Some other Pergamon Titles of Interest

LANDAU and LIFSHITZ
Course of Theoretical Physics (9 volumes)

LANDAU and LIFSHITZ
A Shorter Course of Theoretical Physics (2 volumes)

LANDAU *et al.*
General Physics – Mechanics and Molecular Physics

JOHNSON
Men of Physics – Karl Lark-Horovitz

LINDSAY
Men of Physics – Julius Robert Mayer

LINDSAY
Men of Physics – Lord Rayleigh

ROSENFELD
Men of Physics – Irving Langmuir

SEEGER
Men of Physics – Benjamin Franklin

SEEGER
Josiah Willard Gibbs – American Mathematical Physicist *par excellence*

SEEGER
Men of Physics – Galileo Galilei

TER HAAR
Men of Physics – L. D. Landau, Vol. 2

Related Pergamon journals*

Endeavour
Studies in History and Philosophy of Science

*Free specimen copies available on request

Landau

A Great Physicist and Teacher

by

ANNA LIVANOVA
Moscow, U.S.S.R.

Translated by
J. B. SYKES
Oxford, England

From *L. D. Landau,* published by Znanie, Moscow, 1978 (Series: Tvortsy Nauki i Tekhniki)



PERGAMON PRESS
OXFORD · NEW YORK · TORONTO · SYDNEY · PARIS · FRANKFURT

U.K.	Pergamon Press Ltd., Headington Hill Hall, Oxford OX3 0BW, England
U.S.A.	Pergamon Press Inc., Maxwell House, Fairview Park, Elmsford, New York 10523, U.S.A.
CANADA	Pergamon of Canada, Suite 104, 150 Consumers Road, Willowdale, Ontario M2J 1P9, Canada
AUSTRALIA	Pergamon Press (Aust.) Pty. Ltd., P.O. Box 544, Potts Point, N.S.W. 2011, Australia
FRANCE	Pergamon Press SARL, 24 rue des Ecoles, 75240 Paris, Cedex 05, France
FEDERAL REPUBLIC OF GERMANY	Pergamon Press GmbH, 6242 Kronberg-Taunus, Pferdstrasse 1, Federal Republic of Germany

---

Copyright © 1980 Pergamon Press Ltd.

All Rights Reserved. No part of this publication may be reproduced, stored in a retrieval system or transmitted in any form or by any means: electronic, electrostatic, magnetic tape, mechanical, photocopying, recording or otherwise, without permission in writing from the publishers.

First edition 1980

British Library Cataloguing in Publication Data

Livanova, Anna
Landau.
1. Landau, Lev Davidovich
2. Physicists — Russia — Biography
I. Title
530'.092'4 QC16.L25 79—42762

ISBN 0—08—023076—8

Printed and bound in Great Britain by
William Clowes (Beccles) Limited, Beccles and London

Dau Away From Physics *188*

Appendix:

Landau's plain talk to students of physics *199*

Index 215

Acknowledgements

The publishers acknowledge the use of short extracts from the following works already published in English:

American Journal of Physics (by courtesy of Dr J. G. Adashko)
Andronikashvili: *Supplement to 'Helium'*
Biographical Memoirs of Fellows of the Royal Society (by courtesy of Professor P. L. Kapitza and the Royal Society of London)
Dirac: *The Development of the Quantum Theory*
Einstein: *The World As I See It*
Feynman: *The Character of Physical Law* (by courtesy of Professor R. P. Feynman)
Feynman: *Lectures in Physics*
Kapitza: *Collected Papers*
Keesom: *Helium*
Landau: *Collected Papers*
Lifshitz: Lev Davidovich Landau (in *Mechanics*)
Lorentz and Einstein: *The Principles of Relativity Physics Today*
Rogers: *Physics for the Inquiring Mind* (by courtesy of Princeton and Oxford University Presses)
Soviet Physics JETP
Soviet Physics Uspekhi
Zeitschrift für Physik

The translator acknowledges with gratitude the help of Mrs. Dorothy Eagle in writing both the text and the index.

Preface by Sir Rudolf Peierls

"DAU", or Lev Davidovich Landau, whose image this book is trying to re-create, was one of the great physicists, and one of the great characters of our generation. I had the good fortune to know him well, and we became good friends, although later the War, and other tensions, prevented us from seeing much of each other.

I vividly remember the great impression he made on all of us when he appeared in Wolfgang Pauli's department in Zürich in 1929, where as a fresh PhD I was working as Pauli's "Assistent". He was even a little younger, and quite unknown. But it did not take long to discover the depth of his understanding of modern physics, and his skill in solving basic problems. He rarely read a paper on theoretical physics in detail, but looked at it long enough to see whether the problem was interesting, and if so, what was the author's approach. He then set to work to do the calculation himself, and if the answer agreed with the author's he approved of the paper.

Evidently one could learn much from discussion with him, and from work we did jointly. But physics, however exciting in those days of the early applications of the new quantum mechanics, was by no means the sole subject of our conversations. He was interested in the world around him, in people and their relations, and in the way of life of the West. He approached all these matters with the same attitude with which he looked at problems in physics, theorizing, labelling, classifying. He had started his classification of physicists according to their greatness, though this became refined much later. He certainly had started to assess "situations", which was his word for relations between men and women. There were strict criteria to be met for a situation to be regarded as satisfactory. If he found

amongst his friends or acquaintances an unsatisfactory situation, it was of course his duty to point this out to the couple concerned – who did not always react kindly.

He was sure that progress in physics depended only on young people, and once, when he heard mention of a youngish theoretical physicist of whom he knew nothing, he made his famous remark "What, so young and already so unknown?"

He joined the group of physicists in all their activities, including skiing at the weekends, though he was not a sporting type. I still cherish a snapshot of him on skis, looking gaunt and a bit lost. He was always very thin, and looking more frail than he was.

He was interested in the political institutions of the West, and liked to make fun of some of their features. His first stay in Zürich had to be cut short, because the Swiss authorities, who at the time had no diplomatic relations with the Soviet Union, gave him a permit for only a short time, and then renewed it for shorter and shorter periods, until he finally had to leave. He commented with some pride: "Lenin lived in Switzerland for years and did not succeed in starting a revolution, but they evidently think I could!"

A year later he was back with a Rockefeller Fellowship, and there was no difficulty about his residence permit. During this second visit we did another joint piece of work, concerned with the foundations of quantum mechanics, and their relation with relativity. The initiative in this was certainly Landau's. The subject was one in which the great Danish theoretician Niels Bohr was profoundly interested, and he violently disagreed with our conclusions. Later that year when both Landau and I were visiting Copenhagen, the discussions with Bohr became very heated, in spite of our great respect for him. But this did not diminish Bohr's specially warm affection for Landau.

By this time he was making important contributions to many branches of physics. His work on the diamagnetism of conduction electrons solved in a simple and elegant way a puzzle that had confused many old hands. His way of looking at collisions between atoms in which they exchange energy or undergo

chemical reactions has remained the accepted view. He intervened in a controversy between astrophysicists about the stability of stars, and was the first to point out that the known laws of physics allow no stability for a cold mass more than about 50% greater than that of the sun, a conclusion linked today with the existence of "black holes".

This is only a selection from the list of topics to which he started making lasting contributions. I still had several occasions to exchange ideas with him, and to learn from his penetrating thought and from his powerful intuition, on various visits to the Soviet Union. Intuition was for him an important tool, in spite of his great skill in solving problems mathematically. He regarded it as pedantic and unnecessary to look for a rigorous proof of a statement that was intuitively obvious. If one complained that something he relied on was not obvious, he would say, or at least imply: "Well, if this is not obvious to you, you should not be a physicist." A little of this attitude rubs off even on the excellent *Course of Theoretical Physics,* and has sometimes discouraged timid souls amongst the readers, though it helps to avoid formal details which could prevent one seeing the wood for the trees.

