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History of the Lenz-Ising Model 1920–1950: From Ferromagnetic to Cooperative Phenomena

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Abstract

I chart the considerable changes in the status and conception of the Lenz-Ising model from 1920 to 1950 in terms of three phases: In the early 1920s, Lenz and Ising introduced the model in the field of ferromagnetism. Based on an exact derivation, Ising concluded that it is incapable of displaying ferromagnetic behavior, a result he erroneously extended to three dimensions. In the next phase, Lenz and Ising's contemporaries rejected the model as a representation of ferromagnetic materials because of its conflict with the new quantum mechanics. In the third phase, from the early 1930s to the early 1940s, the model was revived as a model of cooperative phenomena. I provide more detail on this history than the earlier accounts of Brush (1967) and Hoddeson, Schubert, Heims, and Baym (1992) and question some of their conclusions. Moreover, my account differs from these in its focus on the development of the model in its capacity as a *model*. It examines three aspects of this development: (1) the attitudes on the degree of physical realism of the Lenz-Ising model in its representation of physical phenomena; (2) the various reasons for studying and using it; and (3) the effect of the change in its theoretical basis during the transition from the old to the new quantum mechanics. A major theme of my study is that even though the Lenz-Ising model is not fully realistic, it is more useful than more realistic models because of its mathematical tractability. I argue that this point of view, important for the modern conception of the model, is novel and that its emergence, while perhaps not a consequence of its study, is coincident with the third phase of its development.

Introduction

Judging by the number of research papers published on the Lenz-Ising model since 1940, it is one of the most studied models in modern physics. Its greatest success during the last half century has been in the study of phase transitions,[1] but it also has been applied to a wide range of physical phenomena. It is discussed today in virtually every

[1] See, for example, Domb (1996).

modern textbook on statistical mechanics. Because of its ubiquity and importance in modern physics, a historical study of its origin and development is merited.

More generally, a historical study of the Lenz-Ising model will shed light on the use of models in physics. As in all aspects of science, opinions concerning a particular model change both with time and across the scientific community. Some may think that a given model is a useful and realistic representation of a physical phenomenon, while others may reject it because, for instance, it is at odds with fundamental theories. The history of the Lenz-Ising model presents a good opportunity to study such changes in attitude towards a model. It can be viewed as a characteristic example of a certain class of physical models, simple statistical-mechanical models, which also include such models as the Heisenberg model, the Potts model, and the spherical model, all of which play important roles in modern physics. The Lenz-Ising model thus is not a marginal "pathological" case. Moreover, as its theoretical basis changed with the emergence of the new quantum mechanics, it underwent great changes with respect to the phenomena it represents, its physical realism, and the insights it provided.

One particular aspect of the history of the Lenz-Ising model stands out and is related to the primary reason for its ubiquity in modern physics: It strikes a balance between physical nonrealism and realism. That is to say, the Lenz-Ising model usually is considered to be a very crude model of the phenomena it is assumed to represent, but at the same time it is able to capture some of their essential features. Its simplicity, moreover, makes it amenable to mathematical treatment (at least in one and two dimensions). Thus, Kerson Huang remarked in 1963:

> The Ising model is a crude attempt to simulate the structure of a physical ferromagnetic substance .... Its main virtue lies in the fact that a two-dimensional Ising model yields to an exact treatment in statistical mechanics. It is the only nontrivial example of a phase transition that can be worked out mathematically.[2]

This emphasis on its mathematical tractability at the expense of its physical realism seems to be characteristic of statistical-mechanical models after World War II. As Mark Kac noted in 1971:

> Since the detailed nature of interactions in real physical systems is usually not known and since even the most far reaching implications still lead to enormous mathematical difficulties we must, in our attempts to understand phase transitions, rely upon a narrow class of models which are balanced (precariously!) between realism and solubility.[3]

This attitude toward statistical-mechanical models of phase transitions, including the Lenz-Ising model, seems to have emerged around World War II and since then has become widespread among physicists. I will trace the emergence of this new attitude towards physical models between 1920 and 1950 in the special case of the Lenz-Ising model.

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[2] Huang (1963), p. 329.
[3] Kac (1971), p. 23.

Ernst Ising (1900–1998) and the history of the Lenz-Ising model have been subjects of earlier studies.[4] Stephen G. Brush (1967),[5] in a seminal article, traced the history of the Lenz-Ising model between 1920 and 1967, concentrating on its *mathematical* results. Hoddeson, Schubert, Heims, and Baym (1992), in their history of it in the context of collective phenomena,[6] discussed its *physical* aspects, but only briefly. I will give a more detailed account of its early history and focus on its evolution as a *model* by charting its development with respect to three of its aspects: (1) its degree of physical realism, that is, how faithfully physicists believed that it represented physical phenomena; (2) the reasons that physicists investigated and used it; and (3) the changes in its theoretical basis following the creation of the new quantum mechanics in 1925. I will assume no prior knowledge of its history and will not discuss its mathematical aspects, since they have been treated elsewhere[7] and play a minor role in my account.

Regarding terminology, I use the term "realism" in connection with a model to represent the extent of its agreement with a physical phenomenon. One way to measure this agreement is to consider its degree of idealization, where I subscribe to Margaret Morrison's definition:

> An idealisation is a characterisation of a system or entitiy where its properties are deliberately distorted in a way that makes them incapable of accurately describing the physical world. By contrast, an abstraction is a representation that does not include all of the systems [*sic*] properties, leaving out features that the systems [*sic*] has in its concrete form. An example of the former is the electron as a point particle and the latter is the omission of intermolecular forces from the ideal gas.[8]

By this definition, the less idealizations of a model, the higher its degree of realism. I believe that this definition is close to physicists' use of the term, for example, by Kac above.

I also must remark on the appellation, the Lenz-Ising model. Physicists almost exclusively call it the Ising model, even though Lenz contributed significantly in proposing it. Brush (1967), however, followed Ising's own recommendation and called it the Lenz-Ising model, but had little success in changing its name among physicists. Mathematical physicists McCoy and Wu (1973), for instance, stuck doggedly to calling it the Ising model in their monograph on its two-dimensional variant, arguing that Lenz, in contrast to Ising, had not provided any computations. They admittedly acknowledge Lenz's work but to me Lenz's role in proposing the model seems at least as important as Ising's relatively straightforward calculations, so I regard Brush's name for it as more appropriate and accordingly will use it. Finally, I use the term "*n* dimensions" in the so-called *n*-dimensional Lenz-Ising model to refer to the dimensionality of the lattice on which the

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[4] Biographical data of Ising can be found in Stutz and Williams (1999), Kobe (1997), and Kobe (2000). Ising changed his first name to Ernest when he became an American citizen in 1953.

[5] See also Brush (1983), which, however, does not contain new material about the model.

[6] Their study appears as a chapter in Hoddeson, Braun, Teichmann, and Weart (1992), a pioneering book on the history of solid-state physics.

[7] Brush (1967).

[8] Morrison (1999), p. 38.

model "lives." When I specify no dimension, I assume that the dimension of the lattice is arbitrary, in accordance with the terminology accepted today.

I see roughly speaking three phases in the development of the Lenz-Ising model. in the first phase, Lenz introduced one of the two basic assumptions of the model in 1920 and his student Ising introduced the other one in 1924. They proposed their model to give a theoretically more satisfactory account of experimental results for paramagnetism and ferromagnetism than Pierre Weiss had provided with his theory of magnetism, the dominant theory at that time. Lenz believed, as did Weiss, that magnetic materials consist of elementary micromagnets placed in a regular array, but Lenz, in contrast to Weiss, allowed each of these micromagnets to point in either of only two directions. Ising then carried out calculations on Lenz's model by assuming only nearest-neighbor interactions between the micromagnets, finding that it did not display ferromagnetic behavior in one dimension. He concluded erroneously that this result also was valid in three dimensions.

During the second phase, from 1925 to 1936, the validity of Ising's conclusion for the three-dimensional case was a subject of controversy. Moreover, the Lenz-Ising model was dismissed as a model of ferromagnetism, because it was incompatible with Heisenberg's theory of ferromagnetism. It was studied, however, as a simple approximation to Heisenberg's model. Then, in 1936, Rudolf Peierls showed that it does exhibit ferromagnetism in two dimensions. Further, a mathematically equivalent model also was investigated in other areas, such as in the theory of binary alloys and gas adsorption.

In the third phase, after 1936, several physicists realized that the description of sharp transition points of so-called cooperative phenomena — phenomena arising in systems in which the cooperation of its constituent units is of fundamental importance — pose enormous mathematical problems.[9] The way around them was to study very simple models of cooperative phenomena in which the emphasis was on mathematical tractability rather than on physical realism. For example, H.A. Kramers and G.H. Wannier used the Lenz-Ising model in their theory of ferromagnetism even though they recognized that it was an unrealistic representation of ferromagnetism. They conjectured that the specific heat as a function of temperature for the two-dimensional model shows a singularity when the temperature approaches a certain value. Lars Onsager, in an attempt to understand transitions with no release of latent heat, proved their conjecture in 1944. The work of Kramers and Wannier and Onsager precipated a flood of publications on the Lenz-Ising model whose focus was on the general properties of transition points in a wide range of physical systems rather than on the particular features of a given system.

Physics preliminaries

Most physicists today view the Lenz-Ising model as a mathematical structure that can represent a variety of different physical phenomena. For concreteness, I will define the model in this section in *modern* ferromagnetic parlance. Thus, the model lives on a

[9] Cooperative phenomena are not the same as what was termed critical phenomena in the 1960s. Critical phenomena are cooperative phenomena, but the latter class includes phenomena that do not exhibit critical behavior.

lattice and a particle is assigned to each lattice site. The particle has an intrinsic angular momentum, its spin σ, which is restricted to point either up or down (and is typically represented by an arrow). A configuration of the model is a specification of the spin for each lattice site; for a two-dimensional lattice with, say, $N \times M$ lattice sites, there are 2^{NM} possible configurations. For example, one configuration for 6×4 lattice sites can be visualized as follows:

$$\begin{matrix} \uparrow & \downarrow & \downarrow & \uparrow \\ \uparrow & \downarrow & \uparrow & \downarrow \\ \downarrow & \uparrow & \uparrow & \downarrow \\ \uparrow & \uparrow & \downarrow & \uparrow \\ \uparrow & \uparrow & \downarrow & \uparrow \\ \uparrow & \downarrow & \uparrow & \downarrow \end{matrix}$$

Only particles that are nearest neighbors interact, that is, they have to be connected by exactly one bond on the lattice. If the spins of two nearest neighbors in the configuration have the same orientation (both either up or down), then this pair contributes an energy $-J$ to the total energy. If they have opposite orientations, they contribute an energy J, the so-called *interaction* energy, which is the same for all pairs and is assumed to be positive for a ferromagnet (if J is negative, the model describes an antiferromagnet). Each particle (electron) has a magnetic moment μ owing to its spin that couples with an external magnetic field of strength H, so that the energy associated with this coupling is $\mu H \sigma_j$ for site j. The total energy E_i of a configuration i is thus given by

$$E_i = -\sum_{<n,m>} J\sigma_n\sigma_m + \sum_j \mu H \sigma_j, \tag{1}$$

where the first sum is over all pairs of nearest neighbors and the second is over all lattice sites.

Suppose now that the energy of each configuration has been determined. According to a fundamental assumption of equilibrium statistical mechanics, the probability P_i that the system is in the configuration i with energy E_i is determined by this energy and is given by the so-called Boltzmann factor:

$$P_i = Z^{-1} \exp\left(-\frac{E_i}{kT}\right), \tag{2}$$

where k is Boltzmann's constant, T is the (constant) absolute temperature, and Z is the partition function; since the sum of the probabilities for all states has to equal 1

$$Z = \sum_i \exp\left(-\frac{E_i}{kT}\right). \tag{3}$$

Thus, since the total energy E_i depends on the external magnetic field H and the interaction energy J, the partition function Z depends on these quantities, as well as on the absolute temperature T.

In principle, equations (1–3) suffice to determine all of the macroscopic properties of the system. For instance, the average energy is given by

$$E = \sum_i E_i P_i. \tag{4}$$

Other quantities can be computed by taking suitable derivatives of the partition function Z. For instance, the magnetization M is defined by

$$M = kT \frac{1}{N} \frac{\partial \ln Z}{\partial H}. \tag{5}$$

In the Lenz-Ising model, this definition can be shown to be equivalent to another procedure that may appear to be more natural: For every configuration, compute the quantity

$$\frac{1}{N} \sum_i \mu \sigma_i, \tag{6}$$

and then average this quantity over all possible configurations; the result is the magnetization M.

A quantity of particular interest is the average magnetization M_0 as a function of the absolute temperature when the external field tends to zero:

$$M_0(T) = \lim_{H \to 0} M. \tag{7}$$

If $M_0(T)$ is nonzero, then the Lenz-Ising model is said to have a spontaneous magnetization and to be ferromagnetic at T, because this is analogous to the behavior of real ferromagnets. Historically, an important issue was whether or not a temperature T exists in the Lenz-Ising model (as well as other models of ferromagnetism) that mimicks the Curie temperature of real ferromagnets, which marks a transition from a low-temperature, ferromagnetic regime to a high-temperature, paramagnetic (nonferromagnetic) regime. Such a transition, which in mathematical terms is a singularity, can occur only in the thermodynamic limit in which the number of lattice sites tends to infinity. If the dimension of the lattice is greater than one, the computation of the partition function Z presents formidable mathematical difficulties in this thermodynamic limit. For the Lenz-Ising model, this computation has been done only for one and two dimensions, and in the latter case only for zero magnetic field. Strenuous efforts to extend these results have so far proven to be futile.[10]

Models and theories of magnetism prior to Lenz and Ising

Lenz and Ising's purpose in introducing their model was to reproduce the properties of paramagnetic and ferromagnetic solids by applying statistical mechanics to micromagnets. In this they relied on a long tradition of making such microscopic models (sometimes called theories) of magnetism, which I will summarize below, focusing on their microscopic, statistical, and quantum-mechanical aspects. First, however, I note a crucial experimental result that the French physicist Pierre Curie (1859–1906) reported in 1895.[11] Curie singled out three classes of magnetic substances, which today are called

[10] Cipra (2000). In fact, Cipra reports that it has been proven that the solution of the three-dimensional model in zero field is NP-complete.

[11] Curie's work is described in Keith and Quedec (1992), which contains more details than I give here.

diamagnetic, ferromagnetic, and paramagnetic substances and reported what is today called the *Curie law*, that the magnetic susceptibility χ_T of a paramagnetic substance varies inversely with the absolute temperature T:

$$\chi_T \equiv \left(\frac{\partial M}{\partial H}\right)_T \propto 1/T, \tag{8}$$

where as before M is the magnetization and H is the strength of the external magnetic field.

