Nobel Lecture: Topological defects and phase transitions*

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I. INTRODUCTION

It is a great honor to speak to you today about "theoretical discoveries of topological phase transitions and topological phases of matter." Since the main character, David Thouless, is not able to speak here, the two minor characters, Duncan Haldane and I, have been asked to speak for David. This is a very daunting task which I agonized over for a considerable period of time as I feel inadequate for this. Eventually, time ran out and I was forced to produce something relevant so I decided to start by talking about my earliest experience of David and how we ended as collaborators on our prize winning work. Then I will summarize my understanding of his seminal contributions to his applications of topology to classical ($\hbar = 0$) Berezinskii-Kosterlitz-Thouless or BKT phase transition. David has worked on many more applications of topology to quantum mechanical systems such as the quantum Hall effect and Duncan Haldane will talk about David's contributions to these.

My first experience of David Thouless took place in 1961 when I was a freshman at Cambridge University. I was in a large introductory class on mathematics for physics waiting for the instructor to appear to enlighten us when a young man who was clearly too young for this advanced science course walked in. Obviously, he had wandered into the wrong lecture hall. To our astonishment, he stopped in front of the class and proceeded to talk about various complicated pieces of mathematics which most of the class either had not met before or had not understood. It rapidly became clear that the class was in the presence of a mind which operated on a different level to those of the audience. My later experiences of David merely reinforced this early impression. My next meeting with him was in 1971 in the Department of Mathematical Physics at Birmingham University in England where I went by accident as a postdoctoral fellow in high energy physics. After being frustrated for a year, I looked for a new tractable problem and David introduced me to the new worlds of topology and phase transitions in two dimensional systems.

As far as I am concerned, the study of topological excitations started in 1970 when I was a postdoc in high energy physics at the Istituto di Fisica Teorica at Torino University, Italy. As a very disorganized person, I failed to submit my application for a position at CERN, Geneva in a timely fashion and, instead, found myself without a position for the following year. After replying to some advertisements in the British newspapers, I was offered a postdoctoral position in the department of Mathematical Physics at Birmingham University in England. I did not want to go to Birmingham which, at that time, was a large industrial city in the flat middle of England where a lot of cars and trucks were built. It was certainly not my ideal place to live, but my girlfriend and I decided that it was better than the alternative of unemployment. During my first year there, I continued some elaborate field theory calculations but I had an unhappy experience. I was about to write up my calculations for publication when a preprint from a group in Berkeley doing exactly the same thing appeared on my desk. After two or three repeats of this, I became very disillusioned. In desperation, I went around the department looking for a tractable problem in any branch of physics. I appeared in David Thouless' office listening to him describing several new and mysterious concepts such as topology, vortices in superfluids and dislocations in two dimensional crystals. To make matters even worse, my knowledge of statistical mechanics was almost nonexistent as I had omitted that course as irrelevant to high energy physics which I considered to be the only field of physics of any interest. However, to my surprise, David's ideas made sense to me as being new and very different and they seemed worth considering. We began to work on the problem of phase transitions mediated by topological defects, which to my untutored mind seemed just another application of field theoretic ideas and was therefore worthy of consideration. Little did I know just how different and important these ideas and their applications would be in the following decades and where they would take us.

At this point, I would like to talk about David's vital contribution to our understanding of two dimensional phase transitions. In fact, one of our motivations for looking at two dimensions was that we thought that life was easier in two than in three dimensions. David had already done some work on the importance of topological defect driven phase transitions in the context of the one dimensional Ising chain with interactions between spins decaying as $1/r^2$. This model can be discussed in terms of topological defects, or domain walls, interacting as $\ln r/a$ (Thouless, 1969) and David had shown that the magnetization dropped discontinuously to zero at T_c although it was not a first order transition. This was later made quantitative by Anderson, Yuval, and Hamann (1970) who used an early version of the renormalization group (RG). This

*The 2016 Nobel Prize for Physics was shared by David J. Thouless, F. Duncan M. Haldane, and John Michael Kosterlitz. These papers are the text of the address given in conjunction with the award.

work was very influential in our thinking about defect driven phase transitions because it led us to seek other systems in which there are point topological defects with a logarithmic interaction. Examples of this are point vortices in ^{4}He films, in superconducting films and point dislocations in 2D crystals. This, in turn, led us to the Coulomb gas description of such systems. However, those of you who are paying attention to the details will have noticed a serious flaw in this analogy since our basic 1D example is different from our other systems which are Coulomb gases in 2D. The reason why the 1D system with logarithmic interactions works is because of the constraint that the charges or domain walls alternate in sign along the line. If this constraint is relaxed, the phase transition disappears. Of course, this is not the first time that a correct conclusion is arrived at for the wrong reason.

