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FIFTY YEARS OF HARD-SPHERE BOSE GAS: 1957–2007*

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Fifty years ago, Yang and I worked on the dilute hard-sphere Bose gas, which has been experimentally realized only relatively recently. I recount the background of that work, subsequent developments, and fresh understanding. In the original work, we had to rearrange the perturbation series, which was equivalent to the Bogoliubov transformation. A deeper reason for the rearrangement has been a puzzle. I can now explain it as a crossover from ideal gas to interacting gas behavior, a phenonmenon arising from Bose statistics. The crossover region is infinitesimally small for a macroscopic system, and thus unobservable. However, it is experimentally relevant in mesoscopic systems, such as a Bose gas trapped in an external potential, or on an optical lattice.

Keywords: Bose gas; hard sphere.

1. Introduction

On this occasion in celebrating Professor Chen Ning Yang's eighty-fifth birthday, I want to offer a bit of reminiscence of the time Professor Yang and I worked on the dilute hard-sphere Bose gas, and a bit of fresh understanding on this old subject.

I met Professor Yang for the first time in 1956 at the Institute for Advanced Study in Princeton, when I arrived as a post-doctorate from MIT. Long before that, of course, I had known him by reputation. I was enthralled by his beautiful result on the spontaneous magnetization of the Ising model,[1] and his work with T. D. Lee on phase transitions and the "circle theorem".[2,3]

When I met T. D. Lee in 1954, the first thing he told me was that Yang had just proposed a marvelous theory, in "which space was filled with gyroscopes at every point". He was, of course, referring to the Yang-Mills gauge theory of 1954, which has become the foundation of the standard model of elementary particles.[4]

We started working on the quantum-mechanical hard-sphere interaction, and published our first paper in 1957, exactly fifty years ago.[5] That was a memorable

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year, as Yang and Lee won the Nobel prize later that year for their work on parity violation.[6]

The hard-sphere Bose gas has become relevant experimentally since 1995. On the theoretical front, we have continued to gain new understanding up to now.

2. Hard Spheres

My Ph.D. thesis at MIT was on the saturation of nuclear forces.[9] Victor Weisskopf was my supervisor, but I worked on a daily basis with Sidney Drell, who was then assistant professor. Meson theory[a] predicted that nucleons interact with many-body forces,[10] and we wanted to see whether they lead to nuclear saturation. Drell and I did independent calculations, and bet a nickel whenever we disagreed. One of the problems we struggled with was how to treat the hard core in the nuclear potential. We used what was known as the "Jastrow wave function"[11,12] to do variational calculations, but it was not satisfactory from a basic point of view. I continued to think about this problem when I arrived at Princeton.

As it turned out, Yang also worked on the hard-sphere problem the year before, in collaboration with J. M. Luttinger; but abandoned the project because they got divergent results.

Yang and Luttinger replaced the hard-sphere potential by a delta function, and tried to calculate the energy in perturbation theory. For a quantum gas of N identical particles, the ground-state energy per particle to first order is

$$\frac{E_0}{(\hbar^2/2m)N} = 4\pi a n \tag{1}$$

where a is the hard-sphere diameter, m the particle mass, and n the particle density. This result was first obtained by W. Lenz in 1929, by estimating the quantum-mechanical kinetic energy due to excluded volume.[13] However, the second-order result diverges.

3. Pseudopotential

After thinking a bit about Yang and Luttinger's calculation, I thought I knew where the divergence came from and how to fix it. As a graduate student at MIT, I had worked with Weisskopf on the electron-neutron interaction. This was treated earlier by Fermi,[14] using a delta-function potential called a pseudopotential, with the proviso that it be used only to first order in perturbation theory. And Weisskopf knew how to improve it.[15]

The point can be illustrated in the two-body problem. As illustrated in Fig. 1, the delta function potential $\delta(r)$ makes the wave function vanish at a, which is what we wanted. But if we continued it to inside the hard sphere, the wave function

[a]The prevailing meson theory was pseudoscalar theory with psudoscalar coupling, soon to be superceded by pseudovector coupling in the Chew-Low theory of the "three-three resonance".