Eckert, Schubert, and Torkar (1992, pp. 23–24) ascribe the emergence of microscopic models of magnetism in the second half of the nineteenth century to the earlier enunciation of the concept of the atom together with the long-established knowledge that fragments of a magnet are also magnetic. This led to the view that magnetic matter consists of a number of tiny magnetic needles, each of which represents an atom or molecule with a magnetic moment. Wilhelm Weber (1804–1891) put forward in 1852 the first important model based on this idea; he assumed that each atom or molecule is able to rotate freely around an axis.[12]

Keith and Quedec (1992) claim that Pierre Curie's student Paul Langevin (1872–1946) was the first to apply Boltzmann's statistical ideas in the field of magnetism in his theories of diamagnetism and paramagnetism of 1905. I will only discuss Langevin's theory of paramagnetism, drawing on his original paper and the account of it in Mehra and Rechenberg (1928a). Langevin assumed that the total magnetic moment M of the molecular micromagnets arises from the revolution of electrons in them and then employed Boltzmann's theory to a gas of such molecules in an external magnetic field of strength H.[13] If W is the difference in potential energy of a molecule between any two points in the gas, and if its magnetic moment is at an angle α with the external magnetic field, he then found that $W = MHcos\alpha$ and the ratio of the density of the molecules between these two points is proportional to $e^{-W/kT}$ or

$$\exp\left(-\frac{MH\cos\alpha}{kT}\right). \tag{9}$$

Using this result, Langevin then obtained his famous epononymous equation for the intensity of magnetization

$$I = MN\left(\frac{Cha}{Sha} - \frac{1}{a}\right), \tag{10}$$

where $a = \frac{MH}{kT}$ and *Ch* and *Sh* are the hyperbolic cosine and sine functions, respectively.

[12] In a paper of 1890, Alfred Ewing described a "simulation" of Weber's model: Ewing built a large-scale version of Weber's model in two dimensions consisting of an array of concrete, macroscopic magnetic needles attached to a table. By placing this macroscopic physical model in a solenoid, Ewing could reproduce the essential features of the magnetization curve of iron; see Eckert, Schubert, Torkar (1992), pp. 24–25.

[13] Mehra and Rechenberg (1982a), pp. 423–424.

Pierre Weiss (1865–1940), another Frenchman who since 1902 was working at the Eidgenössische Technische Hochschule in Zurich,[14] made several important contributions to the theory of magnetism, including an extension of Langevin's theory of paramagnetism to ferromagnetism (still restricted to isotropic materials), and a theory of magnetism of crystals. I will discuss Weiss's latter theory later in connection with Lenz's work and for the moment only outline his former theory, drawing on the accounts by Mehra and Rechenberg (1982a) and Keith and Quedec (1992).

One of Weiss's most important ideas was his so-called "molecular-field hypothesis" (sometimes referred to as his "internal-field hypothesis"), which he defined as follows:

> I assume that each molecule experiences, from the collection of molecules surrounding it, a force equal to that of a uniform field proportional to the intensity of magnetization and in the same direction.[15]

The total field affecting a molecule, which I will designate as H_{total}, is the sum of this internal-field and the external field,

$$H_{total} = NI + H. \tag{11}$$

where N is the number of molecules and I is the intensity of magnetization. Weiss preserved Langevin's equation (10), but he replaced the external field H by H_{total}, and he concluded that if a molecular field is present, then there exists a characteristic temperature below which isotropic media exhibit a spontaneous magnetization, even in the absence of an external field H, and above which they do not. He later called this temperature the Curie point.

Weiss also was the first to apply quantum-theoretical considerations to the magnetic properties of matter,[16] which according to Mehra and Rechenberg (1982a, p. 424) initiated a new direction of research on paramagnetism. Thus, two Dutch physicists, E. Oosterhuis and W.H. Keesom, in 1913 and 1914, respectively, modified Langevin's classical theory by replacing the energy kT with the average quantum-mechanical rotational energy of the molecular magnets, which enabled them to account for some of the discrepancies between Langevin's classical theory and experiments. Their work stimulated that of others, for example, that of Jan von Weyssenhoff and Fritz Reiche, who by means of a number of refinements were able to achieve good agreement with experimental results for substances at still lower temperatures.[17]

Nevertheless, the German physicist Otto Stern (1888–1969) sharply criticized the basic assumptions underlying the above theories in 1920.[18] He pointed out that Weiss and his successors had applied Langevin's theory to gases at temperatures so low that they would likely have crystallized and hence had implicitly assumed that the gas molecules, as the carriers of micromagnets, were as free to rotate in the solid phase as in the gaseous phase. This assumption, however, Stern went on, was clearly contradicted

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[14] Keith and Quedec (1992), p. 374.

[15] Weiss (1907), p. 662, translated in Keith and Quedec (1992), p. 376.

[16] The results were published in Weiss (1911).

[17] Mehra and Rechenberg (1982a), p. 424.

[18] Stern (1920); Mehra and Rechenberg (1982a), pp. 431–432.

by the anisotropy characteristic of crystalline structure. Weiss actually had anticipated this objection and had tried to circumvent it in a paper of 1913 where he had shown that Curie's law follows from the assumption that the molecules are bound to fixed equilibrium positions whose orientations are in no preferred directions. Stern, however, pointed out a flaw in Weiss's calculations and, by using Boltzmann's formalism, showed that Weiss's assumption led to a much weaker dependence of the magnetic susceptibility on temperature than exhibited by Curie's law. Moreover, Stern did not attach much importance to the apparent agreement between the theories of Weiss's successors and experiments because their theories embodied several adjustable parameters.

By 1920, in sum, there was general agreement among physicists that magnetic materials consist of elementary micromagnets, and that Boltzmann's statistical formalism was the right tool to use to describe their magnetic properties theoretically. Weiss's theories of magnetism, and their subsequent modifications by others, were able to reproduce at least some of the experimental results, but they also faced criticism. Weiss's molecular-field hypothesis did not seem to be satisfactory, despite its empirical success, because it lacked a satisfactory physical explanation. More generally, the physical origin of the magnetic interaction was of great concern at the time. Further, Weiss's approach required clarification because of his assumption of the free rotatability of the molecular magnets in solid materials, which therefore had to be modified, first according to Otto Stern and now according to Wilhelm Lenz.

Lenz's paper

Wilhelm Lenz (1888–1957) *promovierte* in 1911 under Arnold Sommerfeld at the University of Munich with a dissertation on electrodynamics (on the capacitance, resistance, and self-inductance of coils) and continued this work as Sommerfeld's assistant, completing his *Habilitationsschrift* in 1914 (on eigenoscillations in coils).[19] He was appointed as *ausserordentlicher Professor* at the University of Rostock in 1920, where he worked on various subjects (for example, proposing a theory of molecular band spectra[20]) before turning to the theory of magnetism.[21]

His contribution to the Lenz-Ising model was the only paper he published on microscopic theories of paramagnetism and ferromagnetism. This digression may have been inspired by Stern's paper, which he cited in his own paper. In fact, Lenz may have met Stern personally in Berlin in April 1920, since they both attended a lecture that Niels Bohr gave then at a meeting of the German Physical Society. Lenz presented his work five months later at the 86th *Naturforscherversammlung* in Bad Nauheim, which took place from September 19–25, 1920,[22] and published it later that year as a paper entitled "A Contribution to the Understanding of Magnetic Phenomena in Solid Materials."[23]

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[19] Sommerfeld (1948), p. 186, attested that the replacement of Debye with Lenz as his assistant in 1911 caused him no doubts.

[20] Sommerfeld (1948), p. 186.

[21] Mehra and Rechenberg (1982a), p. 334; Brush (1967), p. 885.

[22] Mehra and Rechenberg (1982a), p. 804.

[23] Unless stated otherwise all translations are my own.

To Lenz, the main magnetic phenomenon that demanded understanding was the Curie law of paramagnetism, which Heike Kamerlingh-Onnes (1853–1926) and Oosterhuis had confirmed experimentally for a number of salts at low temperatures. Lenz argued that because this law is so fundamental, by extension "the key to understanding the very complicated ferromagnetic phenomena ought to lie here,"[24] their most prominent aspects being the temperature dependencies of both the susceptibility and the spontaneous magnetization. Nevertheless, despite his general approach, he focussed mainly not on ferromagnetism but on paramagnetism, which he treated quantitatively. Most of his successors thus saw his paper as falling in the domain of paramagnetism,[25] with one notable exception, namely Arnold Sommerfeld, who remarked in a note written on the occasion of Lenz's 60th birthday that:

> Very early [in 1920] Lenz had pointed to the magnetic turnover processes in solid paramagnetic materials in order to explain the Curie law, with an outlook to ferromagnetism.[26]

Throughout his paper, in fact, Lenz stressed the connection between paramagnetism and ferromagnetism, and, as we shall see, he derived the fundamental assumptions of his theory from observations not of paramagnetic but of ferromagnetic materials, so he evidently considered the two types of magnetism to arise from the same basic mechanism. He did not express this idea explicitly, however.

As noted above, Lenz regarded Weiss's theories of paramagnetism and ferromagnetism as insufficient, because he endorsed Stern's criticism of them that the elementary magnets cannot rotate freely in a solid. Moreover, to Lenz the molecular-field (or "self-field") hypothesis, "offers only a purely phenomenological hint"[27] about how to the explain ferromagnetism. Nonetheless, he embraced some of Weiss's basic assumptions that magnetic materials contain small magnetic needles but began to rethink their ability to rotate:

> To obtain the Curie law, the assumption of free rotatability of the magnets thus seems indispensable; and since the crystal structure prohibits free rotation of the molecules, one might think of free rotatability of the atoms. However, this assumption, too, is incompatible with our conception of the crystal structure, since we, following Born, must assume that the symmetry of the crystals is already preformed according to the structure and spatial position of those atoms.[28]

What Lenz meant by "preformed" is not clear, but he may have meant that the atoms determine the structure and hence the symmetry of the crystal, which is consistent with the interpretation that Max Born gave in his influential monograph, *Dynamik der Kristallgitter*, of 1915,[29] where he claimed that atoms, not molecules, are the building blocks of crystal lattices. Further, he assumed that the chemical forces that keep the atoms in place

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[24] Lenz (1920), p. 613.
[25] See, for example, Ehrenfest (1921), p. 793; Kramers (1929), p. 25; and Kramers and Becquerel (1929), p. 49.
[26] Sommerfeld (1948), p. 186.
[27] Lenz (1920), p. 613.
[28] *Ibid.*, p. 614.
[29] Born (1915), pp. 1–2.

in a crystal are identical to those that act between atoms in liquids and gases. Lenz thus may have taken Born to mean that the atoms in a crystal are responsible for its spatial characteristics.

Proposing a new notion of free rotatability "adjusted to the crystal structure,"[30] Lenz cited experimental results on pyrrhotite and magnetite for temperatures where they are ferromagnetic, not paramagnetic. Weiss had concluded that the basic unit of pyrrhotite is a hexagonal prism that can be magnetized only in its magnetic plane, that is, its hexagonal base.[31] Lenz now declared:

> As is well known, it is a property of the crystal state of the minerals mentioned to be magnetized to spontaneous saturation. It is not in practice possible to change the magnetization of the sample to a noticeable degree by applying an external [magnetic] field, but the orientation of the magnetization can be changed, since the crystal symmetry of magnetite corresponds to a turnover [*ein Umklappen*] for each 90°, and for pyrrhotite a turnover for each 60° is observed . . . . Thus, for the orientation of the elementary magnets there are always several equal positions determined by the crystal symmetry . . . and one may assume in general that for every position at least its opposite is equal to it. Since free rotatability of the elementary magnets therefore has to be refuted, it can be concluded from the above that they have the ability to turn over. I want to show that this assumption is sufficient to explain the Curie law.[32]

A reasonable interpretation of Lenz's turnover argument is that the structure of the crystal singles out certain directions of its total magnetization, in the case of pyrrhotite 0°, 60°, 120°, 180°, 240° and 300°, corresponding to its axes of symmetry. Further, since the elementary magnets constitute its total magnetization, exactly these directions of the elementary magnets also are distinctive (note that Lenz is not saying that the elementary magnets can *only* occupy positions that correspond to the symmetry of the crystal). All of these orientations of the elementary magnets are equivalent, and since they cannot rotate freely, they must have the ability to turn over from one position to the next. Lenz did not comment on how rapidly these turnovers occur, but Ising, in summarizing Lenz's theory four years later in his thesis, wrote that they occur instantaneously.[33]

This turnover ability is the new assumption of Lenz's theory. Brush (1967) asserted that its physical basis was the old quantum theory developed mainly by Niels Bohr and Sommerfeld. I find it difficult to see, however, where the old quantum theory enters into Lenz's argument, because Lenz does not refer to it in describing the rotatability of the atoms in the crystals in question. Instead, his argument rested simply on Weiss's observations concerning their symmetry.

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[30] Lenz (1920), p. 614.
[31] Weiss (1905).
[32] Lenz (1920), p. 614.
[33] Ising (1924), p. 2.

To explain the Curie law, Lenz considered a bar magnet that was allowed to perform turnovers and was implicitly confined to a plane whose deflection from its equilibrium position was given by the angle α. Two directions were singled out because:

> In a quantum treatment certain angles α will be distinguished, among them in any case $\alpha = 0$ and $\alpha = \pi$. If the potential energy W has large values in the intermediate positions, as one must assume taking account of the crystal, then these positions will be very seldom occupied, Umklapp processes [turnovers[34]] will therefore occur very rarely, and the magnet will find itself almost exclusively in the two distinguished positions, and indeed on the average in each one equally long.[35]

The basis of Lenz's "quantum treatment" probably was the idea of space quantization as set forth in 1916 by his former supervisor and later colleague in Munich, Arnold Sommerfeld, according to which the vector normal to the orbit of an electron, which is proportional to its magnetic moment, is allowed to point only in certain discrete directions relatively to an external magnetic field.[36] Space quantization, however, was first confirmed experimentally by Stern and Walter Gerlach in 1921, that is, after Lenz published his paper, so it seems likely that he quickly accepted Sommerfeld's idea. This is corroborated in a letter that Ising wrote to Stephen G. Brush:

> At the time I wrote my doctor thesis [under Lenz] Stern and Gerlach were working in the same institute on their famous experiment on space quantization. The ideas we had at that time were that atoms or molecules of magnets had magnetic dipoles and that these dipoles had a limited number of orientations.[37]

In sum, Lenz justified his crucial assumption that the elementary magnets will turn over between two positions first by arguing that their free rotatability was incompatible with Born's theory of crystal structure; then by assuming that they can perform turnovers as suggested by experiments on ferromagnetic materials; and finally by concluding that in a quantum-theoretical treatment they would by and large occupy two distinct positions. His justification for his assumptions thus was based on a mixture of experimental and theoretical considerations, and the carefulness of his chain of argument indicates that he considered his assumption not just tentative, but as essentially correct.

Lenz pointed out that in his theory the time average of the magnetic moment vanishes when an external magnetic field is present and the directions corresponding to the angles α and $\pi - \alpha$ are assumed equally often. If, however, an external magnetic field of strength H is applied along the direction corresponding to the angle α, say, then the difference in potential energy corresponding to the two directions is given by μH where μ is the magnetic moment of the bar magnet. In that case, Lenz used Boltzmann's principle to show that the average magnetic moment $\bar{\mu}$ of the bar magnet is no longer zero but is given by:

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[34] I prefer the word "turnovers" because "Umklapp processes" have another meaning in solid-state physics.

[35] Lenz (1920), p. 614; translation follows Brush (1967).

[36] Mehra and Rechenberg (1982a), p. 435.

[37] Quoted in Brush (1967), p. 886; Ising's letter is in English.

$$\bar{\mu} = \frac{\mu(e^a - e^{-a})}{e^a + e^{-a}}, \tag{12}$$

where $a = \frac{\mu H}{kT}$.

For sufficiently small a, this becomes $\bar{\mu} = \frac{\mu^2 H}{kT}$, that is, the Curie law.