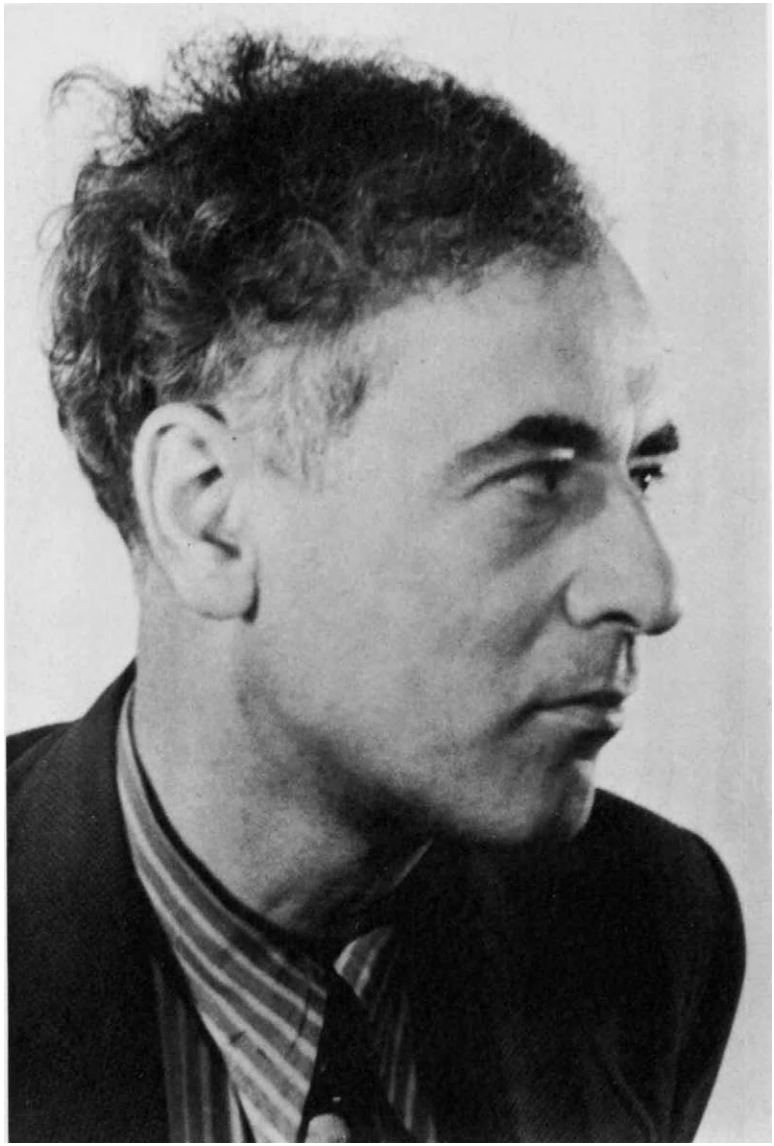
I recall one conversation on one of the visits, actually a walking tour in the Caucasus with him and one of his friends in 1934, which shows Landau's vision. The friend, an engineer, asked "What is this we read about nuclear energy? Is this just science fiction, or is there a real possibility?" Without hesitation, Landau replied: "It is a difficult problem. There are nuclear reactions in which energy is released, but we can initiate them only by bombardment with charged particles. But most charged particles passing through matter are slowed down before they hit a nucleus, and therefore one wastes much more energy than is gained. Neutrons pass through matter without slowing down, but at present the only way we have for producing neutrons is by charged-particle bombardment, so we are back with the same difficulty. But if some day someone discovers a reaction initiated by neutrons, which releases secondary neutrons as well as energy, you are all set." This was barely two years after the

discovery of the neutron, and long before the discovery of fission set people thinking about chain reactions.

He was not a great letter writer, and I am not the most regular of correspondents, so we did not keep in regular touch. I still saw him at a conference in Moscow in 1937, when his physics was going strong, but he was very worried about the worsening international situation and the resulting tensions within the U.S.S.R., with its spy mania and other tensions. This was shortly before he was himself in trouble with the Stalin regime.

I saw him again at a Moscow conference in 1956, surrounded by his pupils and collaborators. He had by now become the great teacher so vividly described by Anna Livanova, but as a person he had not changed. Two more brief encounters at conferences, and then the tragic accident which ended his work as a physicist and teacher, and made his life for the remaining years a misery.

His great contributions to physics, and to the teaching of physics, and his unusual and fascinating personality, are indivisible. His papers, his intervention in discussions, and his behaviour on the many occasions described by Anna Livanova, are all aspects of the same pattern, to those who knew him all unmistakable "Dau".



L. D. Landau

An Introduction

LEV DAVIDOVICH LANDAU was one of the greatest of Russian scientists. He was also, and remains, a personality arousing widespread interest. So it happened that his high level of creativity has become a moral peak too. This was especially clearly felt by physicists and other scientists, but, like any great phenomenon, it has also affected society as a whole.

Landau played a very important part in establishing theoretical physics in the Soviet Union. His pupils remark that he did not concern himself with mankind's ethical problems as such, but he did possess a very high moral potential. He evolved a style of correct relationship to science and was always on guard to protect the most lofty principles. In this respect he was resolute in making no concessions to anyone. Everything must be assessed with complete fairness. A piece of work was appraised by one thing only – its "genuineness". Landau could not endure and did not permit sycophancy. But friendship and affection too were unable to alter his system of appraisal.

What is elementary and simple can be understood by many more people than what is complex, abstract, and in need of effort for its understanding. Yet the former has its followers, often militant, and sometimes wielding much power. What is simple is easily explained, and so gains support for itself. The inaccessibility of what is complex is always plausibly explained as being due to incorrectness or unnaturalness, so that one's own failure to understand it is made out to be a heroic defence of truth and purity.

It may be that Landau's special significance and his special place in our memory exist because by his work, his presence and actions and those of his school, he raised the level of intellectual demands and culture in science and in all that relates to it.

Though seeming to some a person aloof from ethical questions, he became something of a moral paragon by his purely scientific and professional work. It is not necessary to compare scales of values, but as the thought of Pushkin prohibits one from approving of bad poetry, or justifying what is mean or vile or even simply undistinguished, so in thinking of Landau one cannot do similarly in physics or in science as a whole.

In all his actions Landau was essentially defending physics against a lowering of standards, defending its very nature against debasement. The strength of the edifice that he constructed was evident even to those who would never even contemplate dwelling in it. It inspired, too, all who fought for a high standard and for perfection in other branches of science and culture. The movement of thought was like the movement of a liquid in communicating vessels.

Genuine science is essentially moral. Its motto and principle is to question Nature, not to impose upon her the answers that we might find desirable or convenient. A hundred and fifty years ago Lobachevsky, in his speech "The chief purposes of education" before the final-year students at the University of Kazan, emphatically repeated Bacon's appeal: "Ask of Nature, she preserveth all truths, and shall reply fully and without fail to thy questionings."

That is one path in science, one approach to the study of and relationship with Nature. But there is another quite different, even contrary approach. Both have been followed for centuries. The second is the "alchemical" approach. The alchemists wanted gold from Nature. Later, other adepts of this persuasion began to demand other benefits; always to demand – the hard line, as it would now be called.

The true scientist is distinguished also by the ability to "ask of Nature". Therein lies his talent or genius. Few possess it, just as few are outstanding writers or artists or composers. This is why every period in the history of intellect has usually been dominated by a few persons, a few names.

Those whose effort and creativeness stand at a high level and who pursue only the discovery and defence of the truth, in

whatever field of inward life, science, literature, teaching – they become the conscience of society and the yardstick of its spiritual capabilities. And when they are not alone, but carry with them others, their pupils, whom they train in the same principles, they are especially significant and valuable. For this reason, the existence and activity of Landau and his school counteracted the vulgarization of science.

Landau's work was most tangibly embodied in his contribution to physics: his own publications; the creation of a school, and the work of his pupils; the *Course of Theoretical Physics*, the volumes of which are consulted by every physicist in the world. In turn, the expression "the school of Landau" acquired a great and many-sided significance: the seminar, which became a continuous debating-ground for theoretical physicists; the "theoretical minimum", which was both a training programme for the qualified theoretical physicist of today, and at the same time a barrier that only the strongest contenders could surmount.

Landau was deeply concerned with the development of physics in his country. He felt a personal responsibility in this connection. This attitude of a good citizen took concrete shape all through his life. For example, in addition to his own school of theoretical physics, Landau helped to initiate the setting up of the Moscow Physicotechnical* Institute, an educational foundation on advanced principles and in close relationship with the latest developments of science. He was very active in bringing this Institute into existence, and lectured there every year.

He proposed to write physics textbooks at various levels, which could be used by everyone from schoolchildren to high-level theoreticians. He established the principles of mathematics textbooks designed for physicists. He did not achieve all that he planned, but he did achieve a great deal.

Yet with all this Landau had no ambience of importance or loftiness. On the contrary: at the celebration of his fiftieth

*So called because established for work on both pure and applied physics. – *Translator.*

birthday, Alexander Shalnikov called him "the most unpretentious of men".

He was the subject of continual anecdotes and comical stories, which he seems to have taken in good part. He himself liked to be given a label and to think out all kinds of inscriptions. In the early 1930s, for example, he pinned to the door of his office a small notice "Take care. He bites." In the 1940s, he was rather fond of saying "I'm a Christian now. I don't eat anyone." There was a grain of truth in this. He did in fact soften, became less sarcastic and less aggressive in his opinions. But only as regards their form. It was therefore hardly necessary to take these words very seriously. Similarly with his frequent and ostensibly sincere protestations that he was a coward; he did not generally behave like one.

Pyotr Kapitza recalls: "In his evaluation of scientific achievement or of the scientists themselves, Landau was always ready to give his appraisal, which was usually witty and clearly stated. The wit was especially evident in his negative assessments. These were quickly passed round and finally reached the subject under criticism, which naturally complicated Landau's relationships with people, especially when the subject of his criticism occupied a responsible position in academic circles."

In his written opinions, too, Landau used to be quite sharp, and not just a correct exponent of principle. His attitude to other physicists and especially, of course, to theoreticians may have been determined mainly and most forcefully by a correlation between their "absolute worth" (for which Landau naturally used his own scale with very severe standards) and their own estimate of themselves. The greater the discrepancy (again, as Landau saw it) the more caustic and uncompromising he became. If we take as a parameter this "scientific worth on the Landau scale", a very clear relationship is obtained. When a particular person rated himself as a physicist in the same way as Landau rated him (a slight difference was, of course, admissible), everything was in order and there could be normal good and respectful relations. But someone might think of himself as an unrecognized genius, or more often, be sure that his high rank

was commensurate with his scientific merits; if Landau, on the other hand, regarded these merits as distinctly modest, he did not hesitate to say so with considerable relish.

With his pupils and those who shared his views, Landau was still very strict, even captious. The expression "to get something past Landau" was current. It speaks for itself. Whatever got past him had to be impeccable.