The first thing we had to understand was the role of long range order in crystals and superfluids as the standard picture of a crystal in two dimensions is a system of molecules in which knowledge of the position of a single particle means that one knows the positions of all the others from the equation $\mathbf{r}(n, m) = n\mathbf{e}_1 + m\mathbf{e}_2$ where $\mathbf{e}_1$, $\mathbf{e}_2$ are the fundamental lattice vectors and $n, m = \pm 1, \pm 2, \ldots, \pm\infty$. The problem here is the Peierls argument (Peierls, 1934, 1935) which says that long range order is not possible in two dimensional solids because low energy phonons give a mean square deviation of atoms from their equilibrium positions in an $L \times L$ system increasing logarithmically with the size of the system L. A useful picture of a two dimensional crystal is to consider a flat elastic sheet on which is drawn a lattice of dots representing the atoms of a crystal. Now, stretch some regions and compress other regions of the sheet without tearing it representing smooth elastic distortions of the crystal. Clearly, the dots (particles) will move far from their initial positions—in fact a distance proportional to $\sqrt{\ln L}$—although the lattice structure is preserved. The absence of long range order in this form has been shown rigorously by Mermin (1968). Similar arguments show that there is no spontaneous magnetization in a 2D Heisenberg magnet (Mermin and Wagner, 1966) and that the expectation value of the superfluid order parameter that vanishes in a 2D Bose liquid is zero (Hohenberg, 1967).

According to the conventional wisdom of the early 1970s, this implies that there can be no phase transition to an ordered state at any finite temperature because an ordered state does not exist! However, this minor contradiction did not deter David and myself because David understood the subtleties of the situation and could see a way out of the apparent contradiction while I was too ignorant to realize that there was any such contradiction. In hindsight, I understood that, *very* occasionally, being ignorant of the fact that a problem is insoluble, allows one to proceed and solve it anyway. As luck had it, this was one of those few occasions for me. Of course, it also helped that there existed some experimental and numerical evidence for transitions to more ordered low temperature phases in 2D crystals (Alder and Wainwright, 1960, 1962), very thin films of ^{4}He (Chester, Yang, and Stephens, 1972; Chester and Yang, 1973), and 2D models of magnets (Stanley and Kaplan, 1966; Stanley, 1968; Moore, 1969). The most compelling piece of experimental evidence for us is shown in Fig. 1 where the deviation of $-\Delta f$, the decrease in the resonant frequency f of the crystal with a film of ^{4}He adsorbed on the surface, from the straight line is a

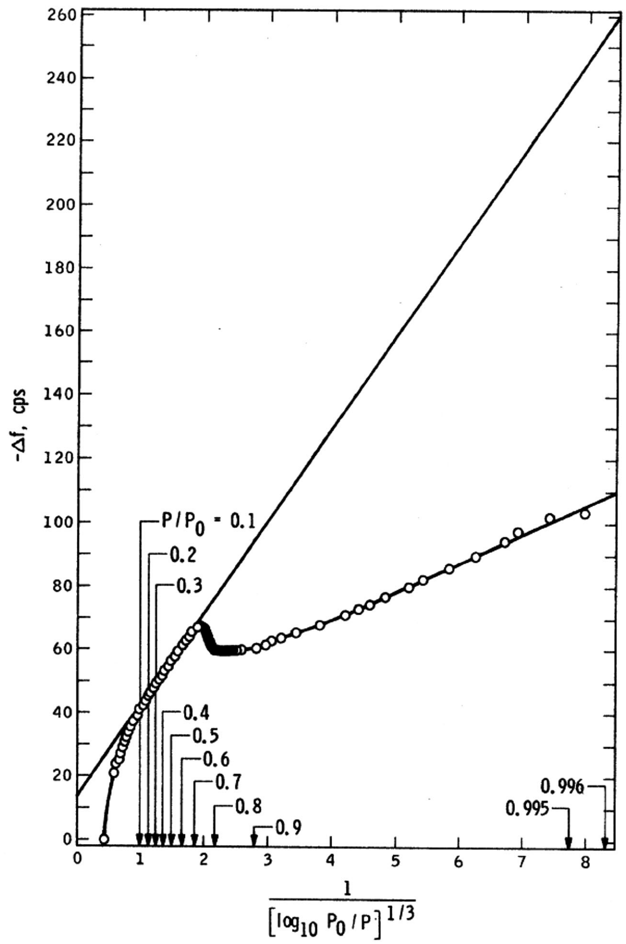

FIG. 1. The horizontal axis is a measure of the total areal mass density of the adsorbed film and the vertical axis is $-\Delta f$, a measure of the adsorbed mass which decouples from the oscillating substrate. Reprinted from Chester, Yang, and Stephens, 1972.