So far, the assumption has been that the elementary magnets do not interact, which is presumably the case for paramagnetic materials, but is not the case for ferromagnetic materials. Lenz did not specify a definite interaction in the latter case but concluded that:

> If one assumes that in the ferromagnetic bodies the potential energy of an atom (elementary magnet) with respect to its neighbors is different in the null position and in the π position, then there arises a natural directedness of the atom corresponding to the crystal state, and hence a spontaneous magnetization. The magnetic properties of ferromagnetics would then be explained in terms of nonmagnetic forces, in agreement with the viewpoint of Weiss . . . .[38]

Lenz, however, did not provide any calculations to support his optimism that this assumption would be sufficient to explain ferromagnetism.

Ising's thesis and paper

After Lenz moved from Rostock to Hamburg,[39] he assigned the calculational tasks on his model to his student Ising, who wrote up his results in his thesis of 1924[40] and in a paper of 1925. Since there is no essential difference between the results that Ising presented in his thesis and in his paper, and since the former is more elaborate than the latter, I will focus on the fomer and only occasionally digress to the latter. There is a significant discrepancy between the two regarding the interpretation of his results, however, to which I will return later.

Ising's assumption

Ising adopted Lenz's assumptions, but in addition employed a specific form of the interaction between the elementary magnets. At the same time, since Lenz was Ising's thesis advisor, one has to be careful to not draw rigid conclusions about the originator of the ideas in Ising's thesis simply because Ising published them. Indeed, as Ising wrote to S. Kobe: "I like to point out that the model really should be called [the] Lenz-Ising model. My supervisor, Dr. Wilhelm Lenz, had the idea and proposed that I make a mathematical workout as my dissertation. . . . "[41]As we will see later, the particular form

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[38] Lenz (1920), p. 615; translated by Brush (1967), pp. 884–885.
[39] Brush (1967), p. 885.
[40] According to the title page, "Dissertation to obtain the doctorate [*Doktorwürde*] of the mathematical-scientific faculty of the University of Hamburg."
[41] Kobe (2000), p. 653; Ising's letter is in German.

of the interaction that Ising introduced was one of the main targets of criticism by his contemporaries.

With Lenz, Ising endorsed Weiss's view that the physical origin of the interaction between the elementary magnets was nonmagnetic in nature, since it was well known by that time that magnetic forces are too weak to give the right order of magnitude for the Curie temperature. Thus, the German physicist Walter Schottky (1886–1976) concluded in 1922, as Ising noted below, that the interaction was electrostatic in nature, because he was able to obtain the right order of magnitude for the Curie point by estimating the electrostatic energy in crystals. Ising, however, did not take a stand on the physical origin of the interaction:

> In addition to an applied external magnetic field, the elements should also be affected by the forces that they mutually perform on each other. These forces may be of an electric nature [he cites here Schottky (1922)], but we cannot make a closer description of them; however, we assume that they decay rapidly with distance, so that we, in general, to a first approximation, only have to take the influence on neighboring elements into account. The latter assumption is somewhat in contrast with the hypothesis of a molecular field, which Weiss . . . has shown is not of a magnetic nature. We assume that of all the possible positions that the neighboring atoms can assume in relation to each other, the one that requires the minimum energy is when they are both acting in the same direction.[42]

Ising's last assumption of a minimum energy is crucial to explain the ordering of the elementary magnets in ferromagnetic materials. It is not clear whether Ising did not justify this assumption and that of the short range of the interaction because he considered them to be obviously correct, or because he was unable to give an argument for their correctness, even though he realized that both were necessary to explain ferromagnetism, in which case he may have considered them as working hypotheses. Thus, it is difficult to judge the extent of Ising's belief in the correctness of these assumptions.

Nonetheless, since Ising's introduction of them is crucial, I will try to uncover their origins. His first assumption of the short range of the interaction seems to have been inspired by Weiss's and Schottky's work, while his assumption of a minimum energy is more difficult to deal with, but also may have been inspired by Schottky's paper. Thus, Schottky had considered atoms placed horizontally in a plane with the magnetic moment of each atom arising from the revolution of its electrons, so that depending upon their direction of revolution the magnetic moment of each atom will point either up or down. He then argued that the electrostatic-potential energy between two neighboring atoms is a maximum when their electrons revolve in the same direction, so that it is smaller when the magnetic moments of the two atoms are parallel. This is in contrast to a situation in which the magnetic moments of the two atoms behave as macroscopic magnets, where the potential energy between them is larger for the parallel case than for the antiparallel case. Schottky, as noted above, was able to obtain the right order of magnitude for the Curie temperature from these considerations, and his idea of a minimum potential energy appealed to some of his contemporaries[43] and may have inspired Ising. Arguing against this suggestion, however, is that Schottky and Ising had different views on the

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[42] Ising (1924), p. 4.
[43] Herzfeld (1925), p. 832.

orientation of the elementary magnets: Schottky took them to be pointing perpendicular to the plane in which they lay, while Ising always depicted them as pointing in this plane, at least in the case of a linear chain of them, which was his main object of study. Thus, if the chain extends horizontally, it consists of elementary magnets pointing left or right, $\leftarrow$ and $\rightarrow$, respectively (and thus not the way the Lenz-Ising model is usually presented in modern textbooks). I conclude that because Schottky's argument does not apply to Ising's linear chain, it very likely did not stimulate Ising's conception of it. Another possibility here is connected to Ising's perception of the orientation of the elementary magnets. The configuration with the smallest *magnetic* energy is obtained when they point in the same direction (for instance $\leftarrow\leftarrow$) instead of the opposite direction (for instance $\leftarrow\rightarrow$). Since there seemed to be no reason to prefer the perpendicular orientation that Schottky proposed, Ising could chose the ones he did in a natural way and obtain the minimal potential energy, which is crucial for his explanation of ferromagnetism. A solid theoretical foundation for his choice was given only after the creation of the new quantum mechanics.

Ising's linear chain

Ising described his goal in introducing his concept of the linear chain as follows:

> We now begin our task proper, the examination of the question whether ferromagnetism is explainable through the assumptions made. First, we carry this task out through a model as simple as possible; indeed through a linear magnet whose elements can assume only two positions. We shall find all essential results already present here.[44]

Ising then examined in detail and extended his concept of a linear chain of elementary magnets. Specifically, his linear chain consists of n elementary magnets placed equidistantly along a line, each of which has a magnetic dipole moment of magnitude $\mathfrak{m}$ that is restricted to point in either of only two directions along it, so that two neighboring ones can assume only the following four configurations: $\rightarrow\rightarrow$ $\leftarrow\leftarrow$ $\rightarrow\leftarrow$ $\leftarrow\rightarrow$. It costs an inner energy ε to go from a configuration in which two neighboring elements have the same sign (the first two configurations) to one in which one of the elements is turned over by 180° (the second two configurations).

Ising evaluated the partition function Z for the linear chain when exposed to an external magnetic field of strength $\mathfrak{H}$ by counting the total number of possible configurations and then, with the help of Eq. (5), he derived the following expression for the magnetization

$$\mathfrak{J} = \mathfrak{m}n \frac{\mathfrak{Sin}\,\alpha}{\sqrt{\mathfrak{Sin}^2\alpha + e^{\frac{2\varepsilon}{kT}}}}, \tag{14}$$

where $\alpha = \frac{\mathfrak{m}\mathfrak{H}}{kT}$. Trigonometric functions written in fraktur are an old German notation for the corresponding hyperbolic functions.[45]

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[44] Ising (1924), p. 5.
[45] See, for example, Courant (1930), p. 149.

Ising obtained the Curie law from Eq. (14) in the paramagnetic case, where $\varepsilon/kT = 0$, that is, when there is no interaction among the elementary magnets. He also showed that Eq. (14) implies that for the ferromagnetic case, if $\mathfrak{H} \to 0$, then $\mathfrak{J} \to 0$, that is, if the external magnetic field vanishes, the magnetization vanishes as well, so that the linear chain does not exhibit spontaneous magnetization. That to Ising was an "unwanted" result,[46] but it did not knock him out:

> Under the assumptions made in the beginning and by means of the models considered previously [his linear chain and another model, irrelevant to my discussion], we succeeded only in explaining paramagnetism, and it is possible that perhaps only too crude an idealization does not let a ferromagnetic relation appear. It is imaginable that a spatial model, in which all elements that in some way are neighbors affect each other, brings with it the necessary stability to prevent the magnetization intensity to vanish with $\mathfrak{H}$. However, in that case the calculations do not seem to be feasible; at any rate, so far it has not been possible to sort and count the appropriate arrangement possibilities. To get a general overview of the situation, we shall make the following three considerations, in which we omit some of the previous simplifying assumptions.[47]

Ising's basis for his three considerations was three "complicated cases," which were generalizations of his linear-chain model. From my point of view, the most important one is the so-called spatial model, which is related to, but not identical with what today is termed the three-dimensional model, as I will discuss below. First, however, I will discuss Ising's other two generalizations because they provide further insight into his conception of his original model.[48]

In his first generalization, Ising took more positions of the elementary magnets into account. He allowed for so-called cross-positions in which each element can be oriented in $r + 2$ directions, the usual two directions along the extension of the chain and another r directions perpendicular to it. Recall that Lenz, based on Weiss's result that the total magnetic moment is confined to the magnetic plane, assumed that the elementary magnets are situated only in this plane. Ising accepted Lenz's assumption of only two directions of the elementary magnets in his initial linear-chain model, but pictured the r cross-positions to be in the magnetic plane and the two original directions to be *perpendicular* to it. Thus, Ising's new assumption seems to contradict Lenz's assumption that the elementary magnets live in the magnetic plane. Ising did not discuss this contradiction, which seems hard to escape within the framework of his new model, either in his thesis or in his paper.

Ising's first generalization, nonetheless, sheds some light on his method of modelling, most notably on how physical phenomena restrict the model. The only restriction that Ising placed at first on the values of r was that they be even numbers, since for every allowed orientation of the elementary magnets, the opposite orientation is also

[46] Ising (1924), p. 15.
[47] *Ibid.*, pp. 24–25.
[48] In my presentation of Ising's generalizations, I have interchanged his second and third generalizations.

allowed. When Ising interpreted the elementary magnets as belonging to a particular solid, however, he imposed another restriction:

> If we think of the sixfold axes of pyrrhotite as the longitudinal direction, then $r = 6$; in contrast, we have to put $r = 4$ for magnetite, corresponding to the fourfold symmetry of the axes. The r cross-positions all have equal roles.[49]

This is a physical and not a mathematical restriction on r because he carried out his calculations under the assumption that r is an arbitrary even number. Thus, it is not quite clear how realistic he considered this model to be, but because he restricted r to values that are compatible only with real magnetic materials, this suggests that he did not regard it just as a "toy model," but instead regarded it to some extent as a realistic one.

Ising provided no arguments for the different energy costs associated with some of the arrangements of neighboring elements, but not to others that could be considered as equally important,[50] and he offered no clear physical basis for his choices. He did carry out some calculations of the general values of these energy costs, but only examined a special case in detail[51] for which he arrived at the same disappointing conclusion that he had come to for the case without cross-positions, namely, that it still did not yield any spontaneous magnetization.

Ising's second generalization involved a linear chain with interactions between next-nearest-neighboring elementary magnets as well as between nearest-neighboring ones. He distinguished between the energy cost of the turnover of an elementary magnet in these two cases, but he made no assumption regarding the relationship between the two energy costs. He performed calculations similar to the ones in his first generalization and concluded as before, that the magnetic moment vanishes as the external magnetic field vanishes. Although he did not motivate his second generalization, it seems likely that he wanted to examine the range of the interaction by considering next-nearest neighboring elementary magnets as well as nearest-neighboring ones.

Ising's spatial model

In his third and last generalization, Ising first examined interactions between elements in two linear chains and then in several linear chains. His double-chain model, however, does not shed new light on his methods of modelling, so I turn immediately to his several-chain model. Given, then, n_1 parallel linear chains, he obtained what he called a spatial model (*räumlichen Modell*)[52] in which the magnetic moment of each

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[49] Ising (1924), p. 25.

[50] Ising did not associate an energy cost with the arrangement of two cross-positioned elements.

[51] In this case, it costs the same energy for a positive as for a negative element to be a neighbor of a cross-positioned element.

[52] Since Ising's paper is clearer than his thesis on this point, I shall follow the description in his paper here.

elementary magnet in one chain can assume two directions with respect to the one in the chain above or below it:

> If similar poles of neighboring elements of the same chain collide at an inner position, then the crystal possesses the energy ε as before. In addition, it contains the energy $\bar{\varepsilon}$ whenever two elements, one directly underneath the other, are directed oppositely.[53]

Ising's model here must be seen as three-dimensional rather than two-dimensional, because he used the word "three-dimensional" in the abstract of his paper and in the paper itself where he termed his model spatial (*räumlichen*), whereas he had termed his earlier model planar (*flächenhaften*). Ising did not explain how the above arrangement of the chains constitutes a three-dimensional crystal, but his use of the word "layer" in relation to their cross section may suggest a regular organization of the elements so that their cross section looks like a two-dimensional square lattice with n_1 points. From a mathematical point of view, however, the exact arrangement of the chains did not play an important role, because he further assumed that the energy $\bar{\varepsilon}$ is so large that all n_1 elements in a vertical layer point in the same direction, which means that a vertical layer can be treated as a single composite unit pointing in one direction and having a total magnetic moment $n_1\mathfrak{m}$. In the limit of large $\bar{\varepsilon}$, each unit interacts with each other one in the same way as the elements in the original linear chain, but the interaction energy between oppositely oriented layers is $n_1\varepsilon$ instead of ε. With these substitutions into Eq. (14), Ising found that the magnetization is given by

$$\mathfrak{J} = \mathfrak{m} n_1 n \frac{\mathfrak{Sin}\,(n_1\alpha)}{\sqrt{\mathfrak{Sin}^2(n_1\alpha) + e^{n_1 \frac{2\varepsilon}{kT}}}}. \tag{15}$$

It follows that there also is no spontaneous magnetization in Ising's spatial model.

To a modern reader, because Ising reached this conclusion by considering his spatial model in the limit of large $\bar{\varepsilon}$, he destroyed any chance of reaching any conclusion about what we today would call the three-dimensional model. Ising himself, however, seemed to think that his conclusion could be extended to the "modern" three-dimensional model, since in the abstract of his paper he wrote:

> It will be shown that such a model [the linear chain] does not have any ferromagnetic properties and that this statement also includes the three-dimensional model.[54]

Indeed, since Ising gave no indication that he restricted the class of spatial models to the class to which this general result applies, where his only assumptions were that of nearest-neighbor interactions and that of only a few orientations of the elementary magnets, he must have included in this class of spatial models the one that we today

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[53] Ising (1925) pp. 257–258. My translation of Ising's paper has been assisted by the translation by Jane Ising and Tom Cummings that can be found at http://www.fh-augsburg. de/~harsch/anglica/Chronology/20thC/Ising/isi_fm00.html.