Of course it would be absurd to suppose that Landau was some kind of abstraction or ideal embodiment of the highest qualities, for those close to him, friends, pupils, other physicists, non-physicists, with whom he may have had dealings. In the first place, he was a living person with a very unusual character. No doubt every great scientist is unique as a person too. Perhaps in Landau the unusual side was particularly obvious; in some ways he was extraordinary. He was an exceptional and unique personality, the celebrated Dau. Those who loved him loved this entirely real Dau. And after his accident it was their Dau that they saved, not some abstract value. Yet perhaps there was also a realization, implicit, not expressible in words, of his abstract worth, his significance in our lives. This significance was probably felt instinctively even by those who did not have and could not have any conception of his work. Otherwise, whence came that broad, widespread and yet somehow very personal interest in his fate, which was so evident after the accident, as misfortune befell him, and as he lay dying? Whence the agitation and anxiety with which people continually inquired after his condition, now daring to hope, now awaiting with grief the tragic end?

This book is meant for those who felt Landau's tragedy as a great personal loss; for those whose proper understanding of him was sometimes obscured by the anecdotes and stories; for the physicists, especially his pupils – it is their approval or disapproval that the author most tremulously awaits. It is also meant for young people, in the hope that the name of Landau, for ever inscribed in the history of science and of civilization, will be associated with a real human being.

It is only a small book, especially if compared with the whole of Landau's life and creative work. It casts light on only a small part of these.

The author has been helped by Landau's pupils and Institute colleagues, friends of his youth, and relatives; their recollections and suggestions were valuable. Though they knew that only a great writer could produce a book adequate to Landau's unique personality as a scientist and a human being, they yet gave generously of their time; for even the most imperfect and incomplete account may, given the effort to make it a true one, still be of some benefit. The author would say here that she is conscious of the contributions made by all these many advisers, and though not in a position to list them by name she offers them her very deep gratitude.

Another Introduction

"I AM a poet. That is what makes me interesting," said Mayakovsky.

Landau was a physicist. A theoretical physicist. In many ways exceptional, unlike the rest. That is what makes him interesting. That is why one cannot write about him without discussing the nature of his work and indicating its place in the vast territory of modern physics.

Just as one cannot discuss a poet and pretend that his poetry exists apart from him and may be either discussed too or left for another occasion, so one cannot discuss a scientist and pretend that his work in science is something external to him, a sort of garment that can be taken off, hung up in a cupboard, and forgotten. Nor can one merely mention its existence, or give just a superficial description of it in passing.

"In the vast edifice of physical science, there were for him no locked rooms," said Academician Alexander Andronov of his teacher, Leonid Mandelstam, who at that time had already spent a long life in science, and from his published works one can perhaps see him successively unlocking those rooms – door after door. But, for Landau as a young man, almost a boy, they were wide open from the start, or sprang open as soon as he approached them.

In the 1930s Landau was already saying, "I am one of the few universal physicists"; after the death of Enrico Fermi, this became "I am the last of the universal physicists". This was not conceit, nor was it only his own opinion. The same thing was noted and emphasized by his colleagues, at different times and on different occasions. They were remarkably unanimous about it. Here are some opinions, which will not even be attributed.

"A characteristic feature of Landau's scientific creativity is

its almost unprecedented breadth, which encompasses the whole of theoretical physics, from hydrodynamics to the quantum field theory. In our century, which is a century of increasingly narrow specialization, the scientific paths of his pupils also have been gradually diverging, but Landau himself unified them all, always retaining a truly astounding interest in everything. It may be that in him physics has lost one of the last great universalists."

"His enormous creative potential, his very wide range of interests, his universalism (rare in this age of narrow specialization), make Landau a kindred spirit to the great men of the Renaissance."

"Literally without moving, without turning to any publications, at any moment, Landau was able to start work on a topic that had attracted his attention, in any branch of theoretical physics."

"Only by mastering the modern style, which found such a vivid and finished expression in Landau's papers and textbooks, can one retain an understanding of developments in almost the whole of theoretical physics: considering today the theory of superfluidity, tomorrow quantum field theory, the next day the theory of metals. Landau is such a person, and he has helped his pupils, both in person and by correspondence, to follow the same road."

The quotations could be continued, but it is already clear that Landau's field of activity was almost the whole of theoretical physics. It would give a distorted picture of Landau the scholar if that field were greatly narrowed: this universalism, as well as other singular attributes, was evidence of his unusualness and his peculiar talent.

So, we can only repeat that, as regards Landau, in the vast edifice of physical science, there were for him only open rooms.

Professor N. D. Mermin, writing in the American journal *Physics Today,* has thus described Landau's scientific work: "That massive volume, *The Collected Papers of L. D. Landau,* stirs feelings not unlike those evoked by the complete plays of William Shakespear, or the Köchel catalogue of W. A. Mozart.

The immensity of the total achievement of one man is almost beyond belief."

However, we see now the other side of the coin. How if the subject and level of Landau's work are accessible only to the few? "To try to give a non-physicist an idea of Landau's scientific creativity in a few brief remarks is bound to fail, and should not be considered," was the decided view of Yury Rumer, a friend of Landau's youth and his co-author.

One might grasp at the saving words "brief remarks". In physics, we might say, there are only very relative and often vacuous definitions, for instance of small and large, short and long, brief and lengthy, if the process concerned is not specified. And a book of modest size would have to content itself with brief remarks, with unintelligible patter, if it tried to deal with all – or much – of what Landau achieved in science.

The only feature common to all Landau's work is that it is of the highest quality. In other respects, it differs, and not only in its subject matter. For instance, in resolving some problems, Landau created his own method, never previously used. In others, he applied ideas and methods from what seemed to be totally distinct branches of physics, unrelated to the one in question; this too was a characteristic of his creative style.

In his period of intensive work, Landau attacked and resolved a multiplicity of problems, some of them relating to just one particular limited question posed by physics. One thing he never suffered from was "scholarly snobbery", the desire to concern himself only with large problems. On the contrary, he despised such an approach to science, and scoffed at those who aimed at making great discoveries. He did, however, always say that one's work should contribute something new and be free of unsupported speculation, "philology" as he liked to call it. "The brevity of life does not allow us the luxury of spending time on problems which will lead to no new results," Landau wrote in his last paper, "Fundamental problems".

Sometimes Landau's work was poured into the broad river of a great problem being jointly solved by many theoreticians. It would then be pointless to refer to his work without taking account of the whole problem.

Sometimes his work was to conclude or resolve some dispute; to understand and assess it, one would then have to know the positions taken by the disputants.

Sometimes his work was concerned with particular nuances of a problem, a theory, or a formulation.

It is thus possible by elimination to narrow the range of his investigations, and we shall select those where a popular account may be attempted, though of course only at the level of ideas.

Landau's work, however, did not consist simply of ideas that can be represented in words without recourse to mathematics. On the contrary, he was distinguished by his exceptional grasp of mathematical methods, the theoretician's chief weapon, and this often surprised and delighted those who were able to appreciate it. His work in fact really consisted in this masterly and ingenious use of the tools of his trade, upon the obviously necessary basis of existing ideas.

Of course, every serious theoretical physicist has to be able to use mathematical methods as his ordinary and normal tool of the trade. The pages of any theoretical paper in any physics journal are full of formulae and calculations. But an experienced eye can judge at once what lies behind, whether the formulae have much content, whether they represent an advance, or a new approach to the problem. It may happen also that all this apparent complexity is seen to cover a total lack of ideas, a triviality, a "vibration of the air", as Landau liked to say.

Our story will proceed, then, "at the level of ideas". Nevertheless many new and complex notions will have to be introduced, and their physical significance explained, if only in simplified terms.

On Landau's festive fiftieth birthday, among other ingenious gifts, he was presented with marble tablets on which were engraved "Landau's Ten Commandments": ten formulae which, so to speak, embodied Landau's ten most important achievements (in the opinion of those responsible for the gift; the tablets were delivered by Academician Isaac Kikoin on behalf of the Institute of Atomic Energy).

This was doubtless not an absolute or the only evaluation of his contribution to physics. Variant opinions are possible. But the majority of the Commandments were certainly the most significant things that he had done, and from them we must choose.

We shall take the theory of the superfluidity of liquid helium, for the following reasons.

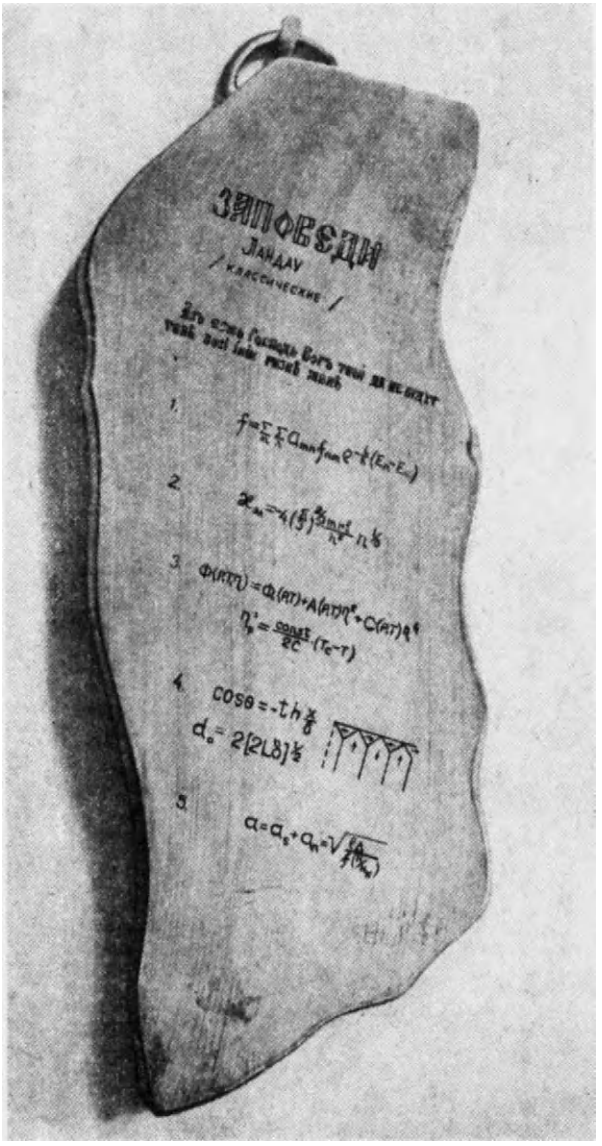
1. This was a discovery in itself, of the greatest value and significance. It opened a new realm in physics, the quantum physics of liquids or, more broadly, of the condensed state, or, what sounds quite astonishing, the quantum physics of macro-objects. The meaning and content of these various terms is that the laws of quantum mechanics are used to describe not atoms or nuclei or elementary particles (for which quantum mechanics was created, since classical physics "would not play"), but macro-systems, which one would have expected to be entirely subject to classical laws. In fact (and this is the point) these macro-objects are in a special state.