measure of the areal superfluid mass density. Clearly, the 2D film undergoes an abrupt transition as the adsorbed mass density increases with a probable finite discontinuity in $\rho_s(T)$. This behavior seemed very strange as conventional wisdom said that ρ_s would increase continuously from zero as the ordered phase is entered. This needed an explanation which, clearly, had to be rather different from anything known previously.

II. BREAKTHROUGH

The solution to this puzzle is that there can be a more subtle type of order called topological order in some two dimensional systems. The simplest example is the Ising ferromagnet which consists of a set of spins $\sigma = \pm 1$ on a D-dimensional cubic lattice. The rules of statistical mechanics are (i) any configuration of the system occurs with probability $e^{(-E/k_B T)}$ where E is the energy of that configuration and (ii) compute the partition function $Z(T) \equiv \sum_{\text{configs}} \exp(-E/k_B T)$, which gives all necessary thermodynamic information. The most probable excitations are the low energy ones which are responsible for the absence of true long range order but, otherwise, have no effect. To discuss the destruction of superfluidity and the melting of a 2D crystal, we have to include the very improbable high energy topological defects responsible for the destruction of a superfluid and of a crystal. These are the vortices in a superfluid and dislocations in a crystal (Kosterlitz and Thouless, 1972, 1973; Kosterlitz, 1974). I should point out that similar ideas had been proposed a bit earlier by Berezinskii (1971a, 1971b) but, when we did our work, we were not aware of this. For some reason, our work has received much more attention than that of Berezinskii.

Of course, you may well ask about the connection between topology which is the study of spheres with N holes while our physical systems all lie on a flat simply connected 2D surface with no holes. The topology we are considering is determined by the underlying physics and its corresponding energetics and a phase transition can be thought of as a transition between topological sectors defined by the topological invariants. We can discuss the importance of topology by comparing the 2D planar rotor magnet with two component spins and the Heisenberg model with three component spins. For the planar rotor model

$$\mathbf{s}_i = (s_{ix}, s_{iy}) = s(\cos\phi_i, \sin\phi_i),$$
$$\Psi_i = s_{ix} + i s_{iy} = s e^{i\phi_i}.$$

where s denotes the length of the spins, usually taken as unity. Consider a large $L_x \times L_y$ system with periodic boundary conditions (similar considerations hold for other boundary conditions). In the planar rotor model, the direction of magnetization in a region is defined by the angle ϕ which varies slowly in space. Although the angle ϕ fluctuates by a large amount in a large system, the number of multiples of 2π it changes by on a path which goes completely around the system is a *topological* invariant, so that

$$\frac{1}{2\pi}\int_0^{L_x} \frac{\partial\phi}{\partial x} dx = n_x,$$
$$\frac{1}{2\pi}\int_0^{L_y} \frac{\partial\phi}{\partial y} dy = n_y$$

are numbers defining a particular metastable state. Transitions can take place from one metastable state to another only if a vortex-antivortex pair is formed, separates, and recombines after one has gone right around the system. This process causes n_x or n_y to change by 1, but there is an energy barrier proportional to the logarithm of the system size to prevent such a transition.

The same system composed of three component spins

$$\mathbf{s}_i = (s_{ix}, s_{iy}, s_{iz}) = s(\sin\theta_i\cos\phi_i, \sin\theta_i\sin\phi_i, \cos\theta_i)$$

is called the Heisenberg model. A quantity such as

$$\frac{1}{2\pi}\int_0^{L_x} \frac{\partial\phi}{\partial x}$$

is *not* a topological invariant. A twist of the azimuthal angle ϕ by 2π across the system can be continuously unwound by changing the polar angle θ, which we take to be the same everywhere from $\pi/2$ to zero. In fact, the Heisenberg model in two dimensions has a single topological invariant $N = 0, \pm 1, \pm 2, \ldots$ where

$$N = \frac{1}{4\pi}\int dxdy \sin\theta\left(\frac{\partial\theta}{\partial x}\frac{\partial\phi}{\partial y} - \frac{\partial\theta}{\partial y}\frac{\partial\phi}{\partial x}\right).$$

If we regard the direction of magnetization in space as giving a mapping of the space on to the surface of a unit sphere, the invariant N measures the number of times space encloses the unit sphere. This invariant is of no significance in statistical mechanics because the energy barrier separating configurations with different values of N is of order unity. Thus, there is no barrier between different topological sectors (different values of N) which implies that there is no ordered state for the 2D $n = 3$ Heisenberg magnet. In the 2D planar rotor model, there is an infinite energy barrier between different topological sectors parametrized by n_x and n_y and, in consequence, there is a phase transition when the system can fluctuate between different topological sectors.

