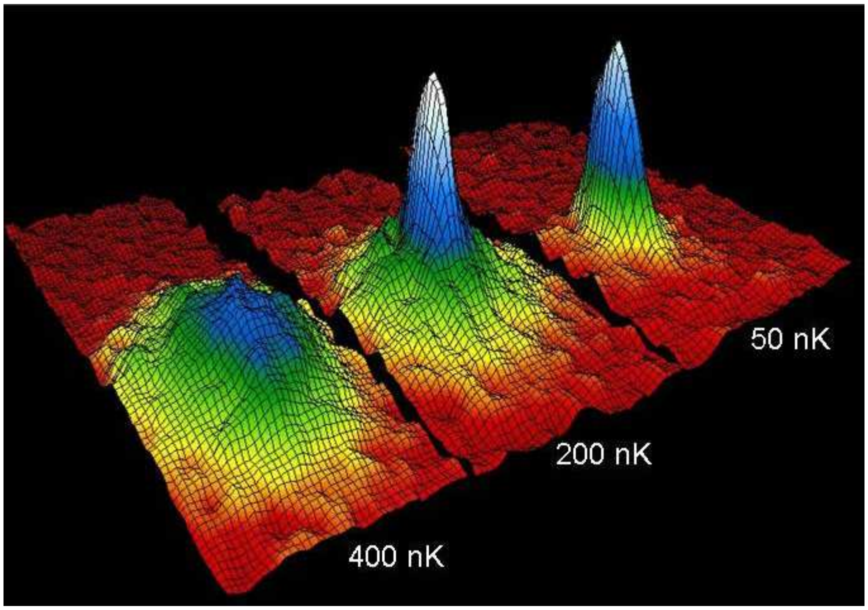


Figure 1: Measured momentum distribution of a dilute gas of rubidium atoms at various temperatures. [3]

Dyson [7] to soften the interaction potential at the expense of part of the kinetic energy, paved the way for much of the subsequent rigorous work on dilute Bose gases.

The formula (1) applies to homogeneous gases. Actual experiments are done with inhomogeneous gases in traps, and hence the natural next step would be to generalize this formula to the inhomogeneous case. The resulting expression is known as the Gross-Pitaevskii (GP) functional, given by

$$\int_{\mathbb{R}^3} \left(|\nabla \phi(x)|^2 + V(x)|\phi(x)|^2 + 4\pi a |\phi(x)|^4 \right) dx \tag{2}$$

with $|\phi(x)|^2$ being the density of the gas at the point x, and $V(x)$ denoting the trap potential. One arrives at this expression in a straightforward way assuming that (1) is valid locally even for inhomogeneous systems. The additional gradient term in the GP functional assures the validity even in the absence of interactions, i.e., when $a = 0$.

That (2) correctly describes the ground state energy of inhomogeneous, dilute Bose gases with repulsive interactions I was able to show with Lieb and Yngvason in 2000. [8] But what about Bose-Einstein condensation? The minimizer of the GP functional is not only expected to describe the particle density of the gas, but should actually be the condensate wave function. The latter is defined as the eigenfunction corresponding to the largest eigenvalue of the reduced one-particle density matrix. In fact, the criterion for the existence of BEC is exactly that there is such a macroscopically large eigenvalue.

The existence of BEC and the validity of the GP minimizer as the condensate wave function was proved in a joint work with Lieb in 2002. [9] Like the Lieb/Yngvason paper mentioned above, it is also rather short and uses as key ingredient a novel type of Poincaré inequality, where a small set is removed from the domain and bounds only in terms of the volume of this set are sought. This work represents the first rigorous proof of the existence of BEC in a continuous system with genuine interparticle interactions.

Shortly after the publication of our work on the ground state energy of inhomogeneous Bose gases, we received a letter from Lev Pitaevskii inquiring whether we expect our result to hold also for rotating systems, and whether our proof might generalize to this case. Rotating condensates have actually been produced in the lab, and beautiful snapshots showing the appearance of quantized vortices have been taken.

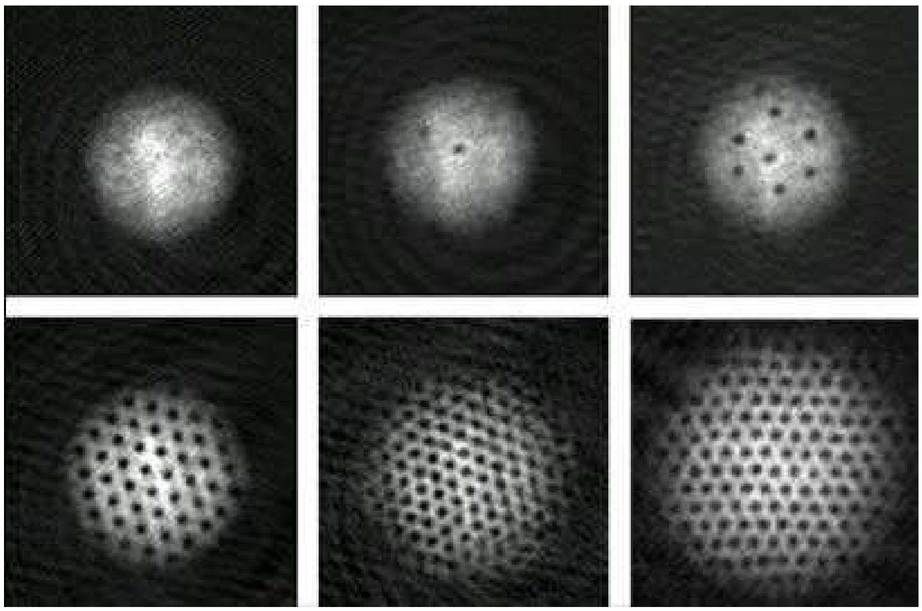


Figure 2: Measured particle density of a rotating Bose condensate at various angular velocities. The black spots represent vortices. [10]