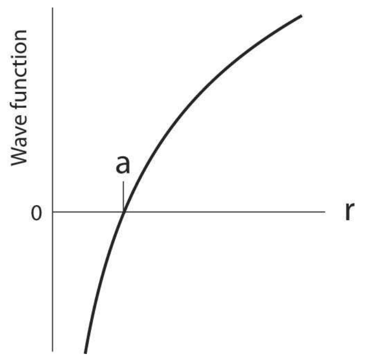

Fig. 1. The wave function of a particle in a delta-function potential vanishes at the scattering length a, which is an effective hard-sphere diameter. When contnued inside the hard core, it develops a $1/r$ singularity, which is spurious.

diverges like $1/r$. We should ignore the wave function inside a, but there is no way to do this in perturbation theory, for the formalism tells us to integrate over all space, and the $1/r$ singularity leads to a spurious divergence. Weisskopf's solution is to modify the potential through the replacement

$$\delta(r) \to \delta(r)\frac{\partial}{\partial r}r\,. \tag{2}$$

The differential operator $\partial/\partial r$ expunges any term proportional to $1/r$, but does nothing if the wave function is regular at $r = 0$. The pseudopotential is really a way to introduce a boundary condition just outside of the potential, to properly describe scattering from the potential. The correct pseudopotential is in fact

$$V(r) = \left(\frac{4\pi a\hbar^2}{m}\right)\delta(r)\frac{\partial}{\partial r}r\,. \tag{3}$$

Here a is the scattering length, the long-wavelength limit of

$$-\frac{\tan\,\delta(k)}{k} \tag{4}$$

where k is the scattering wave number, and $\delta(k)$ the s-wave scattering phase shift. For hard-sphere scattering, $\delta(k) = -ka$, and the scattering length coincides with the hard-sphere diameter. As a quantum-mechanical operator, the pseudopotential is non-hermitian; but it gives an effective Hamiltonian, and should yield real eigenvalues.

I outlined the argument to Yang, and he was somewhat dubious at first, but left a note on my desk that evening:

"I thought about it. What you said about $\tan\delta$ was correct."

We then calculated the energy levels of a Bose gas and a Fermi gas in perturbation theory, with no divergences.[5] We also calculated the virial expansion of the equation of state of a hard-sphere Bose gas, and studied how the interaction modifies the

Bose–Einstein condensation.[16] This was a fulfillment of what Yang and Luttinger had started to do.

4. An Encounter with Pauli

Yang was away when I gave a seminar at the Institute for Advanced Study on our calculations. Besides local luminaries, including Oppenheimer, Wigner, and Dyson, there was a very special guest in the audience, Wolfgang Pauli. When the seminar began, he started to nod and fall sleep, until I mentioned that the effective Hamiltonian was not hermitian.

"Not hermitian?" Pauli opened his eyes.

Oppenheimer, sitting next to Pauli, leaned over to him and said, "That's really OK, because ..."

"It is not hermitian! I do not like this!" said Pauli.

In his more youthful days, I guess, Pauli would have chased me out of the room, or left the room himself. But that day, he nodded back to sleep after some grumbling, and I was able to finish the seminar.

At that time, I was renting a room in a gracious old house in Evelyn Place owned by a European lady, Mrs. Loewy. Her late husband must have been a serious scholar, for the living room was filled to the ceiling with old tomes, mostly in German. When I went back to the house that evening, Mrs. Loewy was waiting for me.

"Guess who came to visit today", she beamed, "Pauli!"

"He said you gave a seminar, and it was '*nicht dumm*'[b]!"

5. Peritization

We calculated the ground state energy per particle to second order in perturbation theory:

$$\frac{E_0}{(\hbar^2/2m)N} = 4\pi an \left[1 + A_2 \left(\frac{a}{L}\right)\right] \tag{5}$$

where L is the linear dimension of the box containing the system, and

$$A_2 = 2.837297\cdots \tag{6}$$

In Ref. 5, this constant was cited as 2.73, a poor estimate due to my clumsy numerical work. It was calculated exactly by M. Lüscher[17] years later, and Yang[18] found an elementary derivation of his result. To the same order of approximation, the excited states are separated from the ground state by an energy gap ϵ_0:

$$\frac{\epsilon_0}{\hbar^2/2m} = 8\pi an\,. \tag{7}$$

[b]'not dumb'

The second-order correction to the ground state energy per particle vanishes in the macroscopic limit, when the system becomes infinite with finite density. So, we got zero instead of a divergence.