We can show this by showing how a configuration with $N = 1$ can be continuously deformed into one with $N = 0$. A simple example of an $N = 1$ configuration is one where θ is a continuous function of $r = \sqrt{x^2 + y^2}$ and $\theta = \pi$ for $r > a$ and $\theta(r = 0) = 0$. The angle $\phi(x, y) = \tan^{-1}(y/x)$. The energy of a slowly varying configuration is

$$E = \frac{Js^2}{2}\int dxdy[(\nabla\theta)^2 + \sin^2\theta(\nabla\phi)^2]$$
$$= \pi J s^2 \int_0^a \left[\left(\frac{d\theta}{dr}\right)^2 + \frac{\sin^2\theta}{r^2}\right] r dr$$

for this configuration. Even if θ varies linearly between $r = 0$ and $r = a$, E is finite and independent of a. Of course, for small values of a this expression for the energy is invalid, but the number of spins in the disk of radius a is small so that any energy barrier is also small and the topological invariant N can be changed by small thermal fluctuations. The conclusion is that the 2D planar rotor and related models can have a finite temperature topologically ordered state while the three component Heisenberg model does not. This is consistent with numerical studies (Moore, 1969), a later renormalization group study by Polyakov (1975) and with experiments on superfluids (Chester, Yang, and Stephens, 1972; Chester and Yang, 1973). Note that the calculation by Polyakov is performed in a single topological sector $N = 0$ so that the absence of a phase transition in the 2D Heisenberg model is verified by both arguments separately.

III. VORTICES IN THE PLANAR ROTOR MODEL IN TWO DIMENSIONS

The importance of topological defects in phase transitions in these two dimensional systems was discussed in our 1972 paper (Kosterlitz and Thouless, 1972) where our defect free energy argument was presented. The planar rotor and the superfluid film free energy can be written as

$$\frac{H}{k_B T} = \frac{K_0(T)}{2}\int \frac{d^2\mathbf{r}}{a_0^2}[\nabla\theta(\mathbf{r})^2]$$

where a_0 is the lattice spacing or some microscopic cutoff length scale and

$$K_0(T) = \begin{cases} \frac{J}{k_B T} & \text{planar rotor} \\ \frac{\hbar^2 \rho_s^0(T)}{m^2 k_B T} & \text{superfluid film.} \end{cases}$$

Here, J is the exchange interaction between nearest neighbor unit length spins so that

$$H[\{\mathbf{s}\}] = \frac{J}{2}\sum_{\langle \mathbf{r},\mathbf{r}'\rangle}[\mathbf{s}(\mathbf{r}) - \mathbf{s}(\mathbf{r}')]^2 = J\sum_{\langle \mathbf{r},\mathbf{r}'\rangle}[1-\cos(\theta(\mathbf{r})-\theta(\mathbf{r}'))].$$

For a ^{4}He film,

$$H = \frac{1}{2}\int \frac{d^2\mathbf{r}}{a_0^2}\rho_s^0(T,\mathbf{r})\mathbf{v}_s^2,$$

where $\mathbf{v}_s = (\hbar/m)\nabla\theta$ is the superfluid velocity, $\theta(\mathbf{r})$ is the phase of the superfluid order parameter $\psi(\mathbf{r}) = |\psi(\mathbf{r})|e^{i\theta(\mathbf{r})}$ and $\rho_s^0(T,\mathbf{r})$ is the position dependent bare superfluid density. $\rho_s^0(T,\mathbf{r}) = 0$ at the vortex cores and constant elsewhere. It turns out that its exact spatial dependence is irrelevant as the only important consequence is that there is a finite energy E_c associated with each vortex core. The physical reason is that a vortex core costs a finite free energy because the vortex core is a region where the superfluid order parameter vanishes. Now we can see how the topology arises—each vortex corresponds to a hole in the surface and the superfluid lives on the 2D surface with a set of holes where $\oint_C d\theta = 2\pi n$ and a vortex can be called a topological defect.