[54] Ising (1925), p. 253.

would call the three-dimensional Ising model. In his thesis, however, Ising refrained from drawing that conclusion:

> So, if we do not assume, as P. Weiss did, that also quite distant elements exert an influence on each other – and this seems to us not to be allowed under any circumstances – we do not succeed in explaning ferromagnetism from our assumptions. It is to be expected that this assertion also holds true for a spatial model in which only elements in the nearby environment interact with each other.[55]

Ising thus changed his mind on this point after writing his thesis and before publishing his paper. The reason seems to be, as he argued in his paper, that this particular spatial model, in which the elements can assume two directions, is more likely to exhibit ferromagnetism than a general spatial model in which cross-positions are allowed:

> [The] positions of the moment perpendicular to the chain would demand too much energy to be realized. This assumption can be only favorable for the magnetization intensity along the direction of the chain which we consider.[56]

Ising inserted a footnote here referring to Weiss's results on pyrrhotite, probably meaning Weiss's observation that pyrrhotite cannot become magnetized in every direction but only in its so-called magnetic plane, which Weiss took to consist of rows of elementary magnets that can interact with each other only if they are in the same row. Since, however, the elementary magnets can rotate freely in the magnetic plane, they tend to become aligned along the direction of the row in the absence of an external magnetic field, but along its direction when one is present.[57] Ising thus concluded in his paper that:

> Even though we here must expect a more favorable result than in a general spatial model, we recognize here, too, the vanishing of $\mathfrak{J}$ with $\mathfrak{H}$.
>
> So, in the model chosen here, whose essential trait is the restriction of the interaction between neighboring elements, ferromagnetism does not appear. [58]

In sum, Ising in his thesis opened up two possiblities for why the two basic assumptions of all his models – that the interaction between two elementary magnets is of short range and that the interaction energy is minimal when they act in the same direction – were incapable of explaining ferromagnetism.[59] First, either or both of these assumptions was not adequate (*zutreffend*). Second, contrary to Weiss's basic assumption and that of his successors, including Ising, thermal equilibrium did not apply to ferromagnetism, in which case Boltzmann's statistical theory could not be employed in explaining it. This possibility, according to Ising, was credible because of the phenomenon of spontaneous magnetization, that is, "the fact that a body once magnetized does not by itself change polarity, even though certainly no direction is energetically distinguished from its

[55] Ising (1924), p. 49.
[56] Ising (1925), p. 257.
[57] This history is discussed in Keith and Quedec (1992), p. 374.
[58] Ising (1925), p. 258.
[59] Ising (1925) does not discuss this issue.

opposite one."[60] Ising did not elaborate on this point, but he could have meant that it "conflicts with Boltzmann's theory, which implies that in the absence of an external magnetic field the polarity of a ferromagnet should point in each direction in an equal amount of time instead of always in the same direction. Ising did not settle on which of these two possibilities was more likely to explain why his models were not able to explain ferromagnetism, but they do reveal his uncertainty about its fundamental assumptions.

Contemporary reactions to the Lenz-Ising model

Ising's negative conclusion about the ferromagnetic behavior of the Lenz-Ising model might suggest that his contemporaries neglected it, especially because the creation of the new quantum mechanics in 1925–1926 had a dramatic impact on the theory of solids in general and on theories and models of magnetism in particular.[61] Thus, Samuel A. Goudsmit and Geoge E. Uhlenbeck's introduction of the concept of electron spin had clear implications for theories of magnetism, since it soon was realized that the ferromagnetic properties of matter were associated with the intrinsic spin angular momentum of the electron and not with its orbital angular momentum.[62] New fundamental theories of ferromagnetism soon appeared that might well have rendered the Lenz-Ising model obsolete shortly after it was proposed. In fact, however, it was not neglected. Why?

To attempt to answer this question, we must examine the reception of Ising's work despite the sparsity of documentary evidence bearing on it. One document, however, consists of comments that Ising made in a letter to Stephen G. Brush concerning Ising's "unwanted" conclusion that the linear chain results in no spontaneous magnetization:

> I discussed the result of my paper widely with Professor Lenz and with Dr. Wolfgang Pauli, who at that time was teaching in Hamburg. There was some disappointment that the linear model did not show the expected ferromagnetic properties.[63]

Brush (1967) and Hoddeson, Schubert, Heims, and Baym (1992) convey the impression that by and large the Lenz-Ising model was not discussed by physicists before 1936, when Rudolf Peierls proved that the two-dimensional model in fact does display spontaneous magnetization at low temperatures. One main source for this impression is another passage in Ising's letter above to Brush, where Ising (who had left physical research after finishing his degree in 1925) states that he was aware of only one contemporary citation to his work, namely, by Werner Heisenberg in his paper of 1928 in which he introduced his theory of ferromagnetism.[64] Incidentally, Ising used the term "contemporary" in quite a narrow sense, since he did not include under it a paper by Lothar Nordheim of 1934, which he also pointed out to Brush.

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[60] Ising (1924), p. 49.
[61] Hoddeson, Baym, and Eckert (1992), pp. 89, 123.
[62] Keith and Quedec (1992), p. 406.
[63] Brush (1967), p. 886.
[64] Heisenberg (1928a).

Nonetheless, Ising and others have exaggerated the neglect of the Lenz-Ising model. Although it is true that it was not cited in a host of publications on ferromagnetism in the 1920s and early 1930s, including, for instance, Edmund C. Stoner's two influential monographs on magnetism,[65] Heisenberg was not the only physicist to cite it.[66] Karl F. Herzfeld, in fact, cited Ising's paper already in 1925,[67] and both Wolfgang Pauli and John H. Van Vleck discussed Ising's results as well, Pauli in his paper given at the sixth Solvay Congress in October 1930,[68] which was devoted to magnetism and which was attended by most of the leading researchers in the field, and Van Vleck in his widely used textbook on magnetism.[69] Thus, although the number of contemporary citations to the Lenz-Ising model was not large, they included ones by the leading authorities in the field who exerted enormous influence on the subsequent theories of ferromagnetism.[70] Moreover, Ising's results and the Lenz-Ising model evidently were discussed personally among physicists in the late 1920s and early 1930s: Heisenberg referred to Ising's conclusions in a letter to Pauli in 1928,[71] and Peierls wrote in 1936 that Ising's results have "led to a good deal of controversy,"[72] a view he repeated in an interview in 1981.[73] Likewise, Hans A. Bethe stated in an interview in 1981 that the model "was discussed very much" in the early 1930s.[74] In sum, the Lenz-Ising model certainly was not neglected by physicists in the 1920s and early 1930s.

Ising's contemporaries generally seem to have accepted his result for the linear chain, but they rarely cited his negative conclusion for his three-dimensional model.[75] Herzfeld, for instance, did not mention it in 1925 when he stressed the importance of checking whether Ising's result for the linear chain also holds for his three-dimensional model.[76] At the same time, some physicists considered Ising's conclusion to be valid, while others did not. Thus, Peierls wrote in 1936 that "the opinion has often been expressed"[77] that the results for the three-dimensional case will be similar to those of the one-dimensional case, and in his autobiography of 1985[78] he recalled that he was so provoked by a mathematician who asserted the correctness of this conjecture in a talk, without offering proof, that he went home determined to prove the mathematician wrong.[79] Heisenberg

[65] Stoner (1926) and Stoner (1934).
[66] Heisenberg (1928b) cites this paper as well.
[67] I am grateful to Helge Kragh for bringing this important paper to my attention.
[68] Pauli (1932).
[69] Van Vleck (1932).
[70] Hoddeson, Baym, and Eckert (1992), pp. 123–124, 135; Keith and Quédec (1992), pp. 407–415.
[71] This letter can be found in Hermann, von Meyenn, and Weisskopf (1979), p. 467.
[72] Peierls (1936b), p. 478.
[73] Peierls interview with Hoddeson (1981), pp. 20–21.
[74] Bethe interview with Hoddeson (1981), p. 9.
[75] In 1938 Lamek Hulthén repeated Ising's conclusion uncritically; see Hulthén (1938), pp. 2, 15.
[76] Herzfeld (1925), p. 832. A later example is the controversy referred to by Peierls, which concerned exactly this point.
[77] Peierls (1936b), p. 478.
[78] Peierls (1985).
[79] This anecdote is pointed out by Hughes (1999), p 106.

and Pauli, by contrast, seem to have believed that some variant of the three-dimensional model would display ferromagnetism. Thus, in a letter to Pauli in 1928,[80] Heisenberg suggested that if Ising had assumed sufficiently many nearest neighbors, probably more than eight (a number that Heisenberg himself had found to be necessary for the appearance of ferromagnetism), Ising would have obtained ferromagnetism. In 1930, Pauli too conjectured that the three-dimensional Lenz-Ising model very likely would display spontaneous magnetization.[81]

Heisenberg's theory of ferromagnetism

Heisenberg's new theory of ferromagnetism of 1928 had a lasting influence on physicists' perception of the realism of the Lenz-Ising model. His interest in magnetism was not motivated by magnetic phenomena *per se*, but by the light they might shed on fundamental issues in quantum theory, that is, on the statistics of many-electron systems and on symmetries of the wave function.[82] He began by echoing Ising's criticism that Weiss's theory was only formally satisfactory in that it was based on the assumption that every atom in a crystal lattice is influenced by an aligning force arising from the other atoms whose origin, however, is unknown. He argued that the two obvious candidates for its origin were inadequate: It could not arise from the magnetic interaction between the atoms, because it is an order of magnitude smaller than the experimentally known atomic field, nor could it arise from the Coulomb interaction, which should be proportional to the square of the cosine of the angle between any two atoms, contrary to Weiss's assumptions. In proposing another candidate, he quoted Ising's result:

> Other difficulties are discussed in detail by Lenz, and Ising succeeded in showing that also the assumption of aligning sufficiently great forces between each of two neighboring atoms of a chain is not sufficient to create ferromagnetism.[83]

Heisenberg's fundamental idea was that the angular deficiency of the Coulomb interaction can be remedied by combining the Coulomb interaction with Pauli's exclusion principle, which, more generally, also will be sufficient to reproduce the results of Weiss's theory.[84] To prove this, Heisenberg used the Coulomb exchange interaction, which he had introduced two years earlier to investigate the properties of helium, and which cannot be described in simple, intuitive language;[85] it leads to an interaction energy between pairs of electrons given by the so-called exchange integral. The case of helium with its two electrons seemed relevant to ferromagnetism, because of the strong spin-dependent

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[80] Heisenberg to Pauli, July 31, 1928; reproduced in Hermann, von Meyenn and Weisskopf (1979), p. 467.

[81] Pauli (1932), p. 210. The publication of the proceedings was underway two years.

[82] Hoddeson, Baym, and Eckert (1992), p. 129.

[83] Heisenberg (1928a), p. 619; translated in Kobe (2000), p. 652. Brush (1967) gives a slightly different translation.

[84] Hoddeson, Baym, and Eckert (1992), p. 134.

[85] Van Vleck (1945), p. 30.

interaction energy between them. In 1929 P.A.M. Dirac derived[86]an explicit expression for the Hamiltonian

$$H = H_I + \sum_{i<k} H_{ik} \frac{1}{2}[1 + (\sigma_i, \sigma_k)], \tag{16}$$

where the sum is taken over the indices of the electrons, H_I and H_{ik} are terms independent of the spin σ of the electrons, and $(\sigma_i, \sigma_k) = \sigma_{xi}\sigma_{xk} + \sigma_{yi}\sigma_{yk} + \sigma_{zi}\sigma_{zk}$. This Hamiltonian, which was seen as characteristic of Heisenberg's theory, is sometimes called the Heisenberg-Dirac Hamiltonian. In any case, Heisenberg was able to derive Weiss's equation (Eq. (10) with H replaced byH_{total} of Eq. (11)) under the assumption that only nearest-neighbor interactions are nonnegligible, that the exchange integrals are the same for all electron pairs, and that the exchange energies have a Gaussian distribution.[87] He showed in this way that he could explain Weiss's molecular field in terms of the quantum-mechanical spin interaction. His contemporaries immediately accepted the exchange interaction as the correct mechanism behind ferromagnetism.[88] Nonetheless, they regarded his theory as only a step in the right direction, not as a final theory, and Heisenberg himself acknowledged some of its problems, including the arbitrariness of the Gaussian distribution.[89] Moreover, some physicists attacked his assumption that all of the nearest neighbors have the same exchange integral.[90]

The above approach is usually called the Heisenberg *theory* of ferromagnetism, whereas the term Heisenberg *model* is used for the situation in which the exchange energy is negligible except for nearest-neighbor pairs of atoms and is the same for all such pairs. In the Heisenberg model, the total interaction energy is given by the so-called Heisenberg Hamiltonian

$$H_l = -2J \sum_{neighbors} \sigma_i \cdot \sigma_j, \tag{17}$$

where J is the exchange energy, which is the same for all neighboring pairs, σ_i and σ_j are the spins of the *i* and *j* electrons (which are represented by vectors), and the sum is taken over all nearest-neighbor pairs of atoms in the crystal lattice.

The relationship between the Lenz-Ising model and Heisenberg's theory

Lenz and Ising considered the elementary magnets in a crystal to arise from the magnetic moments of its atoms or molecules, while Heisenberg took them to arise from the spin angular momentum of the electrons in the atoms or molecules. Lenz and Ising

[86] Dirac (1929). In this paper, Dirac did not refer to Heisenberg.
[87] Hoddeson, Baym, and Eckert (1992), pp. 129–135.
[88] See, for example, the views expressed by Felix Bloch in Hoddeson, Baym, and Eckert (1992), p. 135; by John H. Van Vleck in Van Vleck (1932), p. 322; Edmund C. Stoner in Stoner (1934), p. 352, pp. 424–425; and Peierls in Peierls (1936b), p. 477.
[89] Hoddeson, Baym, and Eckert (1992), pp. 134–135.
[90] See, for instance, Van Vleck (1945), p. 32.

did not discuss the origin of the dipole moment of the atoms or molecules, but they no doubt believed that it arose from the revolution of their electrons about their nuclei.[91] Moreover, considering the skeptism with which several physicists greeted Goudsmit and Uhlenbeck's concept of electron spin,[92] Lenz and Ising probably did not even consider electron spin as origin of the elementary magnets when they proposed their model.

That did not prevent Heisenberg from noting the similarity between the Lenz-Ising model and his own, for instance in a paper he wrote on the occasion of Arnold Sommerfeld's 60th birthday in 1928:

> [The] model grounded here [an extension of the Heisenberg model described above] actually shows great similarity with Ising's model (only interaction between neighboring atoms) and differs essentially only through the value of z [the number of nearest neighbors in the lattice], i. e., through the number of neighbors that surround an atom.[93]

The first use of the term "Ising model" is usually ascribed to Peierls (1936b),[94] but we see here that Heisenberg used it eight years earlier, at least for the one-dimensional case.

Heisenberg, however, did not discuss Lenz and Ising's assumptions from a more fundamental point of view, although his sparring partner Pauli did in the paper he gave at the sixth Solvay Congress in 1930, where he called the Lenz-Ising model "a semi-classical model"[95] and took its Hamiltonian (probably referring to the one-dimensional case) to be:

$$H = -A \sum_{k} (\sigma_k, \sigma_{k+1}), \tag{18}$$

where σ_k is the spin angular-momentum vector of the kth electron and the operator (σ_k, σ_{k+1}) attains the value 1 if the spins of electrons k and $k+1$ and are parallel and -1 if they are antiparallel. Thus, Pauli implicitly reinterpreted the Lenz-Ising model in terms of electron spins so that he (and others) took its basic constituents, the elementary magnets, to be different from Lenz and Ising's and in that sense did not discuss the same model. Pauli was able to reinterpret the Lenz-Ising model in this way only because Lenz and Ising had focused on the general properties of the magnetic moment, not on the details of its origin.