The mathematical analysis of the rotating case turns out to be much more involved. The Hamiltonian for the system is now not real anymore, and the resulting absence of a Perron-Frobenius argument leads to various difficulties. In particular, it turns out that the permutation symmetry of the wave functions (as appropriate for bosons) had to be enforced explicitly, the absolute ground state of the Hamiltonian, without symmetry restrictions, is not bosonic for rotating systems, in general. For non-rotating systems there is no such problem. It took quite a number of years to overcome these difficulties, and the correctness of the GP description for rotating gases was finally proved in 2006, again in joint work with Lieb. [11] An essential ingredient in this work was the use of coherent states as a way of deriving a classical limit of a quantum field theory.

Rotating Bose condensates display a rich variety of phenomena, depending on the angular velocity and the trap geometry. Giant vortices, regular vortex lattices, and a bosonic analog of the fractional quantum Hall effect are merely examples of effects that can occur. Although a lot of progress was made in recent years in order to understand these phenomena from first principles, there are still many open questions left for mathematical physicists to explore.

The GP description of dilute Bose gases is not restricted to equilibrium situations. In fact, the time evolution of a Bose condensate is governed by the (time-dependent) GP equation

$$i\partial_t \phi(x,t) = -\Delta\phi(x,t) + V(x)\phi(x,t) + 8\pi a|\phi(x,t)|^2\phi(x,t) \tag{3}$$

That this is so is not at all obvious. A proof of this fact is actually rather involved, and was achieved in a series of impressive papers by Erdős, Schlein and Yau published between 2006 and 2009. [12] They show that if initially the gas is condensed in a suitable sense, it will stay condensed at later times, and the condensate wave functions evolves in time according to (3). The importance of this result lies in the fact that many experiments on Bose condensation are of destructive nature and take place only after the condensate has been allowed to expand freely for some time. That the time evolution is indeed governed by (3) ensures that properties of the gas in equilibrium in the trap can be deduced from the resulting data.

It should be mentioned that there is a huge mathematical literature on non-linear Schrödinger equations of the type (3), and a lot is known about properties of their solutions. It is remarkable that these results are of direct relevance to the physics of quantum many-body systems and dilute Bose gases, in particular.

So what are Bose condensates good for? Nowadays they serve as a playground for the study of various systems of relevance in condensed matter physics. Bose condensates are now routinely being loaded onto optical lattices, which are created by interfering laser beams. Such systems are quite accurately described by the Bose-Hubbard model, a tight-binding lattice model, which is the bosonic analog to the Hubbard model for fermions. It has a rich and, from the mathematical point of view, rather unexplored phase diagram, with Mott insulator, superfluid or even supersolid phases. The Bose-Hubbard model thus represents a particularly worthwhile and possibly fruitful field for mathematical physicists to explore.

Aside from being subjected to optical lattices, Bose condensates can be squeezed into elongated traps to mimic low-dimensional systems, and manipulated in various other ways to explore the fundamentally quantum-mechanical behavior of many-body systems at low temperature. They are being used to create atomic lasers and are considered for a possible realization of a quantum computer, for instance. The physics of cold gases can thus be expected to continue to produce interesting new ideas and results about fundamental aspects of nature, and to further challenge the mathematical physics community to derive the necessary tools for a comprehensive understanding of these phenomena.

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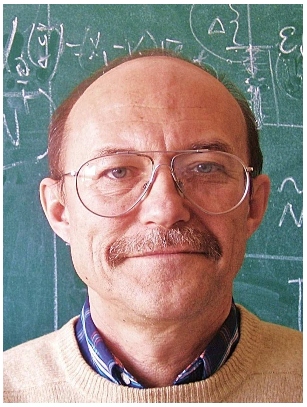
From condensate to “quasi-condensate”

by Valentin A. Zagrebnov (Marseille, France)



The Free Bose-gas. The first mathematical attempt to understand Einstein’s arguments (1925) in favour of boson condensation was due to Uhlenbeck (1927), who “demonstrated” that these arguments are inconsistent. This discouraged any interest in this subject for about 10 years !
It was only when Kramers (1937) pointed out the importance of the thermodynamic limit for these arguments and F. London (1938) established the concept of the macroscopic occupation of the ground-state, that Uhlenbeck (1938) withdrew his criticism. In the paper with Kahn (1938) he developed an explanation of the conventional ground-state Bose-Einstein condensation (BEC) for the free Bose-gas, which is nowadays accessible to the average undergraduate student.

