

Published by

World Scientific Publishing Co. Pte. Ltd.

5 Toh Tuck Link, Singapore 596224

USA office: Suite 202, 1060 Main Street, River Edge, NJ 07661

UK office: 57 Shelton Street, Covent Garden, London WC2H 9HE

Library of Congress Cataloging-in-Publication Data

Mattis, Daniel Charles, 1932–

Statistical mechanics made simple : a guide for students and researchers / Daniel C. Mattis.

p. cm.

Includes bibliographical references and index.

ISBN 981-238-165-1 -- ISBN 981-238-166-X (pbk.)

1. Statistical mechanics. I. Title.

QC174.8.M365 2003

530.13--dc21

2003042254

British Library Cataloguing-in-Publication Data

A catalogue record for this book is available from the British Library.

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Printed in Singapore.

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Preface

I dedicate this book to those generations of students who suffered through endless revisions of my class notes in statistical mechanics and, through their class participation, homework and projects, helped shape the material.

My own undergraduate experience in thermodynamics and statistical mechanics, a half-century ago at MIT, consisted of a single semester of Sears' *Thermodynamics* (skillfully taught by the man himself.) But it was a subject that seemed as distant from "real" physics as did poetry or French literature. Graduate study at the University of Illinois in Urbana-Champaign was not that different, except that the course in statistical mechanics was taught by the brilliant lecturer Francis Low the year before he departed for... MIT. I asked my classmate J.R. Schrieffer, who presciently had enrolled in that class, whether I should chance it later with a different instructor. He said not to bother — that he could explain all I needed to know about this topic over lunch.

On a paper napkin, Bob wrote " $e^{-\beta H}$ ". "That's it in a nutshell!" "Surely you must be kidding, Mr Schrieffer," I replied (or words to that effect.) "How could you get the Fermi-Dirac distribution out of THAT? "Easy as pie," was the reply^a. . . and I was hooked.

I never did take the course, but in those long gone days it was still possible to earn a Ph.D. without much of a formal education. Schrieffer, of course, with John Bardeen and Leon Cooper, went on to solve the statistical mechanics of superconductors and thereby earn the Nobel prize.

The standard book on statistical physics in the late 1950's was by T. L. Hill. It was recondite but formal and dry. In speaking of a different text that was feebly attempting the same topic, a wit quipped that "it was not worth

^aSee Chapter 6.

a bean of Hill's." Today there are dozens of texts on the subject. Why add one more?

In the early 1960's, while researching the theory of magnetism, I determined to understand the two-dimensional Ising model that had been so brilliantly resolved by Lars Onsager, to the total and utter incomprehension of just about everyone else. Ultimately, with the help of Elliot Lieb and Ted Schultz (then my colleagues at IBM's research laboratory,) I managed to do so and we published a reasonably intelligible explanation in *Reviews of Modern Physics*. This longish work — parts of which appear in Chapter 8 — received an honorable mention almost 20 years later, in the 1982 Nobel lecture by Kenneth G. Wilson, who wrote:

"In the summer of 1966 I spent a long time at Aspen. While there I carried out a promise I had made to myself while a graduate student, namely [to work] through Onsager's solution of the two-dimensional Ising model. I read it in translation, studying the field-theoretic form given in Lieb, Mattis and Schultz [s paper.] When I entered graduate school I had carried out the instructions given to me by my father and had knocked on both Murray Gell-Mann's and [Richard] Feynman's doors and asked them what they were currently doing. Murray wrote down the partition function of the three-dimensional Ising model and said it would be nice if I could solve it. . . . Feynman's answer was "nothing." Later, Jon Mathews explained some of Feynman's tricks for reproducing the solution for the two-dimensional Ising model. I didn't follow what Jon was saying, but that was when I made my promise. . . . As I worked through the paper of Mattis, Lieb and Schultz I realized there should be applications of my renormalization group ideas to critical phenomena. . . ."^b

Recently, G. Emch has reminded me that at the very moment Wilson was studying our version of the two-dimensional Ising model I was attending a large IUPAP meeting in Copenhagen on the foundations and applications of statistical mechanics. My talk had been advertised as, "The exact solution of the Ising model in three dimensions" and, needless to say, it was well attended. I did preface it by admitting there was no exact solution but that — had the airplane taking me to Denmark crashed — the title alone would have earned me a legacy worthy of Fermat. Although it was anticlimactic, the actual talk^c demonstrated that in 5 spatial dimensions or higher, mean-field theory prevails.

^bFrom *Nobel Lectures in Physics (1981–1990)*, published by World Scientific.

^cIt appeared in the Proceedings with a more modest title befitting a respectable albeit approximate analysis.

In the present book I have set down numerous other topics and techniques, much received wisdom and a few original ideas to add to the “hill of beans.” Whether old or new, all of it *can* be turned to advantage. My greatest satisfaction will be that you read it here first.

D.C.M.
Salt Lake City, 2003