2. It was an investigation complete in itself and one that Landau, who began it, also concluded. At the same time it had, and has, many important continuations, and consequences in quite different branches of physics, such as the atomic nucleus, the solid state, and even astrophysics.

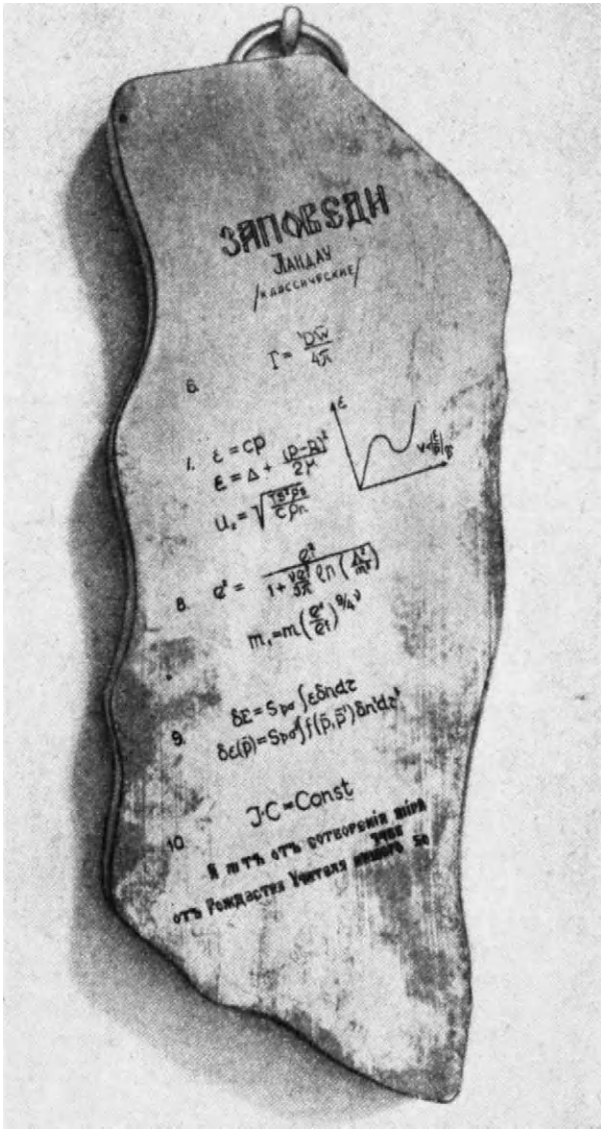
3. It is a case where theory not only explained baffling phenomena which had brought physicists into a blind alley and seemed incapable of explanation, but also predicted new phenomena that were afterwards found by experiment, in exact agreement with the theory.

4. It shows in the clearest form Landau's collaboration with the experimentalists, which was a very characteristic feature of him as a theoretical physicist.

5. It has a special place among Landau's creations. Many of his pupils believe that the theory of quantum liquids is the principal thing in his scientific life. The world scientific community assessed it in the same way. Landau received the Nobel prize "for his pioneering theories concerning condensed matter, especially liquid helium".



Landau's Ten Commandments


ЗАПОВЕДИ
ЛАНДАУ
/КЛАССИЧЕСКИЕ/
6.
1.
8.
9.
10.
J·C=Const

6. Although Landau's chief equipment is considered to be his logic, this work, one of his most notable, showed with exceptional clarity his scientific intuition and the force of his scientific imagination.

There are other reasons too, a final one being that it proved possible to describe this problem in a reasonably intelligible and connected way – again "at the level of ideas", but so that the non-physicist reader should understand its essence, its beauty, and its significance.

It is not inappropriate to relate here a conversation between Landau and one of his pupils, who asked, "Dau, which of your researches do you regard as the best?" "The theory of superfluidity," Landau replied, "because so far nobody has really understood it." There must be an element of jest in this, but how much?

We shall, nevertheless, venture to tell of this theory, beginning at the beginning with the history of the subject and taking the reader gradually upwards, step by step.

Perhaps someone will later establish a more extensive programme of learning about Landau's creative work, for example, relating in detail his interest in the processes whereby matter passes from one state to another, the theory of phases, phase transitions, and phase diagrams, an interest which led to his theory of second-order phase transitions. The content of this theory would have to be explained, and its significance demonstrated, and how and whence it arose, and its connection with other problems of physics (indeed, we shall see how it contributed its mite to the theory of superfluidity also).

Landau was also much occupied with the problem of symmetry: the symmetry of space, the discovery by Lee and Yang of the non-conservation of parity in weak interactions, new difficulties from which he did not turn aside, and the principle of combined parity which he proposed in order to resolve the contradiction (though this was not the final and complete solution of the problem).

He did interesting and valuable work on magnetism, including an early study in which he discovered what is now called

"Landau diamagnetism"; and on nuclear physics, plasma physics, and general topics in quantum mechanics – at least ten "Commandments".

Years, Cities, Institutes, . . .

Baku to Leningrad

LEV DAVIDOVICH LANDAU was born on 22 January 1908 in Baku, on the Caspian Sea. His father, David Lvovich Landau, a petroleum engineer, came from a well-to-do family. He was over forty when his son was born, and had an important position as chief engineer at one of the Baku oil-fields. Lev's mother, Lyubov Veniaminovna Garkavi-Landau, was ten years younger than her husband. She grew up in a poor family, but made her way by assiduous effort, extraordinary persistence, and unusual capabilities. She was mentally a strong and active person, who met adverse circumstances with increased determination. She worked very hard all her life. In 1898 she graduated from the Midwifery Institute in St. Petersburg, and in 1904 from the Women's Medical School there. During her training, she worked at her Institute in the physiology department, and afterwards assisted the pro-rector. Later she was a hospital doctor, and during the first world war worked in a field hospital. She then became a teacher with a wide range of subjects, and undertook practical therapeutic medicine, teaching, and research. Such a variety of interest, knowledge, and ability was rarely found in women of the early twentieth century. She was certainly an exceptional woman. One feels that it may have been her example that gave Landau the foundation of his own versatility, his calling as both scientist and teacher too.

Lev was the second child, having an elder sister Sophia. He completed his schooling at the age of 13. But his interest and ability in the exact sciences, especially mathematics, appeared even earlier.

"I was not a child prodigy," said Landau in conversation with students at the Moscow Physicotechnical Institute in the spring

of 1961. "At school, my compositions were never marked better than 'fair'. I liked mathematics. All theoretical physicists come to science from mathematics, and I was no exception. I knew how to differentiate when I was twelve, and how to integrate when I was thirteen."

Lev's parents felt that at thirteen he was too young to attend university (and he was quite small at that time). He spent a year studying with his sister at a college of economics. Then, in the autumn of 1922, he became a student at the University of Baku, in both the department of physics and mathematics and the department of chemistry.

Landau told one of his friends that when he was only nine he formed a desire to investigate every matter that life brought him into contact with, and everywhere to find his own solutions. Once found, they would rarely be reconsidered. This referred to purely human affairs, of course, not to science. Later, Landau was always surprised that he was not affected in the same way as other people by even the most ordinary of life's conflicts. He felt the need to reinterpret it and construct his systematic attitude. If unable to arrive at a "theory", he satisfied himself with a lower level of systematization, by classification. This was the source of the comment that recurs in stories about him: "Landau liked to classify everything." It proves to date from his early years at school.

At sixteen, Landau moved to Leningrad and entered the physics department of the university there. "Here I had to make a choice: I opted for physics, and have never regretted it."

Landau's "first Leningrad period" lasted about 5 years, until his 18-month trip abroad. Leningrad in the 1920s could rightly be called the scientific centre of the Soviet Union, at least as regards physics. There have been many detailed descriptions of the powerful school of experimental physics established by Abram Ioffe, and of his extensive and many-sided other activities, including purely organizational ones, the setting up of new institutes and laboratories throughout the country. The particular environment and scientific life that accompanied this work

are also well known. Actually, as Kapitza rightly remarks, the senior men – in age, experience, and position – were all experimentalists, with Rozhdestvensky and Ioffe in the forefront, and Leningrad at that time had no outstanding scientists capable of creating a school of modern theoretical physics.

The students led a very unfettered life. "I went to lectures at the university twice a week, so as to meet my friends and see what was happening," Landau tells us. "But I did a great deal off my own bat. So much that I began to dream about formulae."