Since vortices interact pairwise by a logarithmic energy

$$\frac{H}{k_B T} = -\pi K_0(T)\sum_{\mathbf{R},\mathbf{R}'} n(\mathbf{R})n(\mathbf{R}')\ln\left(\frac{|\mathbf{R}-\mathbf{R}'|}{a}\right) - \ln y_0 \sum_{\mathbf{R}} n^2(\mathbf{R}),$$

the Hamiltonian is exactly that of a neutral plasma of Coulomb charges. Also, one can restrict consideration to the lowest charges $n = 0, \pm 1$ since the larger values are suppressed by powers of the fugacity $y_0 = e^{-E_c/k_B T} \ll 1$. Our first attempt at solving this was to consider a single isolated vortex of unit circulation in a $L \times L$ system. The free energy of such a vortex is $\Delta F = \Delta E - T\Delta S = k_B T[\pi K_0(T) - 2]\ln(L/a)$ since $\Delta E/(k_B T) = \pi K_0(T)\ln L/a$ and the entropy $\Delta S = k_B \ln L^2/a^2$. Now, at low temperature T, $2k_B T < \pi J$, $\Delta F \to +\infty$ and the probability of having a vortex $P \propto e^{-\Delta F/k_B T} \to 0$ while for $2k_B T > \pi J$, $P \to 1$ and there will be a finite concentration of free vortices. David and I realized that we could treat the Coulomb plasma of n charges $q = +1$ and n $q = -1$ charges by introducing a scale dependent dielectric function $\epsilon(r)$ such that the force between a pair of test charges separated by a distance r is $2\pi K_0/r\epsilon(r) = 2\pi K(r)/r$. The energy of this pair is

$$E(r) = \int_a^r dr' \frac{K(r')}{r'} = U(r)\ln(r/a).$$

Our self-consistent equation for $K(r = e^l)$ becomes

$$K^{-1}(l) = K^{-1}(0) + 4\pi^3 y_0^2 \int_0^l dl' e^{4l' - 2\pi U(l')}.$$

Kosterlitz and Thouless (1972) derived this self-consistent integral equation for the effective interaction energy (Kosterlitz and Thouless, 1973).

The central problem is to solve this equation since it is clear that a transition between a phase of bound dipoles and a phase of free charges will happen when $\pi K(\infty) = 2$. However, to find the behavior of the system near T_c requires solving the self-consistent equation for $K(l)$. Unfortunately, KT made an unnecessary approximation by replacing $U(r)$ by $K(r)$ and solving self-consistently for $K(r)$. The approximation was justified on the grounds that $U(r) - K(r) \ll 1$ but this led to incorrect results. A proper treatment has been given by Young (1978) who showed that this is equivalent to the renormalization group equations of Kosterlitz (1974):

$$\frac{dK^{-1}}{dl} = 4\pi^3 y^2 + \mathcal{O}(y^4),$$
$$\frac{dy}{dl} = (2 - \pi K)y + \mathcal{O}(y^3). \quad (1)$$

Remarkably, these *approximate* RG equations to lowest order in the vortex fugacity y yield an *exact*, inescapable prediction for an experimentally measurable quantity. The flows are shown in Fig. 2. If the experimental number is different from the theoretical prediction then, either the experiment is wrong or the whole theory is wrong. To our great relief and pleasure the key experiment by Bishop and Reppy was done in 1978 (Bishop and Reppy, 1978).

The theoretical prediction (Nelson and Kosterlitz, 1977)

$$\frac{\rho_s^R(T_c^-)}{T_c} = \frac{2m^2 k_B}{\pi\hbar^2} = 3.491 \times 10^{-8}\ \text{g cm}^{-2}\,\text{K}^{-1}$$

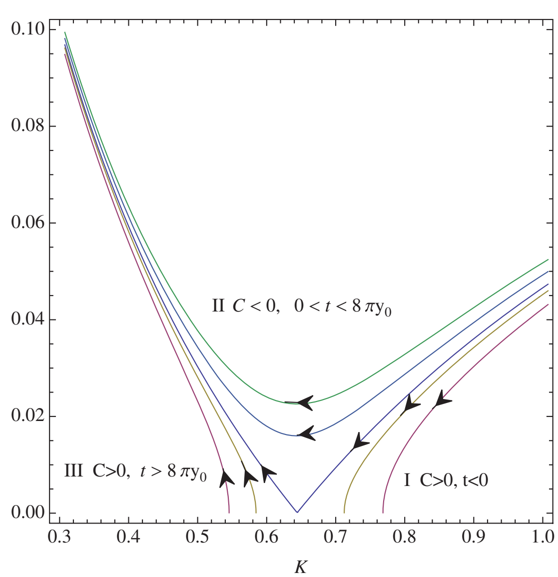

FIG. 2. Renormalization group flows from Eq. (1) for the 2D planar rotor model. Note that for $T \le T_c$, $y(\infty) = 0$ and $K_c^{-1}(\infty) = \pi/2$.