Much later Araki-Woods (1964), then Pulé (1972) and Lewis-Pulé (1974), developed a beautiful mathematical theory of the free-boson infinite-volume equilibrium states and of the corresponding representations of the canonical commutation relations: first, without BEC, then in the presence of the condensate.
Generalised condensation. This concept, which considers BEC as the occupation of the lower energy states instead of the ground state, was invented by Girardeau (1960) to treat BEC in one-dimensional Bose-gas. This definition of BEC has the advantage that it is thermodynamically stable, in contrast to BEC in the ground state. It was revived in a remark by Casimir (1967) about a strange feature of the free gas BEC, when the thermodynamic limit is taken via a sequence of highly anisotropic prisms. Take $3D$ prisms $\Lambda = L_1 \times L_2 \times L_3$ of the volume V with sides of length $L_j = V^{\alpha_j}$, $j = 1, 2, 3$, such that $\alpha_1 \geq \alpha_2 \geq \alpha_3 > 0$ and $\alpha_1 + \alpha_2 + \alpha_3 = 1$, with, e.g., periodic boundary conditions for the single-particle Hamiltonian. Then for the Casimir prisms with $\alpha_1 = 1/2$, the density of the free gas condensate is expressed by unusual formula (condensation of the type II):

$$\rho - \rho_c(\beta) = \beta^{-1} \sum_{n_1 \in \mathbb{Z}} \frac{1}{(2\pi n_1)^2/2 + A} \ ,$$

indicating that it is spread over an *infinite* number of “fragments” sitting in the lowest modes, including the condensation in the ground-state $n_1 = 0$. Here $\rho_c(\beta)$ is the usual free gas critical density for temperature β^{-1} (for the other units we follow the same convention as in the article by Robert Seiringer) and $A = A(\beta, \rho) \geq 0$. For $\alpha_1 < 1/2$ only the *zero-mode* survives in this formula and one recovers the conventional ground-state (type I) BEC, although for $\alpha_1 > 1/2$ *none* of the levels are *macroscopically* occupied and the condensate density is expressed by integral (*non-extensive* or the type III condensation). The complete mathematical theory of the generalised (or “fragmented”) condensate, also called a “quasi-condensate”, and the above classification are due to van den Berg-Lewis-Pulé (1984).

Mean-field models. The importance of this generalisation was demonstrated for the first time by a rigorous analysis of various mean-field models. The first mathematical treatment of the mean-field Bose-gas was given by Davies (1974). The Dublin school (Lewis et al (1983-93)) used the theory of Large Deviations to treat “diagonal” generalizations of this model including the *Huang-Yang-Luttinger* model. They also gave a rigorous treatment of the *Yang-Yang* model by using the same methods, Dorlas et al (1984). These models were later generalised to the full diagonal model and to include non-diagonal BCS-type terms, Pulé et al (2007). The notion of the generalised condensate is indispensable for this analysis.

The second critical point. It was van den Berg (1983), who first proved that the “quasi-two” dimensional free Bose-gas in the exponentially anisotropic prisms $\Lambda = Le^{\alpha L} \times Le^{\alpha L} \times L$, $\alpha > 0$ (*slabs*), manifests a *second* critical density ρ_m with $\rho_m - \rho_c = K\,\alpha > 0$, or a corresponding *second* critical temperature $T_m < T_c$.

It is curious to note that there is *no* second critical point in the “quasi-one” dimensional exponentially anisotropic prisms, but it does exist for the ideal Bose-gas in the exponentially anisotropic “cigar”-type harmonic traps, Beau-Zagrebnov (2010).

So what happens in this “quasi-one” dimensional Bose-gas? Just below T_c a generalized type III condensate (“quasi-condensate”) appears with density growing up to the point T_m. For lower temperatures $T < T_m$ the condensation starts at ground-state (type I condensate) and growing monotonically it absorbs “quasi-condensate”, which in turn starts to decrease. This *coexistence* of two types of condensate for $T < T_m$ lasts up to the zero temperature, when there remains only the conventional ground-state condensate (type I), see Figure 3. The most exciting point in this story is that the second critical point is not

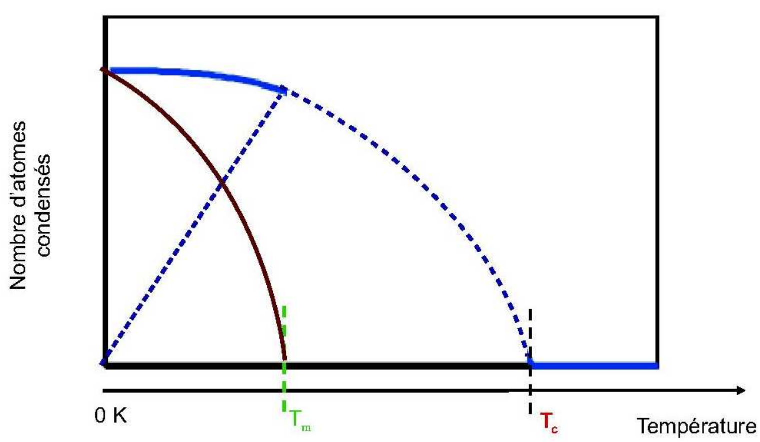


Figure 3: Dotted line is density of the type III “quasi-condensate”. The brown line indicates the type I condensate, and the blue one is a totality of two coexisting condensates below the second critical point.

a mathematical artifact, but it was apparently observed for the first time in experiments with cold atomic gases in extremely elongated ("quasi-one" dimensional) traps by the Aspect group (2003).

The condensate in a random environment. This problem was first studied by Kac and Luttinger (1973), who looked at the perfect Bose-gas embedded into a homogeneous non-negative random potential (repulsive impurities). The first question concerns the definition of the condensate, the second is its dependence on the parameters of the random potential.