Upon closer examination, however, there is trouble. A graphical analysis shows that the expansion has the general form[19]

$$\frac{E_0}{(\hbar^2/2m)N} - 4\pi an = \frac{1}{NL^2}\left[A_2\left(\frac{aN}{L}\right)^2 + A_3\left(\frac{aN}{L}\right)^3 + \cdots\right] + \frac{1}{N^2L^2}\left[B_2\left(\frac{aN}{L}\right)^2 + B_3\left(\frac{aN}{L}\right)^3 + \cdots\right] + \cdots \quad (8)$$

We see that the expansion parameter is aN/L, which diverges in the macroscopic limit. Thus, beyond second order, the divergence comes back in a different form.

We left the issue unresolved over the summer of 1957, when I went to Bell Labs and worked on the electron gas with David Bohm and David Pines.[20] I think Yang spent the summer working mainly on weak interactions with T. D. Lee. In their spare time, they toyed with the idea that summing the series in (8) horizontally might yield a finite result. That is, one should sum the most divergent terms.

The first horizontal line in (8) has the form $[1/(NL^2)]f(aN/L)$, which in the macroscopic limit must tend to a function of the finite combination $aN/L^3 = an^{1/3}$. Assuming $f(x)$ to be a power, one finds that the power must be $5/2$.[21] Thus, instead of a power series, the expansion would become one in fractional powers.

A few years earlier, Lev Landau[22] summed the "leading logarithms" (most divergent terms) in the perturbation series for the renormalized charge of the electron in quantum electrodynamics (QED), and obtained the provocative result now referred to as "triviality": the renormalized charge of the electron is zero in the limit of infinite cutoff. Noticing a pattern, Abraham Pais proposed the name "peritization" for the summing of the most divergent terms.

Back from summer vacation, T. D. Lee joined us to actually sum the series. After some hard work we succeeded, and obtained the well-known result[23]

$$\frac{E_0}{(\hbar^2/2m)N} \to 4\pi an\left[1 + \frac{128}{15\sqrt{\pi}}\sqrt{na^3} + O(na^3)\right], \quad (9)$$

where the arrow denotes the macroscopic limit.

In the old power-series scheme, the excited states were separated from the ground state by an energy gap. In the new scheme, the gap closes, and we have phonon excitations with energy ω_k, given by

$$\frac{\omega_k}{\hbar^2/2m} = k\sqrt{k^2 + 16\pi an}\,. \quad (10)$$

This identifies the sound velocity to be $(\hbar/2m)\sqrt{16\pi an}$, which agrees with that calculated independently from the compressibility of the ground state.

After the work was done, we learned that N. N. Bogoliubov[24] had derived the same spectrum through a transformation of the Hamiltonian that now bears his name. The Bogoliubov transformation, which has become a standard tool, may be looked upon as an easy and elegant way to "peritize" the perturbation series.[c]

Landau's peritization in QED can also be done more elegantly – through the renormalization group.[25]

6. Higher-Order Corrections

Wu[35] calculated the ground-state energy per particle to the order beyond $a^{5/2}$:

$$4\pi an\left[8\left(\frac{4\pi}{3}-\sqrt{3}\right)na^3\ln(12\pi na^3)\right].$$

The logarithmic dependence was interesting and unexpected. However, a variational calculation[36] disagrees strongly with this correction, while agreeing very well with the lower-order terms. A more recent Monte Carlo calculation[37] obtained a similar result. In particular, agreement with the above correction took a dramatic turn for the worse when $na^3 > 10^{-2}$. What this means remains a mystery.

7. Superfluidity

All particles have zero momentum in the ground state, in the ideal gas,. However, the occupation of the zero-momentum level is less than 100% in the interacting gas. The deficit is known as the "depletion fraction", and was calculated to be $8/3\sqrt{na/\pi}$. However, at zero temperature, the entire system is a superfluid, i.e., $n_s = n$, where n_s is the superfluid density. We can find n_s through the linear response of the system to an infinitesimal hypothetical velocity field.[36] A calculation at zero temperature, using the pseudopotential, was carried out by Huang and Meng[37] in the presence of a quenched random potential. As expected, $n_s = n$ in the absence of randomness, but n_s decreases as the strength of the random potential increases, and there is a critical strength above which $n_s = 0$. This indicates that the random potential pins the condensate, preventing it from moving. The result has been verified by an independent calculation.[38] Thus, we can have condensate without superfluidity.