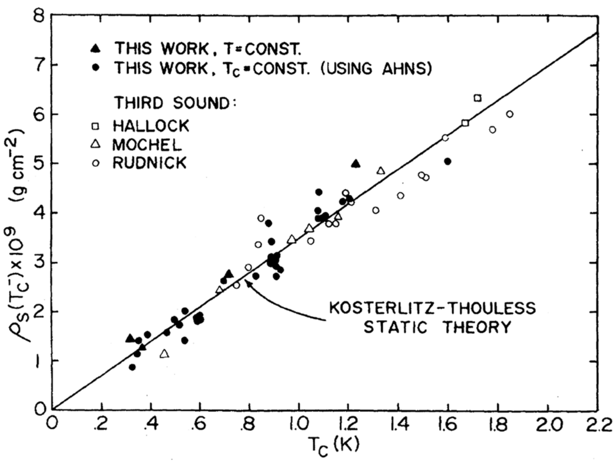

FIG. 3. Results of third sound and torsional oscillator experiments for the superfluid density discontinuity $\rho_s(T_c^-)$ as a function of temperature. The solid line is the theoretical prediction for the static theory. From Bishop and Reppy, 1978.

has been checked experimentally (Bishop and Reppy, 1978, 1980) and the data from several different experiments (Rudnick, 1978; Rutledge, McMillan, and Mochel, 1978; Buck and Mochel, 1981; Maps and Hallock, 1981, 1983) are presented in Fig. 3. It is of interest to note that the experimental data were obtained and plotted before the authors were aware of our theoretical prediction. This can be viewed as experimental confirmation of the BKT theory. There has also been extensive experimental investigation into melting in 2D by the Maret group (von Grünberg, Keim, and Maret, 2007; Gasser *et al.*, 2010).

IV. MELTING OF TWO DIMENSIONAL CRYSTALS

There is quantitative agreement with the theory of melting by topological defects due to Young, Halperin and Nelson (Halperin and Nelson, 1978, 1979; Young, 1979). In the theory of melting of 2D crystals, one starts with the expression for linear elasticity of a triangular lattice, which is the usual lattice structure in 2D:

$$F = \frac{1}{2}\int d^2\mathbf{r}(2\mu_0 u_{ij}^2 + \lambda u_{kk}^2),$$
$$u_{ij} = \frac{1}{2}\left(\frac{\partial u_i}{\partial r_j} + \frac{\partial u_j}{\partial r_i}\right),$$

where u_{ij} is the linear elastic strain tensor and u_i is the displacement field. The strain field can be decomposed into a smooth part ϕ_{ij} and a singular part $u_{ij}^s(\mathbf{r})$ due to dislocations (Nabarro, 1967). These are characterized by the integral of the displacement $\mathbf{u}(\mathbf{r})$ around a contour enclosing a topological defect or dislocation

$$\oint_C d\mathbf{u} = a_0\mathbf{b}(\mathbf{r}) = a_0[n(\mathbf{r})\hat{\mathbf{e}}_1 + m(\mathbf{r})\hat{\mathbf{e}}_2].$$

Here, $\mathbf{b}(\mathbf{r})$ is the dimensionless Burgers vector, a_0 is the crystal lattice spacing and n, m are integers. Within continuum elasticity theory, one can show that (Halperin and Nelson, 1978, 1979)

$$u_{ij}^s(\mathbf{r}) = \frac{1}{2}\left(\frac{1}{2\mu}\epsilon_{ik}\epsilon_{jl}\frac{\partial^2}{\partial r_k \partial r_l} - \frac{\lambda}{4\mu(\lambda+\mu)}\delta_{ij}\nabla^2\right) \times a_0\sum_{\mathbf{r}'} b_m G_m(\mathbf{r},\mathbf{r}'),$$
$$G_m(\mathbf{r},\mathbf{r}') = -\frac{K_0}{4\pi}\sum_{n=1}^{2}\epsilon_{nm}(r_n - r_n')\left[\ln\left(\left|\frac{\mathbf{r}-\mathbf{r}'}{a}\right|\right) + C\right].$$