The only available system for experiments at that time was a mixture of liquid ^4He with (unquenched) impurities of ^3He. Notice that it was in the liquid ^4He that the Bose-Einstein condensation was observed experimentally for the first time ! Two groups: Dubna-Obninsk (1975) and Chalk River (1976), discovered that at the temperature $T = 1,2K$ about 10% of ^4He atoms have momentums close to *zero*. It is instructive to compare these data with Onsager-Penrose calculations quoted in the article by Elliott Lieb. Moreover, these experiments clearly indicated that BEC and *superfluidity* started at the *same* temperature $T_\lambda \simeq 2,17K$.

The temperature T_λ is in turn a *decreasing* function of the ^3He-impurities density. Since the theory of this fully interacting system cannot so far be treated by the methods of mathematical physics, the main directions of research are concentrated either around the perfect bosons in random potentials (quenched impurities), or some interacting lattice systems, e.g., the Bose-Hubbard model of boson glasses, Fisher et al (1989) and exactly soluble models, Dorlas et al (2006).

For the first class of models, Kac and Luttinger predicted (1973) a striking result opposite to that for the ^4He-^3He mixture. Their calculations show that randomness *enhances* condensation and makes it possible even for one-dimensional systems. The rigorous proof of the Kac-Luttinger conjecture including a detailed analysis of the localized versus extended condensation became available only recently, Lenoble et al (2004) and Jaeck et al (2009), due to the concept of generalised condensation à la van den Berg-Lewis-Pulé. For example, if a free Bose-gas manifests type I (zero-mode) BEC, then the presence of a non-negative random potential transforms the condensation into type III.

This last result challenges the rigorous status of a very popular application in the physics literature of the Bogoliubov approximation to random boson systems. This is mainly an open problem, although it has been proved that localised condensation implies a generalised type III condensation in the one-particle kinetic-energy states, Pulé et al (2009). The latter opens a way to apply the recent proof of the Bogoliubov approximation by Lieb et al (2005) to the case of generalised condensation, Jaeck et al (2010).

In conclusion I would like to quote Elliott Lieb: "*Much remains to be done ...* " and the book by André Verbeure [1]. This book gives more details concerning generalised BEC and summarizes some recent results, which are not mentioned above.

[1] "Many-Body Boson systems", Springer-Verlag, Berlin (2010). In particular, readers will find there some recent mathematical results concerning the Raman and Rayleigh super-radiant scattering on condensed bosons in elongated cigar-type traps. See figure on the Bulletin covered page.

My way to quantum physics

What continues to fascinate me in Mathematical Physics?

by Simone Warzel (Munich, Germany)

I was asked to write a few words about what attracted me to my field of research and Mathematical Physics in general. Since I was educated as a physicist, the original motivation to move into Mathematical Physics stems from my interest in models in theoretical physics. However, all too often in my physics education, I was left with a hand-waving explanation or a mere hope that these models indeed carry the proposed physical implications. As a student, I was then thrilled when being introduced to works which combined physical intuition with mathematical rigor to help a more complete understanding emerge. That's what made me turn to Mathematical Physics.

What I also find attractive about Mathematical Physics is the fact that one is not bound to a particular discipline in either Mathematics or Physics. This is true even if one sets one's eyes on a particular problem. Let me illustrate this, using my main field of interest: random operators.

Here from the mathematician's point of view, techniques from analysis and probability or rather ergodic theory get combined for an explanation of the spectral properties of certain self-adjoint operators with random coefficients. The most basic example of such an operator is the Anderson Hamiltonian. It was proposed in 1958 as a basic model in the quest for a theory of quantum transport in disordered media. Among its interesting features is a conjectured sharp transition of the eigenstates from being localized in one energy/disorder regime to contributing to diffusive transport in the other regime. Quite in the spirit of phase transitions in statistical mechanics, this transition is expected to occur only for dimensions larger than a critical one, here: $d = 2$. So far, only the localization regime has been understood to quite some degree: i) complete localization for $d = 1$ at any non-zero level of disorder has been established already in 1970's using transfer matrix methods; ii) the analogous statement for $d = 2$ remains an open problem; iii) localization in general can be established for large disorder and extreme energies; iv) the fact that the phenomenon is stable to short-range interactions in the associated multi-particle set-up has also recently been settled. What is missing are techniques for the explanation of the diffusive regime or the analysis of localization in the truly many-particle setup. The latter remains an open problem even on the level of rigor of theoretical physics.

To come back to my original point of interconnectedness of areas in mathematical physics, it is worth noting that the available proofs of localization rest on techniques, such as the multi-scale analysis or the fractional-moment method, which either originated or have proven useful in analyzing models of statistical mechanics. The connection to statistical mechanics becomes even more immediate in a toy model for the Anderson

Hamiltonian, namely the supersymmetric sigma-model. Apart from this link, the field of random operators is also intertwined with random matrix theory, which enjoys a happy life of its own. One conjecture here is the relation between the statistics of the eigenvalues of Anderson-like Hamiltonians on large boxes and the nature of the spectrum of the infinite-volume operator. In the localization regime, the process of random eigenvalues, as seen under a natural magnification, is known to be Poisson. In the delocalization regime, eigenvalue repulsion kicks in and one conjectures random matrix statistics.