8. Two-Dimensional Bose Gas

In spatial dimension $D = 2$, a Bose gas does not have a condensate, interacting or not. This is due to the absence of long-range correlations, due to strong local fluctuations of the quantum phase of the ground-state wave function. A local condensate does exist, but it contains topological defects, which are vortices. In a low-temperature phase, when vortices and antivortices form bound states, the

[c]There is a technical difference, however. In the Bogoliubov transformation, one assumes that the ground-state occupation is a number instead of an operator. We did not make that assumption in our explicit summation.

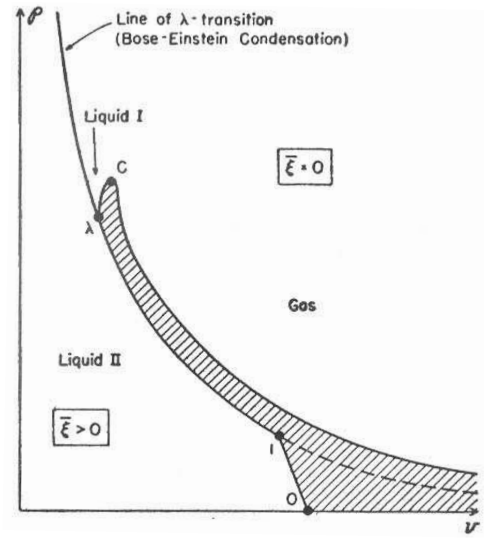

Fig. 2. Equation of state of dilute Bose gas with hard-sphere repulsion plus short-range attractive interaction, as represented in a P-V diagram. the shaded region is the modification to the transition line of the ideal gas, showing the formation of a liquid phase. The Bose–Einstein condensation occurs in the liquid phase, just as in liquid helium. The whole shaded region shrinks about the ideal gas line and slides off to the left, when interactions are turned off.

system is a superfluid. Thus, we can have superfluidity without condensate. At a higher temperature, the Kosterlitz–Thouless phase transition occurs, whereby the bound vortex pairs become ionized, and supefluidity ceases.[39]

9. Liquid Helium and Atomic Trap

The physical motivation to study the hard-sphere Bose gas was the understanding of the superfluid transition in liquid helium. However, that transition in liquid helium happens not in the gas phase, but in the liquid phase. To reproduce the phase diagram, at least qualitatively, one must produce a first-order gas-liquid phase transition, and that requires the addition of an attractive potential. I was able to do that, and show how the phase diagram emerges from that of the ideal Bose gas, as one turns on the hard-sphere and attractive interactions.[26,27] This is shown in Fig. 2.

There, the matter lay for almost forty years until 1995, when a dilute Bose gas was experimentally realized in an atomic trap, and Bose–Einstein condensation was observed.[7,8] This inspired a fury of theoretical activity,[28] all based on an interaction described by an effective hard-sphere diameter, the s-wave scattering length.

Yang[29] returned to the subject to treat the trapped atomic gas, whose density is not uniform in space, through a local density approximation. I participated in formulating a generalized Thomas-Fermi approximation.[30]

10. Crossover between Ideal and Interacting Gas

It has been a puzzle why the expansion parameter changes from aN/L to $a^{1/3}n$. I recently realize that this is an expression of a crossover from ideal gas to interacting gas.[31]

The idea of crossover originates in the study of the renormalization group pioneered by Kenneth Wilson and Michael Fisher.[32] Generally, a system is governed by different effective Hamiltonians at different scales, each corresponding to a "fixed point" in the space of Hamiltonians. When the scale changes, the system "crosses over" from one fixed point to the other. For example, a system confined between two plates may look like a 2D system, but when the plate separation increases, it will eventually cross over to 3D behavior.