From this, one obtains the energy of a set of dislocations of Burgers vectors $\mathbf{b}(\mathbf{r})$ as

$$\frac{H_D}{k_B T} = -\frac{K_0(T)}{8\pi}\int d\mathbf{r}d\mathbf{r}'\left(\mathbf{b}(\mathbf{r})\cdot\mathbf{b}(\mathbf{r}')\ln\frac{|\mathbf{r}-\mathbf{r}'|}{a} - \frac{\mathbf{b}(\mathbf{r})\cdot(\mathbf{r}-\mathbf{r}')\mathbf{b}(\mathbf{r}')\cdot(\mathbf{r}-\mathbf{r}')}{(\mathbf{r}-\mathbf{r}')^2}\right).$$

In our paper, we ignored the second term in this equation on the grounds that it is less relevant than the logarithmic term, which was an unfortunate error. This was corrected by Halperin and Nelson who predicted the now famous hexatic fluid phase with sixfold orientational symmetry. We assumed that dislocation unbinding led directly to an isotropic fluid which is now known to be wrong. Melting in 2D is a two stage process. At temperature T_m, the crystal melts by dislocation unbinding to an anisotropic hexatic fluid and, at $T_i > T_m$, this undergoes a transition where the algebraic orientational order is destroyed by disclination unbinding, resulting in the expected high temperature isotropic fluid (Halperin and Nelson, 1978, 1979; Young, 1979).

The predictions from this theory are similar to those for superfluid ^{4}He films and the corresponding universal jump is for the renormalized (measured) Young's modulus

$$\tilde{K}_R(T_m^-) = \lim_{T\to T_m^-}\frac{4\tilde{\mu}_R(T)[\tilde{\mu}_R(T)+\tilde{\lambda}_R(T)]}{2\tilde{\mu}_R(T)+\tilde{\lambda}_R(T)} = 16\pi,$$

where $\tilde{\mu}_R(T)$ is the renormalized value of $\mu/k_B T$. One of the interesting but unmeasurable predictions of the dislocation theory is the x-ray structure function

$$S(\mathbf{q}) = \langle|\rho(\mathbf{q})|^2\rangle = \sum_{\mathbf{r}} e^{i\mathbf{q}\cdot\mathbf{r}}\langle e^{i\mathbf{q}\cdot[\mathbf{u}(\mathbf{r})-\mathbf{u}(0)]}\rangle \sim |\mathbf{q}-\mathbf{G}|^{-2+\eta_G(T)},$$
$$\eta_G(T) = \frac{k_B T|\mathbf{G}|^2}{4\pi}\frac{3\mu_R(T)+\lambda_R(T)}{\mu_R(T)[2\mu_R(T)+\lambda_R(T)]}.$$

There are no δ-function Bragg peaks in the structure function but algebraic peaks behaving as

$$S(\mathbf{q}) \sim |\mathbf{q}-\mathbf{G}|^{-2+\eta_G(T)}.$$

We see that this diverges at $\mathbf{q} = \mathbf{G}$ for small $|\mathbf{G}|$ so that the expected x-ray structure function looks like that sketched in Fig. 4. This is one of the characteristic predictions of the dislocation theory of melting but, unfortunately, it is not

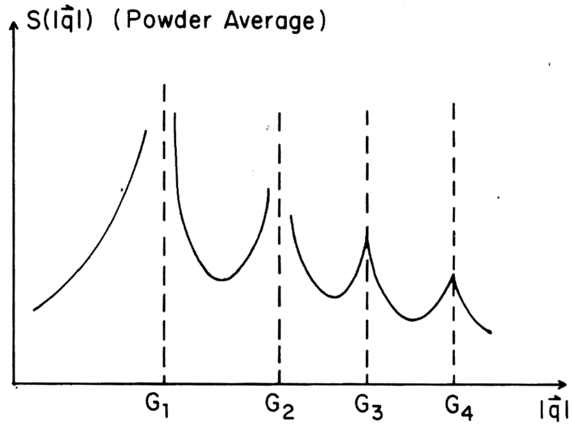

FIG. 4. A schematic sketch of the structure function $S(\mathbf{q})$ of a 2D crystal. For $T \leq T_m$, peaks for small $\mathbf{G}$ diverge as $|\mathbf{q}-\mathbf{G}|^{-2+\eta_G}$ but for larger $\mathbf{G}$ are finite cusps. For $T > T_m$, all peaks are finite with maximum height $\sim\xi_+(T)^{2-\eta_G}$. From Nelson, 2002.