I am very honored to share the young scientist prize of IUPAP with Rupert Frank and Benjamin Schlein. With the former I even enjoyed collaborations. I would also like to take the opportunity to thank all my other collaborators from which I have learned a lot and which made Mathematical Physics even more fun.

An interview with Dr. Akira Tonomura

Dr. Akira Tonomura

Dr. Akira Tonomura is a Fellow of Hitachi Ltd. He is a pioneer and an authority in the field of electron holography. In particular, he is famous for the double-slit experiment with electrons for the demonstration of particle-wave duality, the experiment for the verification of the Aharonov-Bohm effect, and the experiment for the observations of magnetic vortex movement in superconductors. He plays important roles of scientific administration in Japan as a member of the Japan Academy and the Science Council of Japan. He also serves currently as visiting professor at some universities. He was awarded the 1999 Benjamin Franklin Medal in Physics for his contributions to the development of an electron beam and high-resolution microscopic device as well as the Nishina Memorial Prize, the Asahi Prize, Japan Academy Prize, Imperial Prize, etc. in Japan. His works were also duly recognized abroad: a Foreign Associate of the National Academy of Sciences in the US, a Fellow of the American Association for the Advancement of Science, etc.

Bulletin: *The Aharonov-Bohm (AB) effect has been fascinating both mathematicians and physicists with every interesting aspect of the effect. But, in those days when you started several experiments on the AB effect, the effect had not yet been recognized even by many authorities of theoretical physics, or rather, many of them thought that it was a mere armchair phenomenon in mathematics, not real physics. So, there was a possibility that it might be a pipe dream in mathematics. How did you come to tackle the experimental demonstration of such an effect in the institute of Hitachi, a Japanese company?*

Tonomura: Vector potentials had been considered to be merely a mathematical convenience without physical reality since Maxwell added it to electromagnetic equations. The Maxwell equation appearing in his textbook[2] primarily contains the vector potential. Since the AB effect is due to the vector potential, as you said, it was not recognized. In fact, the physicists who denied the existence of the AB effect made themselves heard, which has given rise to much controversy. The early experiments for its verification by physicists, which is represented by R. G. Chambers, were performed shortly after Aharonov and Bohm's paper was published. However, almost all protestors against the AB effect tried to outdebate them, insisting that the phenomena observed by the experimenters were not caused by the AB effect. That is, they claimed that there was a leakage of magnetic field from both ends, the north and south poles, of their solenoids with finite size, and thus that the electron is acted in by the magnetic field leaking out. Thus, the problem of the existence of the AB effect, that is, its reality, was still at the center of a controversy.

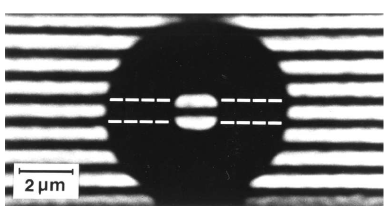
We had developed an electron beam with good coherence and completed the new technology of an electron holography by that time. I wanted to settle the controversy by presenting reliable results with our technology, because I thought that the AB effect is a mathematical yet important evidence for the existence of a gauge field. I wrote a letter and sent it to Prof. C. N. Yang. In the letter I explained to him that we were planning a perfect experiment for the verification of the AB effect, and I asked him whether it is really important for physics. In the early of June 1981, Prof. Yang called me from the University of Tokyo. He stayed with Prof. H. Miyazawa. He said that he would like

[2] A Treatise on Electricity and Magnetism (1873).

to visit the Hitachi Central Research Laboratory (HCRL) to discuss the experiment we planned. Actually, in that period many physicists were skeptical about the AB effect, and, moreover, only a few Japanese physicists knew the AB effect in those days. So, many people in Hitachi said, "Is it useful for Hitachi's development?" "Is such an experiment worth much in physics?" Thus, I had to convince people that it is worthwhile to tackle the problem even in Hitachi. Prof. Yang's visit to HCRL and his authorization of our attempt helped us promote it and get up people's drive in Hitachi. Our project was approved by Hitachi after all, and it started. We were involved in that controversy for about six years.

Bulletin: *So, Prof. Yang was your go-to person for the project in Hitachi, right?*

Tonomura: Yes. I have to mention that he also gave us several academically important advices. In the previous experiments the leakage of magnetic field comes from both ends, the north and south poles, of the solenoid. To avoid such a leakage, we reached the idea that we should connect both ends, namely, replace the solenoid with a toroidal magnet (i.e., donut-shaped magnet). It required of us the cutting-edge technology of microfabrication. So, I talked to some people with the microfabrication technology in Hitachi over to our project. They kindly accepted to take on the extensive work after several negotiations. Although we had suffered many setbacks, we finally succeeded in creating a leak-free, toroidal magnet. Thus, we could observe the AB effect using electron holography (Phys. Rev. Lett. **48** 1443 (1982)). But some physicists made a pointed criticism: That is, although there is no end of the toroidal magnet, the electron beam may still touch its magnetic field. So we required a more perfect experiment to implement the idealized situation of the AB effect. Prof. Yang gave us a comment on what we should do in the 1st "International Symposium on Foundations of Quantum Mechanics in the Light of New Technology (ISQM)." Hitachi together with the Physical Society of Japan and the Japan Society of Applied Physics have periodically organized ISQM. Prof. Yang advised us that we should cover the donut-shaped magnet with a superconductor, and he predicted that we must observe the quantization of flux, a fundamental phenomenon in the theory of superconducting, in the AB effect. Namely, the magnetic field is perfectly confined in the toroidal magnet because of the Meissner effect. If we observe the flux quantization as an interference shift, we can conclude that the shift is due to the vector potential. That is the very effect theoretically predicted by Aharonov and Bohm. We, moreover, covered it with copper so that the electron cannot enter it. It was very difficult to create a sample for the experiment that satisfied all of our desired conditions. We had to create about a hundred thousand samples. Eventually, we succeeded in creating such samples and observing the AB effect under perfect conditions (Phys. Rev. Lett. **56** 792 (1986)).