In 1926, while still a university student, Landau became what was then called a supernumerary graduate student at the Leningrad Physicotechnical Institute, and published his first scientific paper. In those days there was no objection to someone's being an undergraduate and a post-graduate at the same time and indeed in the same subject. Again, conditions were such that one could not really call him a final-year student. Various topics from various years alternated, and examinations were taken in a similar fashion (or not taken at all), so that most of the students simply could not say which year they were in. They usually took part in two or even three years at the same time. All that is certain is that Landau graduated from the University of Leningrad in 1927, at the age of 19.

He then became a regular graduate student at the Institute. At that time a group of theoreticians came together, joined by common interests, and each of them has made his mark in science. Landau also came into contact with "big physics", avidly reading the still "hot" papers on quantum mechanics as soon as they came from the pens of their authors – the creators of the physics of the microworld. Yury Rumer had this period in mind when he said that the most amazing thing about Landau was how he struck out on his own path. Quantum mechanics was just being created; original papers were being published, some containing great ideas, others rubbish, but nobody could give Landau advice about which was which. He himself studied them and made his choice. He did this all his life, but it is surprising that he could do it even as a young man. He grew with every paper. Schrödinger's first paper appeared, then

another. . . . From all these publications and their dates one can see how and when Landau's scientific outlook on the world was formed, how he himself developed, taking in new ideas and working out his attitude to them.

However, he was not alone and he did not single-handedly form his scientific outlook. Other young theoreticians were close to him. But these young men, as has been mentioned, grew up without the assistance of their elders. They were on their own. What was it that united them? Youthfulness, youth's defiance of authority, belief in their own abilities? The desire for complete independence? Shared interest and a shared life? All of them were newcomers to Leningrad. Theirs was no easy life: lack of money, poor food, clothes that were not designed for the Leningrad winter. Extra sources of income had to be sought. But they did have the main thing: their talent and enthusiasm for physics.

They worked, they studied, they fell in love, they amused themselves too — not always inoffensively. Practical jokes and hoaxes, from which nobody was safe, satirical verses and poems by their fellow student and friend Genia Kannegiesser — all this was everyday life to them. Genia was one of those fascinating girls who become a centre of attraction by their lively minds, their quick wits, and the complete absence of triviality in their lives. How these youngsters, separated from their homes, yearning for their families, were drawn to Genia, her younger sister Nina, and *their* family, *their* home, filled with books and poetry! Most of all, they found there a really high level of cultivated society. A few years later, Genia was to marry the German physicist Rudolf Peierls, who often visited Leningrad. (It was a source of considerable entertainment to all that Peierls spoke good Russian in the early part of the day, but in the evening, apparently tiring, made more and more mistakes.) Peierls, whom Landau nicknamed "Peinka" (good little boy), became his friend and did some important pieces of work with him.

Not only Peierls but almost all of them had nicknames, usually derived from their own first names. George Gamow was "Johnny"; Dmitry Ivanenko, "Dimus". Matvey, or Mitya, Bron-

stein was for some reason that nobody remembers called "Abbat" (the abbot). They referred to their little group as "the jazz band". Older physicists also were awarded nicknames. Landau acquired at this time the name of Dau which he retained throughout his life. Anyone who knew him at all well, including his pupils, called him that; and he himself came to prefer it to any other.

This group of young people was very productive scientifically. For example, an important event in physics was the publication in 1927 by the 19-year-old Landau of a paper on "The damping problem in wave mechanics", which dealt with general questions in the newly arisen quantum, or wave, mechanics.

Copenhagen with Bohr, and other scientific centres

In 1929, Landau was sent abroad for 18 months. Such long postings of young physicists to work at the leading scientific centres in Europe and meet great scientists were frequent at that time. Moreover, Landau's exceptional powers were already evident – at the age of 20, he had published several important papers.

"I went to Switzerland, Germany, Denmark, England, and briefly to Belgium and Holland," Landau recalled thirty years later. "This was a very important journey for me. I saw all the great physicists. The only one I did not meet, and now shall never meet, was Enrico Fermi. It was a pleasure to talk with everyone I met. Not one of them showed a trace of conceit, pretentiousness, or arrogance."

The greater part of this time was spent by Landau at Copenhagen with Niels Bohr, whose institute was a world centre of theoretical physics, the "Mecca of physicists", visited by theoreticians from every continent. It was the scene of continuous highly intensive team-work; the atmosphere was one of intense mental life, such as always existed around Bohr simply because of his presence.

The scientific intercourse of theoreticians is usually somewhat

as follows. As well as solitary work, there is a continual exchange of scientific ideas. Seminars, or just discussions among two, three, five people as and where they wish, at the blackboard, over a meal, during a walk. In these discussions, debates, sometimes heated and acrimonious arguments, all kinds of things come up. Thoughts, ideas, proofs, reasonings, either fully worked out or only just conceived, are put forward. Many are found to be without value, as the other person does not fail to point out, with merry scorn if he is that sort of person. But there is still benefit. One of Landau's remarks was "Work can be useless without being pointless". Some valuable grains will be found in the heap – must be, if great talents and outstanding minds are brought together. Someone will see and extract those grains, and they will be jointly smoothed and polished – once again with arguments and discussions.

Landau, with his magnificent command of the methods of theoretical physics, his ability to see and understand instantly something new, his capacity and need to go his own way, his acutely critical intelligence, was one of the most active participants in this process at Copenhagen.

What is the "absolute worth" of a scientist, how can talent be measured? When can one say that a scientist is fully accomplished and has achieved all that Nature has offered him? These are subtle questions, with no single answers. For instance, there is Landau's well-known idea of classifying theoretical physicists according to their contribution to science. It is generally regarded as a light-hearted sally by a great man who was constantly trying to classify and systematize everything. On this scale, physicists were allotted to five classes. Einstein alone occupied the position ½, while class 1 was headed by Bohr, followed by Schrödinger, Heisenberg, Dirac, Fermi, and others. Landau placed himself only in class 2½, transferring himself to class 2 many years later. He assessed the work of theoreticians on a logarithmic scale. This meant that a person in any class had done ten times less for science than one in the class above. Landau placed in class 5, as if in a circle of Dante's Inferno, those whom he termed "pathologists".

Despite the humorous style of this classification, it is not random, and is essentially not just a joke. It finds an echo, for instance, in these words from the reminiscences of Academician Vitaly Ginzburg, one of Landau's co-authors: "Landau's talent is so great and his technique is so polished that one might think he could do even more, solve even more difficult problems. In this connection, I mentioned this once to Landau, and he, as if he had thought about it earlier, answered very distinctly: 'No, this is untrue, I did what I could.' " Behind this factual and restrained comment, many concealed emotions may be sensed. Landau certainly did think about this, probably more than once. The terseness of his reply is particularly notable.

There was another such occasion in his life. A young woman of 22, pretty, full of her own importance, to whom Landau was paying attentions, said, "Now if you were a genius . . . ", to which he remarked quite seriously, "No, I'm not a genius. Bohr is, and Einstein is. I'm not." Then, after a pause, "But I'm very talented. . . . Yes, I'm very talented."

It is hard to believe that these words meant nothing, were blurted out on the spur of the moment. Rather the contrary – they show that he had thought about himself and assessed himself, had decided his role and his place in physics, had reckoned up the account; not impartially or disinterestedly, with a high, perhaps extreme degree of objectivity and at the same time a considerable amount of emotion.

Landau once remarked, "I was born a bit too late. It would have been better six or seven years sooner. I could have done like" (and he mentioned some famous names of the nineteen-twenties).

Very likely he could indeed have done like them, to judge from all we know of the amazing power of his mind and talent, his almost unbelievable freedom of movement in every branch of theoretical physics, his masterly handling of its most complicated mathematical methods.

Dirac recently, on receiving the Robert Oppenheimer prize, said of himself, "I can thank the fact that I was born at just the right time. A few years older or younger, and I would have

missed that opportunity. . . . One might call the period from 1925 onward for a few years the Golden Age of Physics when our basic ideas were developing very rapidly and there was plenty of work for everyone to do."

It was for this Golden Age of the explosive and brilliant establishment of quantum mechanics that Landau was "born too late". For let us consider Bohr, de Broglie, Schrödinger, Born, Heisenberg, Fermi, Dirac, Pauli. The first four were indeed alive before the end of the nineteenth century; but the other four came in a group on time and as if to order: Pauli in 1900, Heisenberg and Fermi in 1901, Dirac in 1902.

At the start of the Golden Age, Dirac was a little over 20, and this was the time of his chief work. Landau was then only 17. By 1930, when he came to Bohr in Copenhagen, the main pioneering work in quantum mechanics was completed.

Rumer recalls being introduced to Landau by Paul Ehrenfest at a colloquium on theoretical physics in Berlin at the end of 1929. Landau remarked sorrowfully, "All the nice girls have been snapped up and married, and all the nice problems have been solved. I don't really like any of those that are left."

However, the future was to show that there were enough nice problems for him too. He himself, and the other physicists, realized this many a time. Although the main work on the foundations of quantum mechanics had indeed been done, they continued to be extensively developed in Copenhagen, and Landau took part with the other theoreticians. From that time dates his lifelong friendship and affection for Bohr. Later, Landau often said that Bohr was his only teacher. Each of their meetings – when Landau was briefly at Copenhagen in 1933 and 1934, and when Bohr visited Russia – was a time of celebration.

As well as in Copenhagen, Landau worked in Zürich with Wolfgang Pauli and in Cambridge with Ernest Rutherford. Whether as a result of Pauli's influence or simply by coincidence, physicists who knew both Pauli and Landau well (in particular Ehrenfest) afterwards noted a similarity in their train of thought and in some aspects of their work.