Pauli now could view the Lenz-Ising model in light of the new quantum mechanics and Heisenberg's theory of ferromagnetism, between which he saw a "narrow kinship"[96] as displayed by Dirac's Hamiltonian Eq. (16) and Ising's Hamiltonian Eq. (18). Pauli argued that since the electron spins σ_i in Eq. (16) are treated as operators in the new quantum mechanics, this also should be true for the magnetic moment $\mathfrak{m}$ in the Lenz-

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[91] This view is expressed by Schottky (1922) as cited in Ising (1925) and Ehrenfest (1921).
[92] Mehra and Rechenberg (1982b), pp. 199–204.
[93] Heisenberg (1928b), p. 122.
[94] See, for instance, Kobe (2000), p. 652.
[95] Pauli (1932), p. 209.
[96] *Ibid.*, p. 210.

Ising model. That, however, implied that the Lenz-Ising model was in conflict with the new quantum mechanics:

> In Ising's calculus, developed from the point of view of the old quantum mechanics, the components of σ_i that are perpendicular to the field are considered to be zero, whereas in the new quantum theory these components do not commute with the components of the field.[97]

That conflict meant that to Pauli and others the Lenz-Ising model was fundamentally deficient.[98] As Hans A. Bethe remarked in 1981, endorsing Pauli's criticism of it:

> Well, even if it was old, it was discussed very much at that time still [in the early 1930s]. But it clearly was not the right model, because the spin is a quantum object and not a classical object, so you couldn't just say up spin and down spin, but have to permit them to change direction.[99]

Others criticized the Lenz-Ising model because it conflicts with the Heisenberg model, which therefore must have been seen as the more realistic (or at least the more satisfactory) of the two. Specifically, the interaction assumed in the Lenz-Ising model was too simple when compared to that in the Heisenberg model. Thus, Van Vleck (1932) remarked in a footnote that Ising has assumed "arbitrarily" that the coupling between the elementary magnets was given by the first part of the scalar product in Eq. (16), rather than by the complete product as in the Heisenberg model. These objections to the Lenz-Ising model were so serious that it was dismissed as a realistic model of ferromagnetism in the early 1930s.

The advent of the new quantum mechanics may well have played other roles in the reception of the Lenz-Ising model among physicists as a model of ferromagnetism.[100] The Lenz-Ising model tacitly assumed that the interaction between the elementary magnets could be split into pairwise interactions, which may have prompted some uneasiness about its validity. In 1929 P.A.M. Dirac gave a quantum-theoretical justification for this assumption, although he did not refer explicitly to two-body interactions. Likewise, Ising's explicit assumption that the interaction is of short range was likewise put on a more secure theoretical footing in the new quantum mechanics. Finally, Heisenberg's model incorporated the restriction of the Lenz-Ising model to nearest-neighbor interactions. The Lenz-Ising model also fell short on the important issue of the details of the interaction and the noncommutivity of the spin with the field. Peierls thus concluded in

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[97] *Ibid.*

[98] Hoddeson, Schubert, Heims, and Baym (1992), p. 521, argue that the Lenz-Ising model was by and large ignored in relation to *critical phenomena* in the late 1920s and early 1930s, since the focus was on the details of the interaction, where the Lenz-Ising model fell short. It is not so clear what they are referring to by the term "critical phenomena" (they seem to use the term "critical phenomena" for what I called cooperative systems in introduction), because there was no general treatment on critical or cooperative phenomena in this period. However, if their argument is restricted to the area of ferromagnetism, it is correct, as shown by Pauli's treatment of the model.

[99] Bethe with Hoddeson (1981), p. 9.

[100] This interesting point was made to me by an anonymous reader to whom I am thankful for it.

1936 that, "The Lenz-Ising model is therefore now only of mathematical interest."[101] Why, then, did he as a physicist study it? He commented:

> Since, however, the problem of Ising's model in more than one dimension led to a good deal of controversy and in particular since the opinion has often been expressed that the solution of the three-dimensional problem could be reduced to that of the linear model and would lead to similar results, it may be worth while to give its solution.[102]

In 1981 Peierls recalled a more substantial motivation for studying the Lenz-Ising model:

> It is true that at that time [around 1936], Ising's paper was rather old, but interest in ferromagnetism had grown recently because with the advent of quantum theory. Heisenberg had shown the physical basis of ferromagnetism; [Felix] Bloch and Bethe had given approximations to the theoretical problem of ferromagnetism. And so, while all this was leading to rather complicated mathematical problems, one did look back to the very simple and attractive model of Ising.[103]

Bethe recalled yet another reason: He thought that the Lenz-Ising model might shed some light on the temperature dependence of ferromagnetism, but he published nothing on this problem.

After Peierls dismissed the Lenz-Ising model as a model of ferromagnetism in 1936, it was rarely discussed in that connection, and when it was mainly in a negative light. Thus, in a paper published in 1945 but based upon lectures he delivered in 1939,[104] Van Vleck (1945) viewed the Lenz-Ising model only as a simplification of Heisenberg's model with a truncated interaction and repeated his criticism of 1932, only more sharply:

> In a certain sense the Lenz-Ising model is a purely mathematical fiction, as it neglects the interactions $-2J(s_{x_i}s_{x_j} + s_{y_i}s_{y_j})$ between the components of spin perpendicular to the direction of the magnetic field, which are often important physically.[105]

He continued to carve up the Lenz-Ising model, stating that even if it could serve as the basis for rigorous calculations for three-dimensional lattices, "the result should not be identified too closely with the actual magnetic behavior of the material simply because of the inadequacy and arbitrariness of the model."[106] He saw its only role as providing mathematical insight into eigenvalue problems associated with magnetic materials, and thus into solutions of Heisenberg's model. It was not significant in its own right as a model of ferromagnetism.

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[101] Peierls (1936b), p. 477.
[102] *Ibid.*, pp. 477–478.
[103] Peierls with Hoddeson (1981), pp. 20–21.
[104] Although published in 1945, a footnote (p. 27) states it is based "to a considerable extent" on one of several lectures given in Paris in 1939. They were meant to be published in the *Annales de l'Institut Henri Poincaré*, but the publication was delayed by the German invasion of France, and they first appeared in Van Vleck (1947).
[105] Van Vleck (1945), p. 34.
[106] *Ibid.*, p. 34.

Van Vleck was a leading authority on magnetism, so it seems fair to conclude that most researchers in the field rejected the Lenz-Ising model as a model of ferromagnetism by 1945, and probably even earlier. Subsequently, it sometimes was referred to as a model of ferromagnetism, and to the extent that its realism was discussed, it was described as unrealistic.[107] A shift occurred, however, in the 1950s when it was used to model the magnetic properties of some pure rare-earth elements and good agreement was found with such real elements, because both have a strong anisotropic interaction; some differences, however, remained.[108]

I should note, at the same time, that there were at least two positive responses to the Lenz-Ising model in the field of ferromagnetism before 1945. Francis Bitter devoted ten pages to Ising's linear chain in his textbook of 1937 although he considered Heisenberg's model as more realistic. Still, Ising's result for the linear chain "has taken on a new importance,"[109] first, because materials that can be represented by it might be found in the future, and second, and more importantly, because Ising's result was a rigorous one, in contrast to Heisenberg's. Kramers and Wannier, however became the most prominent advocates of the Lenz-Ising model in the years 1936–1945, as I will discuss later. We will see that they were not very clear about its realism in relation to ferromagnetism, but they definitely considered its study as being worthwhile.

I have argued above that the problematic relationship between the Lenz-Ising model and the new quantum theory was largely responsible for its dismissal. Others, however, have claimed that the main reason for ignoring it as a model of ferromagnetism was Ising's negative conclusion for the three-dimensional case. Brush (1967) and Keith and Quedec (1992) have advanced this claim, which now seems to be widely accepted.[110] Two objections, however, can be raised against it. First, as I have shown, it is not entirely clear that Ising's contemporaries accepted his negative conclusion; indeed Heisenberg and Peierls did not, and others may have shared Peierls's opinion that spontaneous magnetization may appear in the two-dimensional and three-dimensional cases, even before Peierls proved that it did. Second, even if some physicists had accepted Ising's negative conclusion, they still may have accepted the Lenz-Ising model. Thus, Peierls's proof seems to have had no impact on his opinion of the realism of the Lenz-Ising model, since in the same paper he suggested that it was only of mathematical interest. Thus, it is hard to maintain that the question of whether or not it can display spontaneous magnetization was decisive for not accepting it as a reasonable representation of ferromagnetism.

Peierls's proof

Ironically, despite Peierls's view that the Lenz-Ising model was only of mathematical interest, his paper of 1936, in which he proved that spontaneous magnetization appears in the two-dimensional and three-dimensional cases, was one of the most important

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[107] See, for example, ter Haar and Martin (1950), p. 721; Newell and Montroll (1953), p. 353; or Yang (1952), p. 808.

[108] Wolf (2000), p. 794.

[109] Bitter (1937), p. 145.

[110] See, for instance, Hughes (1999), p. 104; and Mattis (1985), pp. 89–90.

ones in its early history. He showed that at sufficiently low temperatures the Lenz-Ising model in two dimensions does display ferromagnetism,[111] that is, the ratio $M = \frac{n_+ - n_-}{n_+ + n_-}$ is nonzero, and from his proof he concluded that this is also true in three dimensions. His proof, which uses quite elementary techniques, goes like this: He placed boundaries midway between every pair of nearest magnets of opposite signs, thus separating areas of positive magnets from ones of negative magnets by connected boundaries, which are open if they begin and end at the edges of the crystal lattice and are closed if they do not. He then first made an upper estimate of the number of open boundaries and showed that for a sufficiently large array, the elementary magnets enclosed by them do not play an important role. Second, he estimated the number of closed boundaries and, knowing that a boundary of length L in lattice units corresponds to an energy UL (since each boundary element separates two magnets of opposite signs), he concluded that the number of magnets enclosed by closed boundaries decreases with decreasing temperature: When, say, $4e^{U/kT} < 0.8$, the number enclosed is smaller than 1/4. Thus, for sufficiently low temperatures, less than half of the magnets are enclosed by open and closed boundaries and the ratio M/N is nonzero, that is, the system displays spontaneous magnetization.

The transition to a model of cooperative phenomena

Cooperative phenomena

Since the Heisenberg model was considered as a much more realistic model of ferromagnetism than the Lenz-Ising model by the end of the 1920s, why was the Lenz-Ising model not simply forgotten in the 1930s and 1940s? Why was it studied then despite its apparent lack of realism as a model of ferromagnetism? The answer to these questions are closely linked to developments in the theories of alloys, adsorption, and cooperative or collective phenomena.[112]

Nix and Shockley (1938) and Brush (1967) have recounted these developments in the field of alloys. Thus, the Russian-German chemical physicist Gustav Tamman carried out experiments in 1919 that showed that an alloy of 50% copper and 50% gold forms an ordered array of atoms, superlattices, which X-ray experiments a few years later showed also exist in other alloys. Their characteristic feature is that the atoms of one of their components tend to be surrounded by the atoms of the other component, an example being the superposition of two cubic lattices, where the sites of one lattice are in the middle of the other one. At low temperatures, a superlattice is in an ordered state in which each site is occupied by an atom of the "right" kind, and when it is heated

[111] This proof was later considered to be incorrect by N.G. van Kampen, M.E. Fisher, and S. Sherman, because the summations were over all lengths of the boundaries, that is, including infinite, even for finite systems. R.B. Griffith was able to provide a rigorous proof along the same lines as Peierls; see Kobe (2000), p. 652.

[112] Some historians and philosophers, for instance, Liu (1999), discriminate between cooperative and collective phenomena, whereas others, for instance Hoddeson, Baym, and Eckert (1992), do not. In the period that I consider, this type is called cooperative phenomena, and I shall stick to that term throughout.

thermal agitation enhances the amplitudes of vibrations of both atoms, and they can acquire sufficient energy to change sites. The two atoms then are in the "wrong" sites, which decreases the order of the superlattice.

Bragg and Williams (1934) developed a statistical-mechanical theory of this phenomenon based on the idea that the energy cost of moving an atom from a "right" to a "wrong" site is proportional to the degree of disorder previously present in the superlattice.[113] Bethe (1935) noted that their theory and Weiss's theory of ferromagnetism share the assumption that the "force" trying to produce order at a given site in the lattice depends on the average order of the system; in effect, the two theories use similar mathematical methods. Bethe, however, stressed that in both theories the force ought to depend on the particular configuration of the atoms in the neighborhood of the site in question. Accordingly, he refined Bragg and Williams's theory by assuming that only nearest-neighbor atoms interact, with energies of V_{ab}, V_{aa}, and V_{bb} between an A and a B atom, two A atoms, and two B atoms, respectively. He pointed out that this "assumption is essentially the same as that which underlies the modern theory of ferromagnetism [Heisenberg's theory],"[114] which therefore also should embody the notion of order:

> [Experimentally] a sharp Curie point is found at which the "order," i.e., the permanent moment, of the crystal as a whole disappears. Super-lattices should be similar, and one may even hope that it is simpler to treat since it involves no quantum mechanics but only classical statistics.[115]

To pursue this idea, Bethe introduced the important Bethe approximation (sometimes called the Bethe-Peierls approximation). [116]

In 1936 Ralph H. Fowler in Cambridge proposed a statistical theory of adsorption to account for experiments in which a stream of metal vapor was sent towards a glass surface and a critical temperature T_c was found that separates a low-temperature from a high-temperature regime in which the metal vapor can or cannot be deposited on the glass.[117] Fowler pointed out that although this phenomenon had been studied earlier by Irving Langmuir in 1916 and Yakov I. Frenkel in 1924, his model assumed that the metal vapor is in equilibrium with the glass surface, which consists of a regular array of N sites, each of which is a possible site for the adsorption of exactly one metal atom, that is, the film forms a monolayer below which no metal atoms can penetrate. The adsorption energy χ_0 for each adsorbed atom corresponds to the force exerted on it by the glass, and each pair of neighboring adsorbed atoms has an interaction energy $V = -\chi_0/z$. The problem is to determine the ratio M/N, where M is the total number of adsorbed atoms

[113] Gorsky had made a similar theory earlier, but without receiving the credit he deserved; see Domb (1996), p. 17.

[114] Bethe (1935), p. 552.

[115] *Ibid.*, pp. 552–553.

[116] Technically speaking, in this approximation the focus is on a certain site, called the central site or atom and its nearest neighbors, called the first shell. The nearest neighbors of the first shell (excluding the central site) are called the second shell. The effect of the second shell on the first shell is approximated by the same factor for all sites in the second shell, independent of their actual arrangement.

[117] Fowler (1936).

and N is the number of possible sites that can be occupied by an atom. Fowler (1936) applied the Bragg-Williams theory and Peierls (1936a) applied the Bethe approximation to this problem.

Physicists recognized that such phenomena had certain features in common, both from an experimental and from a theoretical point of view. As Peierls wrote in 1934:

> For many, physically very diverse but formally analogous, phenomena it is found experimentally that a transition takes place from an ordered state to a state of disorder at a certain temperature. . . . Typical examples for this are the melting points of solids and the Curie point of ferromagnetism.[118]

According to Fowler the sharpness of transition points was an "ancient problem,"[119] but the recognition that physically diverse systems exhibit them experimentally was new. That also was true theoretically between various theories and models. As Brush (1967) pointed out, Fowler and his collaborators at Cambridge (including Peierls) seem to have been the first to recognize the analogy between a number of different problems;[120] they applied the idea of order-disorder in alloys and the Bragg-Williams theory to other cooperative phenomena, such as the rotation of molecules in solids and the adsorption of gases.