The experimental demonstration of the AB effect

I think the ruling passion of all physicists involved in the project for the AB effect won the hearts and minds of the people in Hitachi, which took our project to great success. Also, I owe it to Prof. Yang that we could continue fundamental research on physics in Hitachi.

Bulletin: *The AB effect is a subject of interest to both mathematicians and physicists in the light of fundamental theory. It is also emerging as a principle for several devices of nanotechnology. How do you feel about this situation?*

Tonomura: The AB effect tells us about foundational part of physics such as gauge theory. I think mathematics and physics are quite involved with each other at the foundation of physics, and, moreover, possible principles for devices show up frequently in such a fundamental phenomenon. In the 1st ISQM the following question came up: Does the electron in a solid exhibit any interference phenomenon? Then, does the AB effect take place in the solid? Y. Imry *et al.*, the IBM researchers, were at the center of a heated argument in that ISQM. R. A. Web , also an IBM researcher, had experimentally demonstrated a positive answer shortly after the argument, and he talked about it at the 2nd ISQM. Some results on the AB effect in carbon-nanotubes were reported there, too. Their results say that the AB effect can become a principle of a possible device of nanotechnology and expand the possibility of such a device.

Bulletin: *Yes. I agree with you. As a current topic, for instance, I take an interest in the result about the absence of the AB effect in the ring which consists of the two Bose-Einstein condensate Y-junctions. I wonder whether there is an interaction between the AB phase and the phase coming from a boundary condition.*

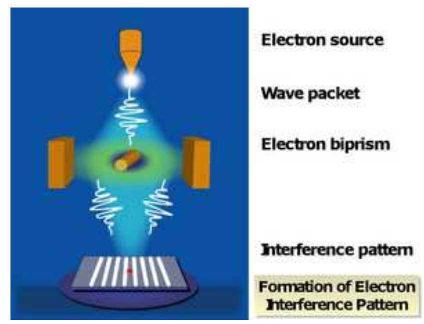
Tonomura: It is interesting if the conversation of mathematics and physics leads to the development of new devices. In that case we may have to seek out someone like Prof. Yang. He can understand both mathematics and physics well, and is good at explaining the essence of physics to a mathematician, for instance to you, as well as to physicists. If we were C. N. Yang, we could play such a role. But, unfortunately, we are not. It is difficult to expect more men of his timber. Thus, it is important that we, physicists and mathematicians, often have heated discussions on common subjects that we are interested in.

Bulletin: *I remember that Prof. Yang told us that he learned some mathematics from Prof. K. Kodaira while three Asians, Prof. Kodaira, Prof. S. Tomonaga, and Prof. Yang, stayed in the Institute for Advanced Study, Princeton. He said that they always enjoy many conversations and discussions there. By the way, besides the demonstration of the AB effect, your experiment on it also shows that the unit of fluxon (i.e., flux quantum) is half of the London unit*[3]*. It means that you had obtained a technique to observe the fluxon of a superconductor. So, did you begin the series of the experiments of the observation of magnetic vortex movement in superconductors?*

[3] The shift of the interference pattern in the experimental demonstration of the AB effect tells us this fact. See also B. S. Deaver and W. M. Fairbank, Phys. Rev. Lett. **7**, 43 (1961).

Tonomura: Yes. There is a problem of flux pinning in the theory of high-temperature superconducting. I am interested in it. We have to solve the problem practically to use high-temperature superconductors as an application. Namely, when an electric current goes through the material of a high-temperature superconductor while still in a superconducting state, many fluxons are formed in it, and they undergo motion. The motion generates heat in the material and breaks the superconducting state. There is the notion of pinning to prevent the fluxon's motion. Thus, it is important to understand the mechanism of the fluxon's motion and the mechanism of pinning. By using the 1-MV (1 million volt) holography electron microscope, we could become the first in the world to succeed in visualizing the fluxon dynamics in a high-temperature superconductor (Nature **412** 620 (2001)).

1-MV holography electron microscope



Formation of holography electron microscope

Bulletin: *One million volts? That many!*

Tonomura: We need it to obtain bright electron beams. I would like to point out that the electron gun (i.e., electron source) of a holography electron microscope has to fire many electrons, namely, it makes a high electric current density, to obtain the brightness of the electron beams.