Landau's friend Rudolf Peierls was also in Zürich, and they wrote two papers together, including a very important one on relativistic quantum theory. This was, at the time, one of the "hot spots", relating to the area of quantum phenomena where processes occur at very high speeds, close to that of light. It was therefore necessary to use simultaneously two different and complex tools – quantum mechanics, and Einstein's relativity theory. The paper by Landau and Peierls met with a strong response from physicists and was much discussed.

It seems that in Zürich also Landau conceived and initiated another investigation, that of the quantum-mechanical description of the behaviour of free electrons, or, as it is usually called, the "electron gas" in metals. The resulting paper was published under the title "Diamagnetism of metals". He predicted (that is, discovered theoretically) the occurrence of quite unusual magnetic properties of the free electron gas in a metal. He was able to do this because, departing from the usual approach based on the laws of classical mechanics and classical statistics, he was the first to quantize the electron gas and thus arrive at the result that in this highly non-trivial "gas" the state known as diamagnetism occurs.

This phenomenon has become known in science as Landau diamagnetism. The work of the 22-year-old has become an essential component in the edifice of modern physics.

There is an interesting detail. The paper was actually written not in Switzerland but in England (the subscription reads "*Cambridge,* Cavendish Laboratory") and it ends with the words: "I should like to thank sincerely Mr P. Kapitza for discussions of the experimental results and for the communication of certain as yet unpublished data." Although Kapitza's youth was spent in the same city as Landau's and at the same institutions (Physicotechnical Institute and the University), their paths never crossed there. Pyotr Kapitza was born in 1894, so that he was fourteen years older than Landau, and by the time the latter came to Leningrad Kapitza was living in Cambridge and working with Rutherford. There he met Landau, and they became friends and colleagues.

Years passed, and the two found themselves together in a city where they had not lived before. The tracks of their journeys in science were to come closer, then further apart, and at one very important stage to run close and parallel. This was the result of a combination of many events and circumstances, both personal and otherwise.

Leningrad to Kharkov

The "second Leningrad period" was an intermediate stage in Landau's life. He returned as the disciple of Bohr and was himself inwardly ready to become a teacher, to set up his own scientific school, as he in fact later did in Kharkov. In Leningrad he was, it is true, active in teaching and in lecturing to students. But he still regarded himself as a "youngster" (though already a professor) and continually had to vindicate, often in quite embittered controversy, his point of view both on the methods of teaching theoretical physics and on the essence and content of it. (He liked to say that it was an independent science.)

At this time he was particularly close to Matvey Bronstein, who had very similar views and ideas. Bronstein was a remarkably able and educated person. He knew absolutely everything, many languages for example, even the most exotic, and added further languages with rapidity and ease. With equal ease he wrote verses, poems, parodies, and not only in Russian; his verses in English have been preserved. He had also memorized a large quantity of others' verse, and could outdo anyone else in this. He was a man of high principles, often sharply so. Recollections of him usually quote such lines as "He smiled and spoke the truth to kings" and "Small in stature but great in spirit". Bronstein was keenly interested in the popularization of physics, writing brilliantly on general problems of the universe, cosmology, and the physical nature of matter. Those who heard him lecture recall what a splendid lecturer this young professor was. His erudition knew no bounds. And he was able to turn and display a topic of discussion from so many sides, so many unexpected facets that

it took on a life of its own, and all its subtleties were permanently fixed in one's mind. Yury Sokolov and Elevter Andronikashvili have spoken with gratitude of the influence of Bronstein on his students.

Landau and Bronstein continued to write papers together even after the former had moved to Kharkov.

Kharkov was in those days the capital of the Ukraine. The Ukrainian Physicotechnical Institute was one of the offshoots of the Leningrad Institute. Ioffe, as we know, put a great deal of effort into the organization of new institutes in various cities of the Soviet Union. His pupils included some who became the directors of these Physicotechnical Institutes or their principal colleagues there.

Ivan Obreimov, who was at that time the head of the Kharkov Institute, perceived remarkably early for his time the independent significance of theoretical physics, saying that an institute cannot flourish without theoreticians. This was a new idea in Russia. Obreimov also believed that for an institute to work profitably, even if it is mainly concerned with experiments, the theoreticians' forces are much better concentrated in a theoretical division under a great physicist than dispersed among the various laboratories. In this way, Landau came to be the head of the theoretical division of the Kharkov Institute, or rather to transform it, and Kharkov itself, into a notable centre of physics in the Soviet Union. He had quick success in this. Soon, theoreticians from other cities began to visit Kharkov to meet, to solve problems together, to discuss their work. Physicists from both Russia and other countries took part in conferences there.

At the same time, Landau began to give a great deal of attention to teaching, acting as head of the theoretical physics department at the Institute of Mechanics and Machine-Building, and later of the general physics department at the University of Kharkov, giving lectures to the students.

Kharkov also stands at the source of much else in Landau's life, of which this period forms a great and important chapter. Here he first organized a theoretical seminar. Here he worked



Landau in 1936

out his first "theoretical minimum" programme in physics for members of the Institute staff. Here he devised and started a vast project, the writing of the *Course of Theoretical Physics.* In brief, everything that later came to be known as "the school of Landau" originated in Kharkov. It was there that Landau fully recognized his vocation as a teacher.

It is worth looking at a photograph taken at that time. Youthful in appearance, young in age; but a teacher. His relationship to his pupils depended on the fact that they too were young. This teaching activity did not at all interfere with his intensive original work or reduce its pace and variety. This is shown by the long list of papers which Landau wrote at the time.

Kharkov saw the beginning of the constant link between Landau and the experimentalists, and of his deep interest in the physical phenomena revealed by experiment. Perhaps it helped

that his closest and dearest friends were experimentalists – Lev Shubnikov and his wife Olga Trapeznikova. Landau had about him a personal magnetism which made him lifelong friends. "Any experimentalist could turn to Dau at any time," Trapeznikova recalls. "One could talk to him about anything; he understood and could advise better than anyone else. One could ask him about absolutely anything, any experimental results, what could be done and why. We were always consulting him. I never again met a theoretician like him."

The same story emerges from Ehrenfest's letter written in Kharkov: "I know very well why each group of experimentalists here is so fond of consulting Landau; he takes a lively interest in everything and is himself interesting. His boyish tricks often make everything he says seem altogether unintelligible at first, but if you keep on arguing with him, you feel the benefit."

It must be added that Ehrenfest always, wherever he was living and whatever the standing of the physicists with whom he came into contact, acted as a very significant catalyst in their mutual understanding. Everyone liked him a great deal, and his close friends Bohr and Einstein particularly stressed his role in this respect, as well as his exceptional critical faculty, and his clear understanding of the details and difficulties of new ideas in physics.

During his stays in Kharkov, Ehrenfest quickly persuaded the physicists there not to be afraid of asking about anything they did not understand. On the contrary, he insisted, one cannot remain uncomprehending, hearing apathetically things that do not convey any meaning. The Institute seminars were consequently extremely lively and interesting, with everyone becoming involved. Landau was especially active. As well as seminars for the whole Institute, there were divisional ones, and in particular the theoretical seminar organized by Landau.

When Landau arrived at the Kharkov Institute, there were intensive studies in progress on the physics of low temperatures. Shubnikov and Trapeznikova had returned from the Netherlands, where they had worked for a considerable time at Leiden, the world "low-temperature centre", and in addition to their interest

in this branch of physics and a fund of experience and ideas, they brought with them also as presents some unique equipment and materials.

Shubnikov was an excellent organizer. He set up and fitted out a fine cryogenic laboratory, at that time the only one in the Soviet Union, containing apparatus for the liquefaction of gases, including helium. Skilled glass-blowers and mechanics were employed. Staff, including laboratory assistants, maintained 24-hour operation. Experiments too were often in progress day and night. Olga Trapeznikova arranged nocturnal tea-parties to keep spirits up.

The subject of low temperatures was an extensive one, but particular attention was given to superconductivity, superconducting alloys, and magnetic properties near absolute zero. It was during his time at Kharkov that Landau acquired a deep interest in the behaviour of matter at very low temperatures. Of the seventeen papers on various topics (for he was a universalist!) that he published in this period, four were wholly or partly on low-temperature physics.

The library was usually busy, even at night. Everyone had a key to it and could take books out, though not for more than a day. Books were hardly ever missing. If one was, the library was closed, and the absent book had to be surreptitiously replaced. The library rules included one which read "No talking, even in whispers". In the "theoretical" rooms, on the other hand, where Landau held sway, there was constant talking, the noise of which was audible to everyone nearby.

In their free time they played tennis, wrote songs, put on plays, held fancy-dress parties, and generally amused themselves. As in Leningrad, the youngsters gave one another nicknames. Olga Trapeznikova, so kind and sympathetic, was called Olenka-Angel. Shubnikov was "Fat Lev", Landau "Thin Lev" (he used to say that he had not a constitution but a destitution). Nevertheless he possessed his own peculiar gracefulness and even dexterity. He played tennis quite well, though with comical effect, holding the racket in anything but a conventional manner.

Kharkov also brought changes in Landau's private life. He became acquainted with Cora Drobantseva, whose beauty bowled him over at first sight, and fell in love with her. A few years later Cora, an engineer in a chocolate-factory, moved to Moscow and became his wife. In 1946 they had a son, Igor.

Landau's talent and vocation as an educator, and not only in physics, was becoming more and more obvious. He strove to make his pupils good and upright people, and fought against what in his opinion did not conduce to true merit and right behaviour. He urged on them the view that one should do work because it is interesting, because one wants to resolve some problem, to understand some new and complex phenomenon, to clarify some question – the desire to make a great discovery should not be what motivates research. And to aim at achieving a revolution in science is simply ridiculous.