Fowler, in the second edition of his textbook on statistical mechanics of 1936, concluded that the essential general feature of a large class of phenomena, including the ones described above, is cooperation, and he thus termed them *cooperative* phenomena.[121] Such systems can be described only in terms of the joint action of the units constituting them, that is, the behavior cannot be captured by treating the units approximately as independent. Fowler focused on individual phenomena whose theories or models involve the cooperation of units and discussed these in great detail; he did not provide a general theory of cooperative phenomena – that was simply a convenient umbrella for phenomena that did not fit into other classifications, not a fundamental entity requiring explanation. He treated the theories or models of ferromagnetism and alloys, for example, as distinct even after their mathematical equivalence was known. Thus, for example, he formulated the Bethe theory specifically in terms of binary alloys and compared it to experimental data and thus focused specifically on such alloys and not on cooperative phenomena in general. Still, others suggested otherwise. For example, John G. Kirkwood, then at the University of Chicago and Cornell University, wrote:

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[118] Peierls (1934), p. 137. This description was given at a conference on the theory of metals in Geneva in 1934.

[119] Fowler (1934), p. 74.

[120] Brush (1967) states that Fowler considered the Ising model, which is not quite correct. I will discuss this point below.

[121] Independently of Fowler, Fritz Zwicky of the California Institute of Technology developed an approach to cooperative phenomena that was more ambitious. Indeed, Zwicky (1933) coined the term "cooperative phenomena." There is some overlap between the phenomena classified as cooperative in the two approaches, but it is far from complete. Since Fowler's took root and are related to the development of the Lenz-Ising model, I restrict my treatment to his approach.

> We believe that the theory may prove useful not only in the study of order and disorder in solids, but also in the treatment of cooperative phenomena in general.[122]

Cooperative phenomena and the Lenz-Ising model

According to Brush (1967), Peierls (1936b) first recognized the *mathematical* similarity of the Lenz-Ising model to other theories of cooperative phenomena, writing that Bragg and Williams's and Bethe's theories of alloys, Fowler's theory of the libration-rotation transition point in solids, and some other theories as well, are all physically different but mathematically equivalent.[123] Peierls probably regarded them as physically different because they do not stem from the same physical mechanism or deal with the same physical objects or situations, but they are mathematically equivalent because they obey the same equations. Strictly speaking, however, Peierls only proved the mathematical equivalence of the theory of adsorption and of the Lenz-Ising model. He replaced the empty and occupied sites in the theory of adsorption by minus and plus signs for the elementary magnets and the quantity *2M-N* by the total magnetic moment and in this way was able to establish the mathematical equivalence between Fowler's theory of monolayer adsorption and the Lenz-Ising model, since the equations for both systems are identical. The results of one can thus be applied to the other; for instance, the adsorption transition point and the Curie ferromagnetic temperature are equal when the Bethe approximation is applied to both.

There were historical precedents for such mathematical analogues. For instance, in the nineteenth century, Pierre Simon Laplace (1749–1827) and Simon Denis Poisson (1781–1840) established the mathematical equivalence between Fourier's theory of heat flow and the theory of electromagnetic action, which allowed William Thomson (1824–1907) to transfer the mathematical results of the former theory to the less-developed latter theory.[124] More generally, several physicists in the ninenteenth century recognized that the same partial-differential equations can be applied to physically different continuum systems: Such mathematically equivalent theories were common knowledge by the 1930s[125] and may have facilitated the acceptance of the Lenz-Ising model as an abstraction of more concrete physical systems.[126]

At the same time, because the different theories of alloys, adsorption, and ferromagnetism were not treated in common in the 1930s, this indicates that one should be cautious in using the term "Lenz-Ising model." Today, it is associated mainly with its mathematical structure, more or less detached from any specific physical phenomenon like ferromagnetism. Peierls's recognition that the Lenz-Ising is mathematically equivalent to other theories of cooperative phenomena, however, did not prompt physicists to search for a unified structure of them in the 1930s and the early 1940s. The Lenz-Ising

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122 Kirkwood (1938), p. 70.

123 Peierls (1936b).

124 See, for example, Smith and Wise (1989), pp. 202–212; and Cat (2001), p. 419.

125 Purrington (1997), p. 170. Whittaker and Watson (1927), pp. 386–387, mentioned six different branches of mathematical physics where Laplace's equation occurs.

126 I am grateful to an anonymous reader for this point.

model then was always considered to be a model of ferromagnetism, and not one also of alloys, for instance, whose mathematically equivalent treatment was called the Bethe method applied to alloys. Thus, one has to distinguish carefully between the mathematically equivalent models of different physical phenomena in the 1930s and 1940s, which has not always been done.[127]

In contrast to the appellation Lenz-Ising *model*, treatments of the behavior of binary alloys and adsorption are all called *theories* by researchers in these fields. In view of the great similarity between the Lenz-Ising model and the theories of alloys and adsorption, however, we might ask whether the distinction between "model" and "theory" that was used in the 1930s and early 1940s reflects a fundamental distinction between these two concepts. I first note that the mathematical similarity of the theories of alloys and ferromagnetism was well known: Bethe (1935) noted the analogy between Bragg and Williams's theory of alloys and Weiss's theory of ferromagnetism, and the analogy between his own theory of alloys and the quantum theory of ferromagnetism. Furthermore, all three theories were viewed as drastically simplified representations of real materials. For instance, the interactions between atoms in the two theories of alloys and between spins in the Lenz-Ising model of ferromagnetism are both quite crude, and none are based on the new quantum theory, which was viewed as the correct fundamental theory for all of them. It therefore seems that the only essential difference between the treatments of ferromagnetism and of alloys that can justify different labels is that the Lenz-Ising model was known to be an approximation to the Heisenberg theory of ferromagnetism, whereas no analogous fundamental theory existed for alloys. Thus, the theories of alloys could be viewed as first steps towards such a fundamental theory of alloys, whereas that was impossible for the Lenz-Ising model in its relationship to a fundamental theory of ferromagnetism. Still, since the terms "theory" and "model" were used in different physical contexts, they need not have been used consistently between them.

In general, while physicists did not focus much on the Lenz-Ising model in the theory of ferromagnetism in the 1930s and early 1940s, they used its mathematical equivalent extensively in the theory of binary alloys during this period.[128] That, however, did not change their attitude towards it, so I will not pursue that development further here.

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[127] For instance, Brush (1967), p. 887, wrote: "The concept of the 'Ising model' as a mathematical object existing independently of any particular physical approximation seems to have been developed by the Cambridge group led by R. H. Fowler in the 1930's." In the 1930s, Fowler discussed the Bethe theory of alloys and his own theory of adsorption, and even though they are mathematically equivalent to the Lenz-Ising model, Fowler made no reference to this model. From a historical point of view, it is therefore not correct to conclude that he discussed the Lenz-Ising model, but rather two of its mathematical equivalents.

[128] See, for example, Kirkwood (1938), pp. 70–75; Nix and Shockley (1938), pp. 2–30; Bethe and Kirkwood (1939), pp. 578–582; and Lassettre and Howe (1941), pp. 747–754.

The Lenz-Ising model in the 1940s

In the early 1940s, the papers in which the Lenz-Ising model was discussed were not aimed at understanding particular physical systems but rather transition points and cooperative phenomena in general.

I will examine the quite different motivations of Elliott Montroll (1916–1983), Hendrik A. Kramers (1894–1952) and Gregory Wannier (1911–1983), and Lars Onsager (1903–1976) for studying the Lenz-Ising model. They all were part of the same network of researchers,[129] with the young Montroll being an important link it. Thus, Wannier taught Montroll statistical mechanics as a graduate student in chemistry at the University of Pittsburgh,[130] but he then shifted to mathematics and received his Ph.D. degree with a thesis on the evaluation of integrals appearing in the theory of imperfect gases. He subsequently spent the academic year 1940–1941 as a postdoctoral fellow at Yale University with Onsager[131] where he told Onsager about his and Wannier's ideas on the Ising problem.[132]

Montroll

Montroll published a paper entitled "Statistical Mechanics of Nearest Neighbor Systems" in 1941, calling attention to a host of studies on such physical systems, which are characterized by an interaction between molecules of sufficiently short range to allow them to be modelled by taking into account only nearest-neighbor interactions. These studies dealt with several areas I discussed earlier, for instance, order-disorder phenomena in alloys and the Lenz-Ising model of ferromagnetism. He noted, however, "that no general mathematical technique for the handling of these problems (which are abstractly alike) has been developed."[133]

Montroll then developed an elaborate "general theory,"[134] a general mathematical theory of nearest-neighbor systems, by reducing the partition function to equations of linear homogeneous operators. He applied his theory to the "two-dimensional ferromagnetic net," that is, the two-dimensional Lenz-Ising model, but only "as a medium of demonstration,"[135] explicitly avoiding an interpretation of it. He had more substantial things to say, however, about phase transitions:

> As one raises the temperature of a physical system (containing a large number of molecules) in a given state there often exist temperatures at which the system undergoes a radical change in state. For example, in a solid with a definite crystal structure the lattice

[129] Hoddeson, Schubert, Heims, and Baym (1992), p. 528.
[130] *Ibid.*, p. 529.
[131] Montroll also spent the academic year 1939–1940 at Columbia University with the chemical physicist Joseph E. Mayer (with whom he published a paper on imperfect gases in 1941) and the academic year 1941–1942 at Cornell University with John Kirkwood; see Weiss (1994), p. 365.
[132] Hoddeson, Schubert, Heims, and Baym (1992), p. 531; Onsager (1971), p. xxi.
[133] Montroll (1941), p. 706.
[134] *Ibid.*
[135] *Ibid.*, p. 713.

> is sharply disrupted at a temperature called the melting point, and above this temperature the liquid state exists. There is a temperature at which a ferromagnetic material sharply loses its magnetic properties. Such changes in state are called phase changes or phase transitions. At the transition temperature both states are equally probable and can coexist at equilibrium in any proportion. ... [136]

Montroll connected these qualitative properties of phase transitions to his mathematical description of nearest-neighbor systems by using Frobenius's theory of matrices, obtaining the important result that for a "sharp phase transition"[137] to occur in a square two-dimensional square lattice, the lattice has to extend infinitely far in both directions.

In sum, because Montroll's focus was on the mathematical exploitation of the similarities of nearest-neighbor systems, and because he considered these systems to be only "abstractly alike," his purpose should be seen as an attempt to establish a general mathematical theory of nearest-neighbor systems rather than as an attempt to provide a general physical theory of cooperative phenomena.

Kramers and Wannier

In their two-part paper on the "Statistics of the Two-Dimensional Ferromagnet" of 1941,[138] Kramers and Wannier were more interested than was Montroll in the physical aspects of the Lenz-Ising model. Their work was a continuation of both of their lines of research. Thus, Wannier had studied the theory of transition points under Fowler in Cambridge[139] and had attempted to apply the Bethe method to the melting process just before he began collaborating with Kramers in Utrecht in 1939.[140] Kramers, according to Dresden (1988), had published two earlier papers on ferromagnetism, but I will consider only the first of these two, which he published with G. Heller in 1934, in which they examined whether a classical system of spins can show spontaneous magnetization.[141] They focused on a particular model whose energy is a function of the spins and whose "classical" part treats the spin operator as a classical quantity.[142] They found that it displays a ferromagnetic transition for three-dimensional lattices (but not for lower-dimensional ones) and thus concluded that ferromagnetism can occur in a classical framework.[143]

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136 *Ibid.*, pp. 710–711.
137 *Ibid.*, p. 711.
138 Kramers and Wannier (1941a) and (1941b).
139 Hoddeson, Schubert, Heims, and Baym (1992), p. 529.
140 Wannier to Kramers, March 7, 1939, Archive for History of Quantum Physics, Niels Bohr Archive, Copenhagen, and other repositories.
141 Heller and Kramers (1934); Dresden (1988), p. 29. The second paper was on series expansions of the free energy in the Heisenberg model; see Kramers (1936); ter Haar (1998), pp. 75–81.
142 Thus, there is no conflict with the theorem of Bohr and van Leeuwen, which states that no magnetism can appear in classical systems.
143 Dresden (1988), p. 29.

Kramers probably had given the Lenz-Ising model some thought well before Wannier started to work on it in 1939.[144] A footnote in their first paper[145] indicates that it was written by Wannier owing to wartime-communication difficulties while Kramers seems to have participated in the writing of the second one. Both dealt with transition points in and statistical theories of cooperative phenomena, specifically applying Boltzmann's formalism to them. They remarked that it "is generally believed" that transition points are a consequence of statistical theories, but this was "by no means immediately obvious," [146] since they had been proven to exist only in condensing vapors. Their general aim thus "was to make statistical methods available for the treatment of cooperational phenomena,"[147] but they restricted their treatment to the special case of Curie points in ferromagnetism. Their purpose in treating ferromagnetism statistically was twofold:

> The problem has a mechanical and a statistical aspect. On the mechanical side we wish to improve our understanding of the responsible coupling forces. On the statistical side we wish to derive with certainty the thermal properties from a reasonable accurate mechanical model.[148]

Kramers and Wannier acknowledged that quantum theory was able to explain "satisfactorily the origin and nature of the coupling forces"[149] and that some theories of ferromagnets can explain their thermal behavior in terms of them:

> Not one, however, applies just straight statistics to the mechanical data. ... Generally some simplifying assumption is introduced to facilitate the evaluation of the partition function. It follows that the results obtained are not necessarily a consequence of the mechanical model, but may well be due to the statistical approximation.[150]

Peierls had expressed a similar concern already in 1934:

> Weiss's theory of ferromagnetism and analogous theories for other phenomena are approximately correct even in that domain [near the Curie point]; but in those cases the type of behaviour at the transition point is basically determined by the approximating assumptions which have to be made in order to simplify the theory. Such methods are therefore very useful for the description of systems whose qualitative behaviour at the transition is already known experimentally, but they are of no help in a theoretical investigation of the transition point itself.[151]

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[144] Wannier to Kramers, March 7, 1939, AHQP.
[145] Kramers and Wannier (1941a), p. 252.
[146] *Ibid.*
[147] *Ibid.*
[148] *Ibid.*
[149] *Ibid.*
[150] *Ibid.*
[151] Peierls (1934), p. 137.

Kramers and Wannier accepted that quantum theory is the fundamental theory of ferromagnetism, but their wish to treat ferromagnetism without any approximation prompted them to examine the Lenz-Ising model:

> The present paper is an attempt to gain sound statistical information about some model of a ferromagnet. The Ising model has been chosen because its extreme simplicity makes it particularly suitable for such a purpose.[152]

Moreover, Peierls's rigorous proof that spontaneous magnetization can occur in the two-dimensional Lenz-Ising model made it reasonable to "suspect"[153] that a single transition point exists in it. They reasoned as follows. On the one hand, we know that at high temperatures thermal agitation destroys any ordering of the elementary magnets, so spontaneous magnetization vanishes completely in that temperature range. On the other hand, Peierls proved that spontaneous magnetization is nonzero at low temperatures. Nothing, however, is known about the behavior of spontaneous magnetization in the intermediate temperature range. Specifically, it was not known if the magnetization vanished completely at a definite intermediate temperature, "which is the standard idea associated with the Curie temperature,"[154] so Peierls's proof, while insufficient to rigorously infer the existence of a Curie point, makes it natural to suspect its existence: "It follows that the two-dimensional Ising model is a fair test case for the general statistical theory of ferromagnets."[155] Kramers and Wannier's approach,[156] in contrast to Peierls's proof, was much more mathematically involved and time-consuming: Wannier's letters to Kramers show that he worked on the problem for more than a year, at times very intensively.