Bulletin: *Sounds like an accelerator. To observe an object you put it between the electron gun and the electron biprism. Firing electrons to the object from the electron gun, you get some data as a hologram on the detector. Thus, to get the exact image of the object, your holography electron microscope needs the solution of an inverse problem with respect to phase, right? If the electron in the* 1*-MV holography electron microscope has very high energy, then the electron has to be considered a relativistic particle, doesn't it? Is there any theory to analyze the image of the object for the holography electron microscope?*

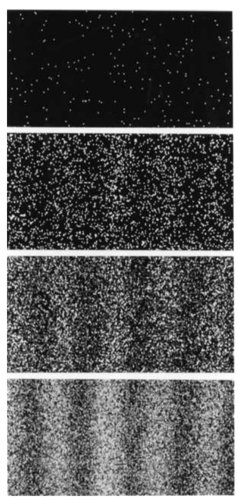
Tonomura: Unfortunately, I don't think there is such a strict theory to the best of my knowledge. I think the high-voltage holography electron microscopes are not so familiar to theorists that they are motivated to find such a theory, because not so many people can construct the high-voltage holography electron microscopes. We may expect some collaboration of applied mathematics and technology in this field.

Bulletin: Meanwhile, in the 2002 poll the readers of Physics World[4] *selected your double-slit experiment with electrons as the most beautiful experiment in the history of science. I've thought that you would have another goal from the demonstration in the double-slit experiment. Can I ask about that?*

Tonomura: In fact, I hesitated to submit the result to a journal because I felt it was just a demonstration of the interference experiment with electrons proposed by R. Feynman[5]. But Prof. H. Ezawa talked me into writing the paper on the result. With his collaboration the result led to the publication (Amer. J. Phys. **57** 117 (1989)). Actually, there was an experiment stimulated by Hanbury Brown and Twiss. Their original experiment, of course, was for photons and they discovered the photon bunching in light emitted by a chaotic source, which is called the Hanbury Brown-Twiss effect. This property comes from the Bose nature of photons. I wanted to observe the anti-bunching of electrons, that is, I wanted to study an electron complement to the experiment of Hanbury Brown and Twiss for photons. Interestingly, we could sometimes observe the phenomena which show the bunching property for electrons.

Bulletin: Does it mean that two electrons make a boson? I wonder what is the force to connect them, but it reminds me somewhat of the d-pairing, caused by the exchange of spin fluctuation, of a conventional superconductor. What is the progress on it?

Tonomura: That is in the pipeline. It depends on the budget we can get.



Double-slit experiment

IAMP Editorial Board would like to thank Dr. Tonomura for providing the photos with their kind permission.

The interview was taken in Japanese and translated into English by
Masao Hirokawa (Okayama, Japan)

[4] a British science journal.
[5] in §1-4 of "The Feynman Lectures on Physics, Vol. 3," Addison-Wesley (1965).

News from the IAMP Executive Committee

New individual members

IAMP welcomes the following new members

1. Jean-Marie Barbaroux, Department Mathematiques, Centre de Physique Theorique, Luminy, Marseille, France
2. Eyal Lubetzky, Microsoft Research, Redmond WA , USA
3. Hatem Najar, Departement de Mathematiques, Universite de Kairouan, Kairouan, Tunisia
4. Alessandro Pizzo, Department of Mathematics, University of California, Davis, USA
5. Sylvia Serfaty, Laboratoire Jacques-Louis Lions, Universite Pierre et Marie Curie, Paris, France and Courant Institute, New York, USA
6. Mikhail Sodin, School of Mathematics, Tel Aviv University, Tel Aviv, Israel
7. Rafael Tiedra, Facultad de Matematicas Pontificia Universidad Catolica de Chile, Santiago, Chile

New associate members

IAMP welcomes a new associate member:

- The Center for Mathematics and Theoretical Physics (CMTP)
  Link: http://cmtp.uniroma2.it/

Open positions

- Deadline October 31, 2010: The research group Mathematical Theory of Quantum and Classical Magnetic Systems of the Pontificia Universidad Catolica de Chile (PUC) invites applications for a one-year postdoctoral fellowship beginning March 15, 2011. Applicants should have a recent Ph.D. in mathematics or physics and should work in the group's research area (quantum mechanics, spectral analysis, scattering theory, functional analysis). The postdoctoral fellow is expected to interact with group members and should therefore be able to communicate in either English, Spanish or French. The fellowship involves no teaching. The annual stipend of CLP$18'000'000$(US 33'500) is tax-free but compulsory Chilean medical insurance is required. While this is a one-year fellowship, the successful applicant may re-apply for a similar fellowship in 2012 on an equal basis with other applicants. The PUC is a leading Chilean research university with strong doctoral programs in mathematics and physics. A group headed by R. Benguria at PUC has been funded by the Chilean government's Iniciativa Cientifica Milenio for a 3-year renewable period beginning in 2010 to conduct research in the Mathematical Theory of Quantum and Classical Magnetic

Other group members are O. Bourget (PUC), E. Friedman (U. de Chile), M. Mantoiu (U. de Chile), G. Raikov (PUC) and R. Tiedra (PUC).

Applicants should arrange that a curriculum vitae, research statement and two letters evaluating the applicant's research be e-mailed to Rafael Benguria at

rbenguri@fis.puc.cl

In addition, applicants are encouraged to contact relevant group members directly. For full consideration, complete application materials in English, Spanish or French must arrive by October 31, 2010. Review of applications will begin on October 31, 2010.