The expansion parameter in an extremely dilute Bose gas is

$$\frac{aN}{L} \sim \sqrt{\frac{\text{System size}}{\text{Mean-free-path in condensate}}}\,. \tag{11}$$

The mean-free-path in the condensate is of order $(n\sigma)^{-1}$, with scattering cross section $\sigma \sim Na^2$, where the factor N comes from Bose enhancement. For small aN/L, the system is a Knudsen gas, in which collisions are infrequent. In another region with small mean-free-path, one goes over to the hydrodynamic regime. This is illustrated in Fig. 3 for a gas coming out of a hole in the wall.

The crossover happens because of the factor N in aN/L, which makes the parameter diverge in the macroscopic limit. It is thus a property of Bose statistics, and there is no corresponding phenomenon in the Fermi gas.

Can we observe the crossover experimentally? To answer this question, let me retrace the origin of the idea.

In the ideal Bose gas, Bose–Einstein condensation occurs in 3D because the state of nonzero momentum cannot accommodate more than a certain number of particles. This number depends on temperature, and when the temperature falls below a critical value, these states can no longer accommodate all the particles present, and the excess is forced into the state of zero momentum, forming the

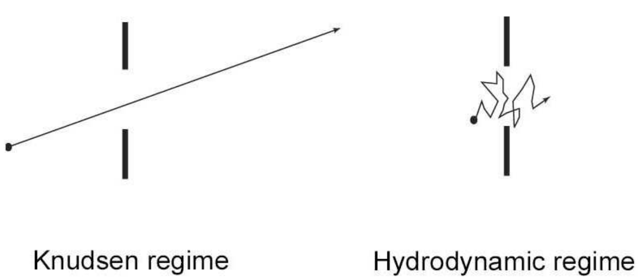

Fig. 3. In the Knudsen (or collisionless) regime, the mean-free-path is much greater than relevant lengths in the system. The opposite is true in the hydrodynamic regime. There is a crossover between these regimes in an interacting Bose gas as a consequence of Bose statistics, as explained in the text.

Bose–Einstein condensate. The critical temperature $T_c^{(0)}$ for an ideal Bose gas in 3D, of density n, is

$$k_B T_c^{(0)} = C n^{-2/3} \tag{12}$$

where $C = 2\pi\hbar^2/m[\zeta(3/2)]^{-2/3}$.

I calculated the critical number for the hard-sphere gas, using the virial series we calculated forty years ago, and obtained a shift in transition temperature T_c due to the interaction[33]

$$\Delta_0 \equiv \frac{T_c - T_c^{(0)}}{T_c^{(0)}} = c_0\sqrt{an^{1/3}} \tag{13}$$

where $c_0 = 8\sqrt{2\pi}/3[\zeta(3/2)]^{2/3} \approx 3.527$. This disagrees strongly with numerical computations based on a scheme due to Gordon Baym,[34] which give a linear dependence on a:

$$\Delta_1 = c_1 a n^{1/3}\,. \tag{14}$$

The constant c_1 is of order unity, and varies according to the computation scheme.

The two results differ because they were calculated in different regions. My calculation was done in the gas phase, approaching the transition from the high-temperature side, whereas the other calculations approach it from within the condensed phase. These give different answers because of the crossover.

11. Observation of Crossover

The Knudsen regime is relevant for kinetic processes, such as effusion through a hole, when the mean-free-path is much larger than the dimension L of the hole, as illustrated earlier in Fig. 3. For a macroscopic system in thermal equilibrium, L is the size of the whole system, and the Knudsen region consists of an infinitesimally small neighborhood of zero density, and is of little experimental interest.

For a Bose gas condensed in a potential or on an optical lattice, however, the Knudsen regime spans a region accessible to experimentation. In this case, one can vary the system size, particle number, and even the scattering length through Feshbach resonances. The relevant dimension L in a harmonic trap of frequency ω_0 is the harmonic length

$$L = \sqrt{\frac{\hbar}{m\omega_0}}\,. \tag{15}$$

The trapped gas behaves like an ideal gas when $aN/L \ll 1$, and crosses over to the Thomas-Fermi regime when $aN/L \gg 1$. During the crossover, the size of the condensate increases from the ideal-gas radius