He despised those who sought at all costs to revolutionize science and gain renown from it, together with all careerists and opportunists in science. He disliked even the word "learned". "A poodle can have learned something," he would say. "A man can have learned his lesson. *We* are scientific workers." He also could not endure the high-flown "devotee of science". "There are some people that you can see by looking at them are 'devotees of science'. They use science to fill their bellies. They have nothing else to do with science."

Landau was a remarkably pure man, says Trapeznikova. For that reason, there was much in his behaviour that cannot be measured by ordinary standards. He fought against "diehards", he detested "vermin". Nevertheless, she recalls, when she asked him what quality he most valued in people, Landau unhesitatingly answered "kindness".

The conflicts in which Landau and some of his friends and pupils became involved led to considerable unpleasantness that was a serious matter. In the end, it was necessary to think of moving to another city.

Moscow: the Institute of Physical Problems

At the beginning of 1937, Kapitza invited Landau to be head of the theoretical division of the Institute of Physical Problems. This invitation was received with the greatest pleasure. Landau moved to Moscow two months before the Institute was officially opened.

Kapitza's Institute arose on vacant ground, both literally and figuratively. He had worked for many years with Rutherford at Cambridge, but always spent his holidays with his family in Russia. Arriving on holiday in the summer of 1934, he learned that he must now work in the Soviet Union. He was promised his own Institute, with extensive freedom to organize the scientific work as he thought fit. It was decided that Moscow would be the best location.

Kapitza and the first of his future colleagues, the Leningrad physicist Alexander Shalnikov, visited government offices and ascertained the manufacturing possibilities for getting the necessary equipment and apparatus. Then they spent many hours tramping the streets and lanes of Moscow, looking for a site for the Institute. Kapitza's first choice was a house in the Neskuchny Gardens. But he did not get it. Yet he was still attracted by the banks of the River Moscow. And there, at the end of the Gardens, they found a vacant and ownerless piece of land. Upon it was a huge rubbish-dump, with cats on the prowl. But the river was there at the foot of the embankment, and in those days it seemed almost like being in the country.

This Institute became a special one, different from all others, even university science establishments. Its external appearance was different too. There was much greenery around it, and a tennis court. Then the staff quarters: self-contained cottages, as in England. Excellently equipped mechanical and glass-blowing workshops. Everything to hand and for the Institute's use only. Living quarters, laboratories, offices, library, all within a few seconds' walk.

Rutherford had earlier obtained permission to equip a Cambridge laboratory, unequalled in the world in those days,

for his favourite pupil Kapitza. The latter, a brilliant experimentalist who combined boldness with keen inventiveness, now had the means to produce very powerful magnetic fields for the study of the behaviour of matter under such extreme conditions. He was also interested in the properties of matter at low temperatures, and gas-liquefying apparatus was therefore installed in his laboratory. Now, seeing that his closest pupil would no longer be able to live and work in Cambridge, Rutherford obtained the agreement of the British Government to the sale of this equipment to the Soviet Union.

The basic principles of work and life at the Institute were very clearly stated by Kapitza in his written and spoken announcements. The chief one was that there was to be no dissipation of effort and resources, no triviality. Only "big science" would be studied, the basic phenomena of Nature, leading to a deeper understanding of them and to a knowledge of the essence of things.

In our days, when science has become frankly industrial, and scientists have become a large profession, it is legitimate to inquire as to the role of an individual scientist's personality in the further development of science. Is the big man still as important? Should he not be replaced by a well-organized team of trained, modern-minded, capable, energetic scientific workers?

"In big science," Kapitza replies, "good progress can be made only by one who is creatively gifted and creatively aligned towards his work. . . . Although the path of science is predetermined, movement along it results only from the efforts of a very small number of exceptionally gifted people. . . . The core of an institute must therefore necessarily consist of a small team of very carefully selected scientists, who must devote themselves entirely to science."

In February 1937, Kapitza wrote in a letter to the Chairman of the U.S.S.R. Council of Ministers (as it now is): "This month, Comrade L. D. Landau begins work with me. He has a doctorate in physics and is one of the most gifted theoretical physicists in the Soviet Union. He is to work on all theoretical topics

which relate to the experimental research at this Institute. Experience has shown that the co-operation of experimentalists and theoreticians is the best way of preventing theory from becoming divorced from experiment, while at the same time the experimental results are given suitable theoretical analysis, and all members of the scientific staff acquire a wide range of scientific interests."

In his public remarks, Kapitza described other principles in the organization of his Institute's work; for example, he explained that one cannot treat scientific research in the same way as rigorously planned industrial production: "Newton himself, for instance, could not have discovered the law of gravitation according to a prescribed plan, because this happened spontaneously when he was inspired by seeing the fall of the famous apple. Evidently, nobody can make plans for the moment when a scientist sees a falling apple and reacts in a particular way. The most valuable things in science, and the foundations of big science, cannot be planned, since they arise from the creative process, the successful outcome of which depends on the ability of the scientist himself."

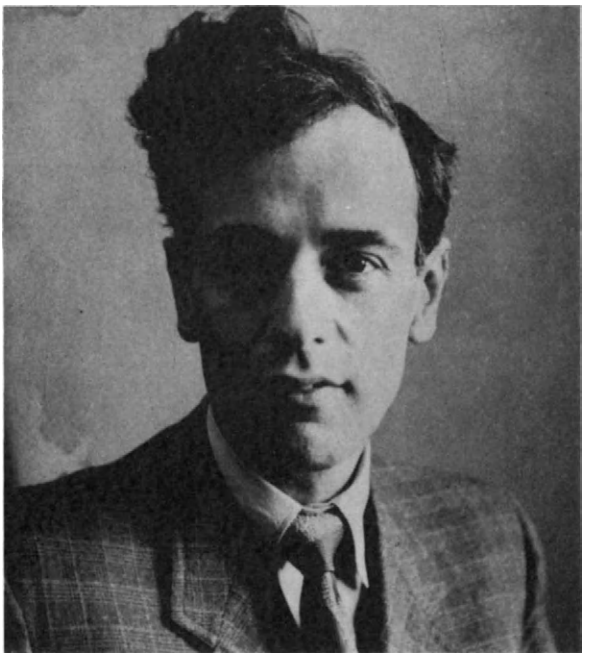
There were polemical remarks too, aimed at the departments of finance: "Would it be possible that in looking at Rembrandt's pictures you would like to know how much Rembrandt paid for his brushes and canvas? Then why in considering a scientific piece of work do you want to know how much was spent for equipment and materials?" And half-ironically: "I asked: how much would the Ministry of Finance find admissible for Newton to spend for his work which led to the discovery of universal gravity?"

Gradually, the Institute took its final form. Members of staff were appointed. Expert glass-blowers, mechanics, and electricians filled the workshops. The seminar, the famous "kapichnik", was started, and soon gained popularity.

At the early seminars, Kapitza told of a remarkable phenomenon discovered by the Dutch physicists Keesom, father and daughter. It seemed that liquid helium had an impossibly large

thermal conductivity. Its capacity to conduct heat was far above that of the most conductive elements, the metals copper and silver. Simply orders of magnitude greater. The thermal conductivity of liquid helium was hundreds of times that of copper. Kapitza, baffled by the fantastic results of the Keesoms' experiments, which contradicted all known laws of physics, and remembering that in Nature every "impossibility" is eventually explained and understood, decided that he would himself repeat these experiments. What happened thereafter and what was the final outcome, the reader will learn presently: the second half of this book is devoted to that story.

At the Institute of Physical Problems, Landau found his true



Landau in 1940

home. His life and destiny were tied to it until the end, in war and in peacetime.

In the summer of 1941, the Institute was evacuated to Kazan. There Landau, like his colleagues, devoted his efforts mainly to wartime tasks. He devised theories and made calculations of the processes governing the efficiency of armaments. In 1945, after the end of the war, the *Comptes Rendus (Doklady)* of the Academy of Sciences published three papers by Landau on the detonation of explosives. Each bore two addresses: the Institute of Physical Problems, and the Engineering Committee of the Red Army.

The war ended, but tension remained on the earth. The United States had continued to develop the atomic bomb, and had devastated Hiroshima and Nagasaki.

Soviet physicists believed that in the resulting situation they could not cease work on nuclear weapons and so imperil the safety of their country. Most of their leading physicists took part in the series of researches headed by Igor Kurchatov. Some of them spent a period entirely on the design of nuclear weapons, left their institutes, and were in their home towns only for brief visits. Others combined their ordinary life and work with the fulfilment of special duties. Landau was one of these. He made a large contribution to the theoretical calculations of the processes being studied, at various stages of research and development.

Landau received many national awards for his work. In January 1954 he was made a Hero of Socialist Labour; he received the State Prize three times and the Lenin Prize once; he held many orders, including the Order of Lenin twice.

At the Institute of Physical Problems, Landau gained wide international recognition as an outstanding representative of Soviet science. He was elected an honorary fellow of the British Institute of Physics and Physical Society, a Foreign Member of the Royal Society of Great Britain, a member of the United States National Academy of Sciences and the American Academy of Arts and Sciences, and a member of the Danish and

Netherlands Royal Academies of Sciences. He received the Max Planck Medal, the Fritz London Prize, and finally the Nobel Prize for physics.

Landau's intensive and fruitful work continued unabated until the day of disaster.

On 7 January 1962 there was an accident on the highway to Dubna. . . . A fight to save Landau's life, after many months, succeeded. But the grave injuries he had suffered were too numerous. He was for long racked almost constantly by severe pain. He was not able to resume scientific work. He was somehow a different person ("This was not in my time," he once said), and lived a different life. Let us not touch upon it here.