At some point, Kramers and Wannier found it convenient to study the Lenz-Ising model in the absence of an external magnetic field. This strategy simplified the mathematics considerably, but it precluded the possibility of obtaining an expression for its magnetization. However, they took an important step (Wannier's letters to Kramers seems to indicate that this was Wannier's idea[157]), shifting from calculating its magnetization to calculating its energy and specific heat, which, since they "show singularities concurrently with the magnetization at the Curie point,"[158] a Curie point might appear in their temperature dependence even at zero magnetic field. An analysis of them thus would reveal the thermal behavior of the Lenz-Ising model, at least in principle.

Kramer and Wannier reformulated the problem of determining the partition function as one involving what later was called *transfer* matrices (which were independently

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[152] Kramers and Wannier (1941a), p. 252.
[153] *Ibid.*, p. 256.
[154] *Ibid.*, p. 264.
[155] *Ibid.*, p. 256.
[156] ter Haar (1998), pp. 81–88, has an accessible description of the technicalities of their derivation.
[157] Wannier to Kramers, June 30, 1939, AHQP.
[158] Kramers and Wannier (1941a), p. 259.

employed by Montroll (1941) and Lassettre and Howe (1941)[159]). A transformation of the matrices showed that singularities in the temperature dependence appear in a high and a low temperature pair. There is one temperature, T_c, however, that did not pair with another temperature, which (in our notation) is given by

$$\sinh J/kT_c = 1. \tag{19}$$

Kramers and Wannier were not able to establish the existence of a Curie point, but they did establish that *if* one does exist, then it must be at the temperature T_c of Eq. (19), since it had to be a unique singularity, while all of the other singularities appeared in pairs. Their transformation method thus could not determine the nature of a possible singularity, that is, the behavior of the specific heat in the neighborhood of the Curie point, but it could be used to rule out some types of singularities. They proved by an exact derivation that if the energy is continuous at the transition point, then either the specific heat is also continuous at that point or its tends to infinity, and they used an argument based on numerics to show that it is in fact infinite at the Curie point. They compared none of their conclusions with experimental data on ferromagnets, but they criticized various approximations, including Kirkwood's, Bethe's, and a new one of their own. These fell into two classes. In the first, both the energy and the specific heat are found to be continuous functions of temperature, which presents a fundamental problem since the specific heat should be singular at the Curie point. In the second class, the energy is a continuous function of the temperature and the specific heat is discontinuous, but this behavior too is ruled out because the former contradicts the latter one. Thus, both classes of approximations are problematic.

Kramers and Wannier subscribed to the general view that the Lenz-Ising model was unrealistic,[160] but they opposed the general view that it was of no interest for the theory of ferromagnetism. They realized that it might provide insight into the theory of ferromagnetism and, more generally, into the nature of transition points: It could serve as a foundation for examining the validity of various uncontrolled approximations, and its exact solution might provide an example of a model that would yield a transition point. They thus evinced a balance between the realism and unrealism of the Lenz-Ising model, which was just what they required. They shared Francis Bitter's emphasis on exactness, but Bitter, unlike them, did not recognize this balance and did not focus on transition points.

Kramers and Wannier's mathematical methods played an important role in the subsequent development of the Lenz-Ising model, but I shall argue that their *attitude* towards it, as indicated above, was as significant to its future development. Their realization of its balance between realism and unrealism brought it "in from the cold," and to the attention

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[159] Wannier (1945), pp. 50–51.

[160] They did not explicitly state how realistic a model of ferromagnetism they considered the Lenz-Ising model to be. Since their intention was to study "a reasonable accurate mechanical model" (p. 252), this statement must apply to the Lenz-Ising model. However, they acknowledged the existence of more correct theories of ferromagnetism, and four years later Wannier described the model as a "schematic representation" of ferromagnetism (and order-disorder phenomena in alloys for that matter); see Wannier (1945), p. 52.

of Lars Onsager. In this sense, Kramers and Wannier's work marks the beginning of the modern era of the study of the Lenz-Ising model, even though the excitement following Onsager's solution of the two-dimensional model in zero external field caused the number of publications on it to increase dramatically.

Onsager

The Norwegian-born Lars Onsager (who became an American citizen in 1945) was a chemist with an unusual flair for mathematics and very broad interests in physics and chemistry. He had made valuable contributions to the theory of electrolytes and to the thermodynamics of irreversible processes (for which he received the Nobel Prize in Chemistry for 1968) and was trying to calculate the entropy of ice when Montroll informed him about his and Kramers and Wannier's results, which turned Onsager's attention to the Ising model.[161]

Onsager's pioneering contribution to the theory of phase transitions was his determination of the specific heat of the two-dimensional, zero-field Lenz-Ising model as a function of temperature in a closed form. He announced this result in the discussion following a paper that Wannier presented at a meeting of the New York Academy of Sciences in the spring 1942;[162] he published his derivation of it two years later (Onsager 1944). He followed up on this paper, which is notorious for its mathematical inaccessibility, by publishing a second one with Bruria Kaufman in 1947, which gives a simplified treatment of the mathematics (mainly due to Kaufman) but embodies the same physical content.

Onsager's realization of the mathematical intricacies of the description of transition points is evident in the very first sentence of his paper of 1944: "The statistical theory of phase changes in solids and liquids involves formidable mathematical problems."[163] Like Kramers and Wannier's, Onsager's focus was on transition points, but while Kramers and Wannier placed no further restrictions on their nature and almost exclusively discussed the Curie point, Onsager used Paul Ehrenfest's classification scheme to single out and concentrate on a general class of transitions. Its physical systems, which include the ferromagnetic transition, are characterized by no release of latent heat during the transition.

Onsager pointed out that this general class of transitions is comprised of several types of systems. For one type, the specific heat varies as $(T_c - T)^{-1/2}$ and becomes infinite as the temperature T approaches the transition temperature T_c. This behavior is observed in the so-called α-β quartz transition in which quartz when heated changes quite rapidly from one solid form of quartz (α quartz) to another form (β quartz). Onsager conjectured that this behavior "may be the rule for a great many structural transformations in

[161] Quoted in Hoddeson, Schubert, Heims, and Baym (1992), p. 531.
[162] Domb (1996), p. 130. Further information on how Onsager became interested in the problem and the origin of his mathematical ideas can be found in Domb's book, and in the various papers in Hemmer, Holden, and Kjelstrup Ratkje (1996) commenting on Onsager's work.
[163] Onsager (1944), p. 117.

crystals."[164] For a second type, the specific heat at the transition temperature is finitely discontinuous, a feature exhibited experimentally by superconductors and theoretically by Bose-Einstein condensation. The Curie point of ferromagnetism and the transition of liquid helium to superfluid helium may be of the first type, because they both seem to be characterized by essential singularities, although it is not clear in both cases whether the singularities appear in the specific heat, as in the quartz transition, or in the first derivative of the specific heat with respect to temperature.

Owing to the very nature of such transitions, a crucial mathematical problem arises for systems characterized by an essential singularity: In computing the thermodynamic functions analytically by using a sequence of ever-better approximations, difficulties arise because the convergence of such a sequence is "notoriously slow"[165] when applied to a singularity and tends to become worse as it is approached. That suggested, Onsager wrote, that:

> When the existing dearth of suitable mathematical methods is considered, it becomes a matter of interest to investigate models, however far removed from natural crystals, which yield to exact computation and exhibit transition points.[166]

That prompted Onsager to examine the Lenz-Ising model, because "[it] is known that this model should have a transition."[167] Thus, like Kramers and Wannier, Onsager sacrificed the realism of the Lenz-Ising model for the sake of obtaining exact results.

Onsager, in contrast to Kramers and Wannier who maintained that the Lenz-Ising model represents a ferromagnetic crystal, was unconcerned about the specific system it represented, because his focus was on its abilitity to represent a particular kind of transition. He characterized it as follows:

> The two-dimensional 'Ising model,' originally intended as a model of a ferromagnetic, [ref. to Ising (1925)] is known to be more properly representative of condensation phenomena in the two-dimensional systems formed by the adsorption of gases on the surfaces of crystals. [ref. to Peierls (1936a)].[168]

Whether Onsager saw it as a good representation of ferromagnetism in three dimensions is less clear, but three years later he and Kaufman returned to its physical representation as a ferromagnet, writing that, "The model may be called a crystalline 'ferromagnetic' with a scalar 'spin'."[169]

Setting aside Onsager's exceedingly complicated mathematical methods in his 1944 paper,[170] he concentrated, as had Kramers and Wannier, on the specific heat of an "infinite [two-dimensional] crystal" lattice for the case of a vanishing field and found that

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[164] *Ibid.*
[165] *Ibid.*, p. 118.
[166] *Ibid.*
[167] *Ibid.*
[168] *Ibid.*
[169] Onsager and Kaufman (1947), p. 139. Another two years later, Kaufman (1949) referred to the model as representing a binary alloy, the third of the three phenomena that Peierls had shown to be describable by equivalent models.
[170] Krieger (1996) gives an accessible outline of Onsager's treatment.

the energy is continuous as a function of temperature but, further, that the specific heat goes to infinity as $-\log|T - T_c|$ as $T \to T_c$, where T_c is given by Kramers and Wannier's equation (19). He showed that this result is at odds with the approximation used by Kramers and Wannier, which he considered to be the best one in the literature, by plotting the essential singularity of the exact result and showing its disagreement with the discontinuity they had found. This meant that all of the approximations used prior to 1944 were fundamentally incorrect.

Onsager did not confront his result with experimental data of any kind, which seems natural for problems such as ferromagnetism and alloys, since he solved the Lenz-Ising model only in two dimensions and that solution would likely differ in essential ways from that of three-dimensional problems. Wannier made this point in 1945 when he evaluated Onsager's result:

> The detailed structure of our singularity... is not of ... much significance. The logarithmic infinity of the specific heat, for instance, is probably due to the fact that the model is two-dimensional. It is not likely to show up in three-dimensional cases.[171]

That Onsager did not compare his result with experiments on the adsorption of gases on a solid surface, which is essentially a two-dimensional problem and one that he did consider the Lenz-Ising model to represent, does not seem to be as natural. This testifies to his focus on the mathematical intricasies of transition points rather than on the Lenz-Ising model as representing specific physical systems.

The role of mathematics

The dramatic rise in the level of mathematics used in examining the Lenz-Ising model after Peierls's proof of 1936 warrants our attention. The application of the mathematical theory of matrices in the form of the transfer matrix played a crucial role not only in the work of Montroll, Kramers and Wannier, and Onsager, but also in that of Lassettre and Howe (1941). Kramers, according to his biographer, had used the idea that the partition function can be written as an eigenvalue of a matrix prior to Kramers and Wannier's papers,[172] but their papers contained "the first systematic and extensive application of matrix methods in statistical mechanics. ..."[173] Prior to the advent of matrix mechanics in 1925, this highly efficient mathematical technique had not been part of most physicists' toolbox,[174] thinking that it belonged "exclusively to the realm of pure mathematics."[175] Thereafter, however, physicists became thoroughly acquainted with matrix methods and could apply their newly acquired mathematical skill to other areas in physics such as statistical mechanics, which was precisely what Kramers and others did. Kramers and Onsager, two of the protagonists in the development of the

[171] Wannier (1945), p. 58.
[172] ter Haar (1998), p. 6, quotes Uhlenbeck that Kramers "had found" the transfer matrix in 1937.
[173] Dresden (1988), p. 31.
[174] Jammer (1966), pp. 206–208.
[175] *Ibid.*, p. 207.

Lenz-Ising model between 1936 and 1944, probably learned the mathematical theory of matrices on their own since they received their formal education before the advent of matrix mechanics.[176] Wannier and Montroll, the other two main protagonists, who belonged to a later generation, probably did not since they received their mathematical training after matrices had become important in quantum theory.

These four scientists all shared a keen interest in mathematics and possessed extraordinary mathematical abilities,[177] but they seem to have differed in their attitudes towards mathematics. Onsager seems to have been the only one of the four who enjoyed abstract mathematics. Kramers, by constrast, "did not have much interest in the investigation of mathematical structures or in abstractions, generalizations, or axiomatizations,"[178] and although he had studied Hermann Weyl's monograph, *Group Theory and Quantum Mechanics*, he deliberately avoided using group theory even in cases in which others found it natural to do so.[179] Onsager solved the Lenz-Ising model by using abstract mathematical methods involving quaternion algebras. Kramers's hesitancy in using abstract mathematics might explain why he and Wannier were not able to solve the Lenz-Ising model, or why they were unable to achieve Onsager's solution of 1944, but they may have been able to do something similar to what Bruria Kaufman did in 1947, namely, rewrite Onsager's quaternion solution in terms of n-dimensional spinors. Wannier, in fact, had applied spinor algebra to the Klein-Nishina formula for Compton scattering already in 1935,[180] so he might in principle have been able to apply this mathematical theory to the Lenz-Ising model. Nevertheless, none of the other protagonists possessed Onsager's extraordinary mathematical abilitities and probably could not have solved the Lenz-Ising model. At the same time, the work of all four shows that statistical mechanics had become a mathematically demanding discipline by the 1940s.

The post-Onsager era

The work of Kramers and Wannier and Onsager showed that the Lenz-Ising model was worthy of study as a model exhibiting transition points: Onsager and Kaufman noted

[176] Dresden is somewhat vague concerning the origin of Kramers's knowledge about matrix theory, saying only that Kramers "either knew or rapidly mastered matrix techniques"; see Dresden (1987), p. 468. There is no account of the source of Onsager's skills in matrix theory in the various biographical notes. However, it is likely that he acquired it on his own as he did other areas of mathematics, according to Longuet-Higgins and Fisher (1996), p. 10.

[177] Onsager received his Ph.D. degree in mathematics at Yale University; see Longuet and Fisher (1996), p. 19. For Kramers's mathematical interests, consult Dresden (1987), pp. 466–470; Wannier's is described in Anderson (1984), p. 101, and Hofstadter (1984), pp. 273, 275; information about Montroll is given in Shlesinger and Weiss (1985), p. 3.

[178] Dresden (1987), p. 467.

[179] *Ibid.*, pp. 468–469.

[180] Lacki, Ruegg, and Telegdi (1999), p. 478.

already in 1947 that by then this property had made it an object of intense study. Most of the papers dealing with it during the decade following Onsager's solution concerned five of its aspects:[181] (1) The application of Onsager's approach to other kinds of lattices;[182] (2) The determination of an expression for the spontaneous magnetization;[183] (3) The introduction of new methods that were mathematically equivalent to Onsager's, including simplifications of Onsager's approach;[184] (4) Attempts at generalization to the three-dimensional case; and (5) The introduction of new approximations.[185] Three new models also were introduced in the spirit of Kramers and Wannier and Onsager, that is, they were models with an emphasis on their mathematical amenability rather than on their physical realism.[186]

In all of these papers, the Lenz-Ising model was discussed almost exclusively for the purpose of obtaining a better understanding of transition points. In the 1940s and 1950s, however, divergent views arose as to what phenomena the model represents. Some, for instance, Domb (1949), Berlin and Kac (1952), Yang (1952), and Temperley (1956) maintained that it was a model of ferromagnetism, while others described it as representing binary alloys, and still others, such as Ashkin and Teller (1943) and Siegel (1951), did not attach it to any particular physical phenomenon but considered it to be a model of cooperative phenomena in general. Many of these people emphasized the importance of studying the model rigorously mathematically at the expense of its physical realism.