$$R_0 \sim L \tag{16}$$

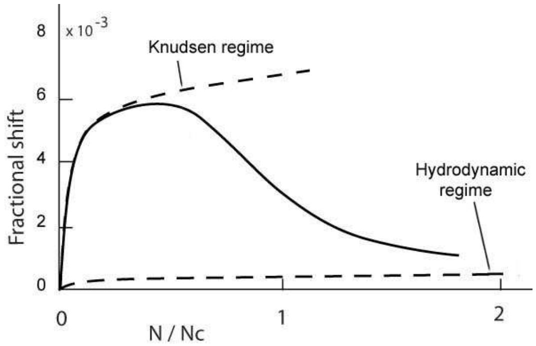

Fig. 4. Fractional shift of the Bose–Einstein transition temperature for the Knudsen and hydrodynamic regimes are shown as dotted curves. The solid line is a visual interpolation between the two.

to the Thomas–Fermi radius[30]

$$R_{\mathrm{TF}} \sim \left(\frac{aN}{L}\right)^{1/5} L\,. \tag{17}$$

The excitations change from ideal-gas states in the trap to collective oscillations, the lowest ones being the "breathing" (monopole) and quadrupole modes.[28]

As illustration, take $a = 1$ nm, $L = 100$ μm. The critical particle number is

$$N_c = \frac{L}{a} = 10^5\,. \tag{18}$$

The transition temperature depends on N, and interpolates between two fractional shifts:

$$\Delta(N) = \begin{cases} \Delta_0 & (N \ll N_c) \\ \Delta_1 & (N \gg N_c) \end{cases}\,. \tag{19}$$

The values given in Eqs. (13) and (14) are for the uniform gas, but may be used for order-of-magnitude estimates. The two curves corresponding to Δ_0 and Δ_1 are plotted in Fig. 4. By interpolating between them, we get an estimate of how the actual shift might behave. The fractional shift is more than ten times greater in the Knudsen gas, indicating that the condensate is more stable against excitation owing to the energy gap.

Currently, experiments involve millions of atoms, and are well in the hydrodynamic regime. It would be interesting to explore the Knudsen regime on an optical lattice, not so much for measuring the temperature shift, but for kinetic properties of adding atom to and extracting from the lattice.

12. Conclusion

After fifty years, the dilute hard-sphere Bose gas still enjoys a vigorous life, and we wish the same for Professor Yang.

13. Questions and Answers

C. N. Yang:

I want to make several remarks. The first one was: actually around 1956 to 1957, I wrote more than 10 papers more or less about the subject. But later on since there were no experimental verification and, theory-wise we hit the stone wall. So I tend to forget it. In the 1990's there was a Prof. B. A. Li of Kentucky who wrote an article about my contributions to physics and he asked me to name something like 10-areas. I recently checked, I did not mention these 10 papers. Of course, I'm very happy now that with this beautiful and the unbelievably exciting new developments about Bose–Einstein condensation, and the cold atom physics that Prof. Cohen-Tannoudji had told us about, the subject is revived and the people are now measuring sound velocities in both Fermi and Bose gases. And I checked the science citation — my papers of 1956, 1957 and around that time — netted something like 600 references. It is a bit like a revival of an old friend who has been dead for many years!

My second remark is that the subject is extremely subtle and reading some of the new papers, I'm afraid a number of theorists working on it did not completely appreciate the subtlety. I would advise them to read the paper of Lee, Huang and me, ... which by the way was discussed at the same time, in a different area, by Bardeen, Cooper and Schrieffer, in particular, its pairing, the elimination of the singularity that Kerson just mentioned and the excitation spectrum, etc. are very non-trivial and very subtle things.

The third remark I want to make is that if you read the paper, the three of us calculated the number of particles in the momentum zero state. We made a series expansion of that and clearly demonstrated that it is less than 100%, when you have any interaction. And yet because of this beautiful experiment by a scientist with long Russian name which I can't pronounce ... (some name) the percentage is 100%. So did we make a mistake? I don't think so. In fact also Onsager and Penrose had in another way made an estimate, and they said the momentum zero state for zero temperature in liquid helium is only 8%. And not 100%. And yet this beautiful Russian experiment showed that the condensate had to be 100%. OK, I know the secret. I believe I know the secret. It's because of the shape of the excitation spectrum near the ground state. What happens is that although you have less than 100% condensed from a mathematical calculation, the part which is not condensed does not form new states. There is a gap. Now I think that is a very deep subject and I wish I have the energy that Kerson Huang wished me to have to explore this. I think this is the most important subject. Thank you.