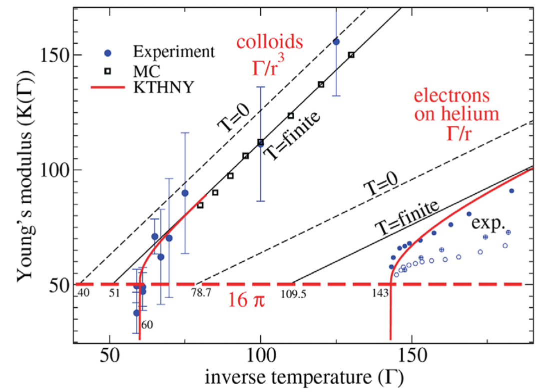

FIG. 5. Young's modulus $K_R(T)$ as a function of the effective inverse temperature Γ. The solid line is the dislocation theory prediction of Halperin and Nelson and the symbols are the experimental points. From Zanghellini, Keim, and von Grünberg, 2005.

measurable by experiment because the accessible system size and quality are not yet sufficient.

One of the main measurable predictions of the dislocation theory of melting is the renormalized (measured) Young's modulus for which there is remarkable agreement between experiment and theory as shown in Fig. 5. Although the theoretical predictions were made in the 1970s (Halperin and Nelson, 1978, 1979; Young, 1979), experimental measurements (Zanghellini, Keim, and von Grünberg, 2005; von Grünberg, Keim, and Maret, 2007) were not done for several decades because of the difficulties of realizing a suitable experimental system. In general, these 2D systems are extremely sensitive to perturbations due to the supporting substrate and the theory assumes no substrate effects.

In our original papers, we did consider melting of a crystal by dislocations but we did not discuss the fluid phase described by a periodic lattice with a fine concentration of free dislocations. A periodic solid has *two* types of order—translational order and orientational order describing the orientation of the crystal axes. These order parameters are the density $\rho(\mathbf{r})$ and the orientational order parameter $\psi_6(\mathbf{r}) = e^{6i\theta(\mathbf{r})}$:

$$\rho(\mathbf{r}) = \rho_0(\mathbf{r}) + \sum_{\mathbf{G}} |\rho_G(\mathbf{r})| e^{i\mathbf{G}\cdot\mathbf{u}(\mathbf{r})},$$
$$\psi_6(\mathbf{r}) = e^{6i\theta(\mathbf{r})}.$$

The topological defects are (i) dislocations which are responsible for the melting of the solid to an orientationally ordered hexatic fluid, and (ii) disclinations (vortices) responsible for the transition to a high temperature isotropic fluid (Halperin and Nelson, 1978, 1979; Young, 1979).

The theory has been worked out by Young (1979) and Halperin and Nelson (1978, 1979) with very detailed predictions which have been confirmed by experiment (Zanghellini, Keim, and von Grünberg, 2005; von Grünberg, Keim, and Maret, 2007) and summarized in Fig. 5. One of the most sensitive tests of the theory to date is the numerical simulations by Kapfer and Krauth (2015) who performed large scale simulations on up to 10^6 particles interacting by $V(r) = \epsilon(\sigma/r)^n$ repulsive potentials. They found that melting does proceed via the Kosterlitz-Thouless-Halperin-Nelson-Young (KTHNY) scenario with an intermediate hexatic fluid for long range $(n < 6)$ potentials, which includes the colloid experiments with $n = 3$ (Zanghellini, Keim, and von Grünberg, 2005; von Grünberg, Keim, and Maret, 2007) and the electrons on the surface of ^{4}He (Grimes and Adams, 1979) $n = 1$ while for $n > 6$, the hexatic-isotropic transition becomes first order, which agrees with the hard disk $(n = \infty)$ simulations. Note that these simulations are on larger systems than the experimental ones.

BKT theory has also been applied to superconductivity in thin films. In our original paper, we stated that true superconductivity in a 2D superconducting film could not exist because of the finite penetration depth $\lambda(T)$ which limits the range of the logarithmic interaction between vortices. For separations $r > \lambda(T)$, the vortex-vortex interaction behaves as $1/r$ so that the vortices are always free at any $T > 0$ thus destroying superconductivity. Although our argument is correct, in many thin film superconductors, the penetration depth can be $\mathcal{O}(1\ \mathrm{cm})$ which is a typical system size. For the small applied currents used, this is so large that its effects are smaller than that of the finite currents or the finite frequencies so that the behavior of the system is indistinguishable from that of the $\lambda = \infty$ limit (Resnick *et al.*, 1981; Hebard and Fiory, 1983). The theory has also been applied to 2D layers of cold atoms (Hadzibabic *et al.*, 2006; Holzmann *et al.*, 2007) with reasonable agreement which may be improved in the future.

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