I will not discuss the reception and enormous influence of Onsager's solution on modern statistical physics,[187] but I will comment instead on how these new models illustrate how other physicists shared Kramers and Wannier's and Onsager's attitude towards models.

Julius Ashkin received his Ph.D. degree at Columbia University in 1943, a year before Onsager published his solution, and in a paper based on his thesis he and his supervisor Edward Teller proposed a generalization of the two-dimensional Lenz-Ising model. In the parlance of alloys, they defined it in terms of four different atoms instead of two as in the Lenz-Ising model, but they proceeded no further towards a concrete physical interpretation of it.[188] Their mathematical analysis mimicked Kramers and Wannier's, and for the case of attraction between similar atoms, they assumed the existence of a unique temperature singularity and were able to locate the critical point and determine

[181] It is remarkable that several of the authors of papers on the Lenz-Ising model later became famous for their contributions to nuclear and elementary particle physics, areas quite different from the "crystal statistics" of the Lenz-Ising model. These include C.N. Yang, Y. Nambu, W. Lamb, and E. Teller.

[182] For instance, Newell (1950).

[183] Yang (1952).

[184] For instance, Nambu (1949).

[185] For instance, Kikuchi (1951).

[186] The models were described in Ashkin and Teller (1943) and Berlin and Kac (1952).

[187] For some aspects of this, see Brush (1983), pp. 244–246; Hoddeson, Schubert, Heims, and Baym (1992), pp. 528–533, 572–575, and Bhattacharjee and Khare (1995), pp. 816–821.

[188] This "neutrality" is reflected in the title of their paper: "Statistics of two-dimensional lattices with four components."

the behavior of the specific heat at that point. Like Kramers and Wannier and Onsager, they did not compare their model to any experimental results: Their focus was on its mathematical aspects rather than on its physical interpretation.

Theodore H. Berlin at Johns Hopkins University and Mark Kac at Cornell University together introduced two models in 1952, the Gaussian model and the spherical model,[189] both of which are continuum modifications of the Lenz-Ising model. They employed these models to investigate the nature of the ferromagnetic transition, which (as they noted in their introduction) presents great difficulties, as Onsager's solution shows. They endorsed Onsager's view of models:

> We agree with Onsager that it is desirable to investigate models which yield to exact analysis and show transition phenomena. It is irrelevant that the models may be far removed from physical reality if they can illuminate some of the complexities of the transition phenomena.[190]

In 1964 Kac recalled how he and Berlin created their models. He had attempted to solve the Lenz-Ising problem in three dimensions but concluded that he could not do so, and "in the best mathematical tradition, not being able to solve the original problem, I looked around for a similar problem which I could solve."[191] That problem involved the determination of the partition function in the Gaussian model, so he designed this model to solve a mathematical problem, not to be physically realistic. Their spherical model was a continuation of this work, which they invented to remedy some of the mathematical shortcomings of the Gaussian model.

Berlin and Kac considered the spherical model to be the more realistic of the two, but they indicated their skeptism about its physical realism at the end of their paper:

> The virtue of the spherical model of a ferromagnet is that its properties can be rather extensively discussed and that a three-dimensional lattice has ferromagnetic properties. It is of further interest that the model provides a classical mechanism for the Weiss phenomenological theory.
>
> With respect to the physical ferromagnet, the model has nothing to say positively. We may briefly consider, however, the bearing of our results on the nature of the transition.[192]

Berlin and Kac thus continued the tradition of Kramers and Wannier and Onsager by focusing on the mathematical aspects of their models.

Ashkin and Teller were not so outspoken regarding the lack of physical realism of their model, but they detached their presentation of it from any physical phenomena it might represent, so they too seem to have followed in this same tradition. This illustrates the emergence of a new attitude among physicists towards models in statistical physics in which the exact mathematical solution of a new model was judged to be much more significant than its physical realism. That, of course, was not the *only* attitude physi-

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[189] Whereas Berlin was a physicist by training, Kac had a background in mathematics, an indication of the level of mathematical sophistication needed in the mathematical study of such models.

[190] Berlin and Kac (1952), p. 821.

[191] Kac (1964), p. 41.

[192] Berlin and Kac (1952), p. 827.

cists held after Onsager's solution, but it was one that was held by the highly influential physicists noted above.

In 1953 Newell and Montroll summarized the new attitude that physicists had adopted towards the Lenz-Ising model:

> Actually the widespread interest in the model is primarily derived from the fact that it is one of the simplest examples of a system of interacting particles which still has some features of physical reality in it. The model forms an excellent test case for any new approximate method of investigating systems of interacting particles. If a proposed method cannot deal with the Ising model, it can hardly be expected to be powerful enough to give reliable results in more complicated cases.
>
> Underlying the interest in this problem as a study of some physical model, there rests the more fundamental question. Does the formalism of statistical mechanics predict phase transitions and, if so, how? We can hardly give satisfactory answers to these questions without examples. Even an artificial example is better than none. So far only a few examples have been successfully studied. One of these is the famous Einstein-Bose gas condensation and another is Onsager's brilliant analysis of the two-dimensional Ising model. A third is the spherical model of cooperative phenomena. The model and not some mathematical approximation is in each case the sole cause of the phase transition represented mathematically by a singularity in some of the thermodynamic quantities.
>
> Even though the Ising model is not considered to be a very realistic model of ferromagnetism, it is equivalent to a very good model of a binary alloy and an interesting model of a gas or liquid.[193]

Discussion

Three questions deserve consideration. First, why did the Lenz-Ising model survive these tumultuous years between 1920 and 1950 despite the high odds that it simply would be forgotten? Second, is my claim correct that its development during this period was driven entirely by theoretical considerations and not by experimental results? Third, was the attitude of physicsists towards the modelling process really as novel as I have claimed above?

Brush (1967) has suggested that the use of the Lenz-Ising model (or its mathematical equivalent) in the field of alloys may have saved it from oblivion. It was studied thoroughly in this field, as we have seen, and important results were obtained. However, if with Domb (1996) we accept the reasonable assumption that it owes its prominent place in modern physics to Onsager's solution because it displayed its potential in such an important area as critical phenomena, the picture becomes somewhat different, since that raises the question of Onsager's motivation. His solution was inspired by the mathematical results of Lassettre and Howe (1941) for alloys, and of Kramers and Wannier

[193] Newell and Montroll (1953), pp. 353–354.

(1941a, 1941b) for ferromagnetism and transition points. Onsager (1971) commented on his inspiration in light of Kramers and Wannier's work:

> He [Elliott Montroll] brought the news of this development on the Ising model, that Wannier had brought home from Holland after working with Kramers. Well, this intrigued me considerably; by that time the methods they had been using were a little more advanced than the ones I'd been playing with, which got very near to using a transfer matrix.[194]

Onsager thus was already trying to solve the Lenz-Ising model when he heard about Kramers and Wannier's work, which suggests that his work should not be seen as a direct continuation Kramers and Wannier's or Lassettre and Howe's. His reasons for studying the Lenz-Ising model may well have been different from those of his predecessors. I shall argue that since Onsager was mainly interested in describing transition points of continuous transitions, and was not concerned with alloys or ferromagnets, his focus on transition points was responsible for its survival. Onsager no doubt was aware of Peierls's proof that the Lenz-Ising model shows nonzero spontaneous magnetization at zero temperature in two dimensions. I therefore claim that it was rescued from oblivion because of Peierls's proof in combination with Onsager's (and secondarily of Kramers and Wannier's) interest in a mathematical description of transition points, and not, as Brush (1967) suggested, because of developments in the field of alloys.

Several other physicists shared Onsager's, Kramers's, and Wannier's interest in transition points beginning in the 1930s: Paul Ehrenfest published his famous classification of phase transitions in 1933, and Lev D. Landau published his famous theory of phase transformations in 1937 (although he announced it in 1936).[195] Why did they and other physicists become interested in transition points then? One explanation is that following the creation of quantum mechanics in the 1920s, physicists turned to other areas of research in the 1930s, one attractive area being the study of transition points since a large body of experimental results on diverse systems exhibiting transition points was being accumulated. Moreover, as exemplified by cooperative phenomena, physicists realized that these diverse systems shared certain characteristic features and could be classified accordingly. Thus, not only was there a host of experiments that required explanation, a new classification scheme had be to understood as well. Transition points also gave rise to significant theoretical concerns: Approximations had to be involved to treat the various models or theories, so were these approximations responsible for the existence of transition points or were they a consequence of the models or theories? And, was it possible for the Gibbs partition function to exhibit a discontinuous transition point?[196] These must have seemed to be fundamental questions at the time.

The physical interpretation of the Lenz-Ising model and the reasons for its study, as we have seen, changed considerably between the time that Lenz and Ising proposed it and when it was used in the context of cooperative phenomena. Comparisons to experimental results were almost absent during this period, except that in the context of ferromagnetism the model should show spontaneous magnetization or, in the theory of cooperative

[194] Onsager (1971), p. xxi.
[195] Hoddeson, Schubert, Heims, and Baym (1992), pp. 510–511, 522–523.
[196] Dresden (1988), p. 30.

phenomena, it should exhibit a transition point. But if its development was not a response to experimental results, what was the reason for it?

Even initially its development was not driven by discrepancies between the model and experiments. Lenz and Ising realized that Weiss's theory, which was based on the hypothesis of a molecular field, was able to explain a number of experimental results on paramagnetic and ferromagnetic materials; instead, they raised theoretical objections to Weiss's theory. Lenz criticized Weiss's assumption of free rotatability, arguing that it was not compatible with current theories of solids. They (and others) found the hypothesis of a molecular field formally acceptable, but not theoretically satisfactory owing to the absense of a physical explanation for it. To remedy this deficiency, they proposed a model based on what they considered to be more correct assumptions. Thus, Lenz gave a careful and complex argument for one of its basic assumptions that involved observations, the old quantum theory, and the theory of solids, while Ising did not give much of an argument for its other basic assumption but on the basis of both attempted to construct a simple model to explain ferromagnetism. Nonetheless, Ising himself admitted that it failed to display basic properties of ferromagnetism. His erroneous extension of his result to three dimensions seems to have been dismissed by some of his contemporaries, such as Pauli and Heisenberg.

After the creation of the new quantum theory in 1925, the elementary magnets in the Lenz-Ising model were reinterpreted as arising from the spin angular momentum of the electrons instead of their orbital angular momentum – a move that was possible because the model does not depend on the detailed properties of the elementary magnets. It then turned out that the model violated a fundamental principle of the new quantum theory, noncommutativity. Moreover, when it was compared to the more realistic Heisenberg model, it fell short and was rejected as a realistic model of ferromagnetism. Ising's conclusion that it was unable to explain ferromagnetism, however, seems not to have played an important role in its rejection; instead, its negative reception seems to have been shaped by its conflict with the new quantum theory and a desire to find a more realistic model.

The most striking change in the early fortunes of the Lenz-Ising model, however, probably was the increase in interest in it in the late 1930s and early 1940s concurrently with the decrease in interest in it as a model of ferromagnetism. I described the increasing interest in transition points in the 1930s and argued that the reason for the increasing interest in the Lenz-Ising model was physicists' realization that a more general theory of cooperative phenomena presented formidable mathematical difficulties. Because it was known to exhibit a transition point, which was the focus of interest in studying cooperative phenomena, its exact mathematical treatment became *en vogue* as a means of checking that its transition point arose from its intrinsic properties and not from the particular approximations employed in its analysis. This new motivation had significant implications. Even though physicists recognized that the Lenz-Ising model was, as Onsager wrote, "far removed" from real materials, and even though they acknowledged that it was less realistic than the Heisenberg model, they also realized that precisely because it struck a balance between realism and unrealism, it could be put to good use. Thus, whether or not the degree of its realism was deemed important depended upon the specific purpose physicists had for studying it. They did not consider it to be of much use in capturing the behavior of ferromagnetic materials, but they regarded it of great interest for studying transition points and the mechanisms that generate them.

Is this attitude of physicists towards models really new? One could argue that Kramers and Wannier's and Onsager's attitude towards the Lenz-Ising model did not differ much from that adopted by nineteenth-century physicists toward models in kinetic theory.[197]

Stephen G. Brush has shown that in the nineteenth century physicists devoted much attention to the problem of computing observable properties of gases from their constituent molecules. Thus, James Clerk Maxwell (1831–1879), in his second kinetic theory of gases of 1866[198] (I will return to his first theory shortly), assumed that gas molecules are point-like and repel each other with an inverse-fifth power law of force, first, because all other force laws did not predict that the viscosity is proportional to the absolute temperature, which was thought to be the case experimentally, and second, because it greatly simplifed the mathematical treatment. That was significant because, in modern parlance, it enabled him to the calculate the desired transportation coefficients without having to know the velocity-distribution function explicitly.

It is not clear to me in reading Maxwell's paper which of these two reasons was the more important to him,[199] but if we assume for the sake of argument that he chose the inverse-fifth power law because it simplified the mathematical treatment, then we might ask: How did Maxwell's approach in his kinetic theory differ from that of Kramers and Wannier and Onsager in analyzing the Lenz-Ising model? Was not an ideal system chosen in both cases for the same reason? I suggest that the fundamental difference was that Maxwell's idealization did not violate a more fundamental molecular theory at that time, while Kramers and Wannier and Onsager knew that the Lenz-Ising model was in conflict with Heisenberg's fundamental theory of ferromagnetism.

Maxwell presented his first kinetic theory at a Meeting of the British Association in 1859 (it was published in the *Philosophical Magazine* in 1860), in which he assumed, as others had before him, that gas molecules behave like billard balls and undergo elastic collisions.[200] He broke with this assumption in his second theory of 1866, but he continued to use his first theory (at least once, in 1873). Moreover, his contemporary Oscar E. Meyer accepted and stuck to the billiard-ball model even after Maxwell's second theory appeared, and when a discrepancy arose between it and experiment, he suggested that the internal structure of the gas molecules should be taken into account.[201] In sum, Maxwell could not violate a fundamental theory because there was none at the time to violate, whereas Kramers and Wannier and Onsager deliberately violated a fundamental physical theory.

The study of the Lenz-Ising model was not the only cause for the increasing focus on mathematical models in statistical physics. Developments in the theory of gas con-

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197 I am grateful to an anonymous reader for posing this question on the difference between these attitudes.

198 Published as Maxwell (1867).

199 In his writings, Brush seems to weigh the reason of the mathematical simplicity higher than the reason concerning the compatibility with experiments; see, for instance, Brush (1966), p. 5; and Brush (1983), pp. 206–207.

200 See, for instance, Brush (1983).

201 Brush (1976).

densation, initiated by Joseph E. Mayer and pursued by George Uhlenbeck, Boris Kahn, and others,[202] may have been equally or even more important in this trend. Kramers and Wannier mentioned that the theory of gas condensation was the only theory in which physicsists had attempted to prove that the existence of a transition point follows from a statistical treatment. Nevertheless, Onsager's proof that the Lenz-Ising model offered the possibility of a mathematical treatment of ferromagnetism surely reinforced this trend. At the very least, the development of the Lenz-Ising model played a significant role in the emergence of mathematical models in statistical mechanics.

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