David Thouless:

I think I would like to take the chair's privilege to answer at least part of Prof. Yang's question. Yes, we've known for a very long time that there's a difference between the proportional particles in the condensate and the superfluid density. We've known what it is. And part of our understanding comes from work in the 1960's by Brian Josephson who discussed the fact that the critical experiment relating to the superfluid fraction appeared to be thermal coefficients and were not related to the magnetization in the magnetic analogy, so that's something which is water under the bridge a very long time ago. I want to make another comment which is of course at the same time as the beautiful work on the Bose system came out, there were a lot of papers on the Fermi system where in the sense the experimental challenge is much greater because people knew an awful lot about plasma frequency and such things and they had to get it right. And there was a lot, I won't mention the names, Bowman, Pines's work two of the pioneers on this resumming things was a major part of getting that straight, if less elegant than work on the Bose–Einstein. Sorry for taking your time.

Michael Fisher:

I just want to add to the history because one of the things in our chat was to show what was wrong with the BCS theory. And it was such a successful theory you couldn't see where the fluctuations were buried, and that's one of the things David Thouless did which for me removed the eye shade entirely. I'm glad that he said because otherwise I would have to interrupt on the understanding of the ρ_s. And again this is an oversight really of the simple mean field theory of the Landau approach. You have to understand the difference between the condensate and essentially ρ_s and that's again crucial in what David would be too modest to mention the cause of Thouless' transition of two dimensions where the n_0 is rigorously zero but the ρ_s is not and so again it is a bit like the question I was asked about $D_\mu = -\alpha$. You have to understand the correlation in quite a lot of detail and in a simple situation, all put together, you'll first think, you have to look much more carefully what the experiment measures.

Kerson Huang:

I believe the difference is that the condensate fraction is an equilibrium property where ρ_s is a transport coefficient.

Michael Fisher:

No, that's quite wrong again. Because again this is where finite size effect comes in. The issue again goes back to Onsager because you can ask: What's the interfacial tension? You can say: Well I have to have two faces, I have to work out the free energy difference between them. But what Onsager did in the way I said in a paper: "Well let me just change the boundary conditions. So he considered anti-ferromagnet and he had an odd number of systems, so when you go around that you find $+, -, +, -, +, -, \ldots$ If the whole system is an even number, it fits together as one phase. But Onsager said: Well, let's take it as odd number, $+, -, +, -, \ldots,$ then you come to $+, +,$ and so the free energy is not going to be as low. So there's a difference between odd and even on the full well, and Onsager identified that as simply the interfacial tension. Now if you go to a x–y system here, all you really have to do is to take the ground state of Bose system and you say I'm going to have twisted boundary conditions and that adds to the free energy that essentially is the ρ_s and of course it goes out as r^2 because you can think the phase is changing. So it comes out of the boundary and there's completely an equilibrium situation . . .

Kerson Huang:

. . . that's one way to calculate as the transport coefficient.

Michael Fisher:

. . . ah, no, but again the question is if you think it is a dynamic effect, again this is the same thing . . .

Kerson Huang:

. . . the chance of . . . it is just near equilibrium.

Michael Fisher:

Well in this case it is just a question of the boundary conditions and remember that if you took periodic boundary conditions and set up a flow then it would decay by nuclear Abrikosov vortices. So just as what Prof. Yang said the more you look into the detail at each level, it gets more subtle and it is difficult to keep abreast, but I'm glad that you (Thouless, Yang) brought this up. Because I think it is fairly well understood. And in particular, I insist that ρ_s is a fully equilibrium property.

David Thouless:

I think we better cut this.

Michael Fisher:

I think we have.

David Thouless:

We have one more question.

Anonymous audience:

You told that there's no Bose condensate in 2D system but can we conclude that there's no superfluidity for 2D system?

Kerson Huang:

Well Michael just said that in spite of the fact that there's no Bose condensate there is superfluidity. There is. The Thouless' transition.

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