Recent conference announcements

With support from IAMP:

- Feb 14–19, 2011, "School and Workshop on Mathematical Methods in Quantum Mechanics", Bressanone, Italy
- Sep 28–Oct 1, 2010, Quantum Field Theory and Gravity, Regensburg, Germany
  LINK: `http://www.uni-regensburg.de/qft2010`

Other announcements:

- Sep 22–25, 2010, Seminal Interactions between Mathematics and Physics, Rome, Italy
  LINK: `http://cmtp.uniroma2.it/10SIMP/`
- Sep 21–25, 2010, Geometry and Physics in Cracow, Jagiellonian University, Krakow, Poland
  LINK: `http://th-www.if.uj.edu.pl/~krageomp/GPC/GeomPhysCracow.html`
- Sep 20–24, 2010, New Approaches in Many-Electron Theory: Basic Physical Principles and Mathematical Rigor, Mainz, Germany.
  LINK: `http://www.mpip-mainz.mpg.de/theory/events/namet2010`

Jan Philip Solovej (IAMP Secretary)

Center for Mathematics and Theoretical Physics

The Center for Mathematics and Theoretical Physics (CMTP), dedicated to Tullio Levi Civita, has been founded on November 17th 2009 by a group of researchers from the three Roman Universities: Sapienza, Tor Vergata and Roma Tre.

The Center aims at promoting cross fertilization of Mathematics and Theoretical Physics at the highest level, by fostering creative interactions of leading experts from both subjects and by taking advantage of the high quality and wide spectrum of research in Mathematical Physics presently carried on in Roma: this ranges from statistical mechanics to disordered and complex systems, condensed matter theory, quantum field theory, operator algebras, algebraic geometry, differential geometry and dynamical systems.

The Center promotes scientific research by organizing workshops, congresses, periods of thematic research, invitations to scientists and by assigning study grants. The Center wants to attract to Roma foreign scientists of great international prestige, even with part-time positions, and young talented foreigners by offering a natural place for scientific education and a base of cultural interchange with other scientific centers abroad.

The Center is particularly pleased to join IAMP as an associate member and we are looking forward to start a close and fruitful collaborations in promoting mathematical physics. IAMP Members who are interested in participating in our activities are invited to visit our website at http://cmtp.uniroma2.it/.

The Director: Roberto Longo.

The Vice-directors: Alberto De Sole and Alessandro Giuliani.

The Scientific Board: Massimo Bianchi, Corrado de Concini, Sergio Doplicher, Giovanni Gallavotti, Francesco Guerra, Giovanni Jona-Lasinio, Carlangelo Liverani, Roberto Longo, Rossana Marra, Fabio Martinelli, Vieri Mastropietro, Giorgio Parisi, Paolo Piazza, Errico Presutti, Claudio Procesi, John Roberts.

Communicated by
Roberto Longo
Alberto De Sole
Alessandro Giuliani

Vladimir Igorevich Arnold

June 12, 1937 (Odessa) - June 3, 2010 (Paris)

On the 3rd of June, 2010 one of the greatest mathematicians of our time, Vladimir Arnold, died in Paris. This was absolutely shocking news for all his friends and colleagues, the more since he enjoyed enviable health and recent tests did not show anything dangerous. Arnold's heritage includes many remarkable results in Mathematical Physics. We give below only a brief list of them.

In the famous KAM-theory the letters K and A stand for Kolmogorov and Arnold, while the letter M stands for Jürgen Moser. Arnold's advisor Andrey Kolmogorov made the first fundamental steps and outlined the whole program. Arnold found many important cases when the theory works, and proved the theorem in the analytic setting. Moser proposed complete proofs of many results and described the case of finite smoothness. Applications of the KAM theory considered by Arnold included the existence of magnetic surfaces, the theory of adiabatic invariants, etc.

Arnold's diffusion started with his construction of a very beautiful example of a one-parameter family of Hamiltonian systems to which KAM-theory can be applied, but the system is unstable and has trajectories, which escape to infinity.

The Arnold-Liouville theorem states that in Hamiltonian systems with n degrees of freedom invariant submanifolds of dynamics consist of tori or multi-dimensional cylinders.

The Arnold tongues appear, in particular, in families of one-dimensional diffeomorphisms of the circle as a fractal set of parameters, for which the limiting maps have irrational rotation numbers. Later it was shown that the fractal has a non-trivial Hausdorff dimension.

In fluid dynamics Arnold described the Euler equation for an ideal fluid as the equation of geodesics on the group of volume-preserving diffeomorphisms. This allowed him to apply geometric and group-theoretical methods to the study of this equation. In particular, he developed what is now called "Arnold's stability method" for fluid flows. He conjectured that the ABC (Arnold-Beltrami-Childress) flows, which are steady-state solutions of the incompressible Euler equation, have chaotic streamlines, leading to a kind of Lagrangian turbulence and favoring the growth of a magnetic field when the flow is conducting. Arnold and collaborators got involved in advanced numerical experimentation with such flows. He also introduced topological methods in hydrodynamics, e.g., Arnold's asymptotic Hopf invariant.

Arnold was a founder of symplectic topology, which is a far-reaching generalization of the geometric Poincaré theorem in classical mechanics. He developed the theory of fronts and caustics, singularity theory, and several other domains.

Vladimir Arnold wrote many remarkable textbooks and monographs, and some of them, like his undergraduate textbooks "Ordinary differential equations" and "Mathematical methods of classical mechanics", became modern classics, and an integral part of mathematical education around the world. Arnold also left behind a brilliant school of mathematicians, many of whom continue to work in directions initiated by him.

Arnold influenced mathematical physics in many ways. His impact keeps inspiring many of us in our research, which is the best reminder of Arnold's remarkable heritage and personality.

The editors