Lev Landau died on 1 April 1968. In January 1978 he would have been seventy years old.

Kapitza several times suggested to Landau that the Academy of Sciences might set up a special large Institute of Theoretical Physics; that he ought to be the head of an institute, not of a small theoretical department at the Institute of Physical Problems. But Landau "not only declined, but even refused to discuss the matter. He said that size was not important and that he was extremely happy to be classed as a staff member of the experimental institute."

Looking back now, we can all see that Landau chose the best option. It is difficult even to count how many hares he had started at once. He remained on the staff of the Institute of Physical Problems, which gave him at least three chances. His relationship with the experimentalists, animated and constant, was not interrupted, but continued to be close and integral. He always valued this relationship highly; it was essential to him. He himself remained a member of a team that he found extremely attractive. Lastly, here in his home, he was one of the leading participants in the "kapichnik" seminar, where reports were given on every branch of physics and related sciences. In this way he had orientation in all the exact sciences, and had only to decide where to concentrate his attention. All these benefits came to him simply as a staff member at the Institute

of Physical Problems, not as head of the theoretical division of the Institute.

But there was more. Landau, refusing to leave Kapitza's institute for some possible personal advancement, nevertheless created in that institute something new and unique and probably more significant than the proposed Institute of Theoretical Physics.

Despite Landau's love of rigour and order in all that pertained to physics, this "something" had as a whole no definite organizational form. Though it may sound paradoxical, each constituent of the whole was yet subject to a precise internal – and in some cases external – order and routine.

Having no organizational form, this invisible "Landau Institute" was never planned or established. Nobody financed it, allocated funds, drew up a list of staff. This is not surprising. It was the complex, remarkable, and very significant entity known as the school of Landau.

The School of Landau

Landau the teacher

A TEACHER. Indeed, as his pupils emphasize, a Teacher. The most usual associations of the word are respectability, venerableness. Landau came to the Institute of Physical Problems before he was 30. Before that, as we know, Kharkov. Everything that developed so broadly in Moscow began in Kharkov. So, when we speak of the school of Landau, Kharkov has the right of primogeniture. A young man (at 24, many are only just graduating), with all his boyish – still boyish, at that time – whimsicality. Landau then and there became a Teacher. Devising, undertaking, and beginning to carry out most serious projects.

A young teacher, the same age as his pupils, sometimes younger, often on familiar terms with them, as is evidenced by Isaac Pomeranchuk's famous "O Master, what a load of tripe." Yet there was always respect and a sense of proper distance.

We are now beginning to be accustomed to this combination of what seemed incompatibles. It had previously seemed that "academician" was not only a title of honour, but also a token of age. Academician, leader of a school. Then here was young Landau shuffling the cards and refusing to conform to the rules.

This was not the only way in which Landau and his school were nonconformist. His pupils still recall with astonishment its style, characteristics, and habits. Indeed, if we compare the school of Landau with other scientific schools, or approach it with conventional ideas – conventional because they represent the actual and usual state of affairs – there will be many negations and contrasts.

In that school there was maintained a constant micro-climate. Man does not live in a vacuum, and it will not do to depict him out of time and without an environment. But whereas time and

environment on the "global" scale are fairly well known and understood, so that only the date and the place of action need be mentioned, a micro-environment needs description to give it shape and presence. Like anyone else, Landau acted and existed in a group, in a particular atmosphere. As regards his school, it was the atmosphere created by Landau himself, the group that he had fostered and educated.

The story of the school should perhaps begin with its location. The term "school" suggests a particular building, and indeed in the early years the pupils worked alongside their teacher, in the same institute, department, laboratory, or division. But, as Mayakovsky says, "there is really no location, they simply pretend".

Now, there is an Institute of Theoretical Physics, named after Landau and headed by his pupil Isaac Khalatnikov. The theoretical division of the Institute of Physical Problems is now under Academician Ilya Lifshitz, whose independence at one time caused Landau to dub him "the archduke"; but Lifshitz says that he belongs to the school of Landau and that they were linked for many years as friends and colleagues. The theoretical division now occupies a cottage in the staff quarters of the Institute. It has sacred relics too. In Lifshitz's office, at the top left-hand corner of the blackboard, which is generally covered with formulae, a piece of transparent plastic preserves in perpetuity the autograph "P. A. M. Dirac", written in chalk, and the date, 17 July 1973, of the visit of the last surviving member of the company of great physicists of the twentieth century.

In Landau's time, the theoretical division occupied four small rooms in the main building of the Institute. That was the location of his school. Or it was upstairs in Landau's home. Or it was the few yards of tarmac from there to the Institute door. Or it was the wide corridors of the Institute itself.

As Landau and his pupils were all theoretical physicists, it is not surprising that various aspects of the activity of his school involve the word "theoretical": the theoretical seminar, the theoretical minimum, the *Course of Theoretical Physics*. In the



Landau in 1948

process of organizing the school, all these became very closely linked and interdependent: Landau's pupils were usually the main actors and speakers at the theoretical seminar: a pupil was one who had completed the theoretical minimum, and to do this one had to know and understand the volumes of the *Course*. That was also necessary in order to be a useful participant at the seminar, because the *Course* was a kind of glossary, the basis of a common language. A person who understood it and could express himself freely in it felt at ease. "Strangers" often found things difficult.

The school, like any living organism, developed and changed in the course of time, evolved. In Kharkov the school was founded. Landau was searching, groping for the best ways. In Moscow it came into full bloom. Later, its multiplicity and diversity sometimes put an end to hallowed traditions.

For instance, so many people wanted to achieve the theoretical minimum that eventually it was a physical impossibility for Landau himself to examine all of them, and he then shared the task with his closest pupils. Again, so long as the audiences at the theoretical seminar were small, it remained largely homogeneous and all were active participants. Later, when it became a meeting-ground – and a place of pilgrimage – for theoreticians from Moscow and beyond, the difference in levels of knowledge, preparation, and even just scientific style and approach made it difficult for many to understand what was going on, and this produced an inert and silent mass of listeners at the back of the room.

There is perhaps more to be told about the seminar than, say, about the theoretical minimum; more incidents can be recalled. Of course there is only one source of information about the seminar and indeed about the school, namely the regular participants in the weekly meetings of the seminar, Landau's pupils. Our story is therefore largely an account of their recollections, often in their own words and views.

The theoretical seminar

Every Thursday at 11 a.m. sharp. No, we must begin a little earlier. Not yet eleven. But already a throng of theoreticians in the corridor, walking up and down, forming groups, exchanging lively quips. Here and there an outbreak of argument, enthusiastic and excited talk. About physics, of course; perhaps the topic of the forthcoming seminar, perhaps a paper that has attracted everyone's attention, perhaps the unexpected research results of someone present. Among the crowd is glimpsed the tall and slightly stooping figure of Landau, "striding along the wide corridor with one of his pupil friends, discussing a newly published paper or the latest 'intractable' problem", recalls one of these "pupil friends". But more often he is strolling with one of the speakers at today's seminar, making some arrangement, or eliminating some obscurity. He is like an artist putting the finishing touches to a picture.

Around him, the buzz of excited talk continues. There is an atmosphere of expectation, rather like that before an opening-day or a first night. Here it happened before every seminar. Most of those present regarded it as a festive occasion. But not one of idyllic joy or general pleasure. Nothing idyllic about it. And not a pleasure for everyone. Intense mental effort was needed. Not all the participants were equal to it; there were often rough passages, and concealed tears. . . . And yet, a festive occasion.

Landau took the seminar very seriously, prepared for it, and decided who should give reports; the seminars were not impromptu affairs. If the report was to be not on the speaker's own work but a review of a journal, Landau himself selected the papers that were to be discussed ("Dau marked up the *Phys. Rev.*", everyone says.) The reports were given by his pupils, who usually took turns in describing the various papers and chose who would discuss what from among the marked items. There was no selection as regards subject, only as regards level. There might be reports on the same occasion on thermodynamics and quantum mechanics, one speaker on solid-state physics, another on a topic in nuclear physics or elementary particles. Landau alone made the choice. From the flood of information, he selected what was the most important for the majority of his pupils. The theoretical seminar had a subject range that made it unique of its kind. This was an enormous undertaking for Landau to begin in Kharkov, before he was 30.

"The presentation of papers at this seminar became a sacred duty for all pupils and staff, and Landau himself was extremely serious and thorough in selecting the material to be presented," writes Evgeny Lifshitz. "He was interested and equally competent in every aspect of physics, and the participants in the seminar often did not find it easy to follow his train of thought in instantaneously switching from the discussion of, say, the properties of 'strange' particles to the discussion of the energy spectrum of electrons in silicon. To Landau himself, listening to the papers was never an empty formality: he did not rest until the essence of a study was completely elucidated and all traces of 'philology' – unproved statements or propositions made

on the principle of 'why might it not' – therein were eliminated."

The seminar was always a serious piece of work for Landau, his pupils stress. He understood everything from start to finish. Yakov Smorodinsky recalls that whenever something unusual came up at the seminar Landau always had the best knowledge of the problem. He quickly performed in his mind the calculations or reasoning that were not written on the board. He never took anything on trust. During the seminar he was all attention.

And did it happen that Landau failed to grasp something, were there hitches, or did the seminar always run like a perfectly adjusted machine? It *did* happen; these were after all human beings, and sometimes there was a breakdown. We shall see later how Landau acted in such cases.

Usually, however, the preparation ensured the proper quality of presentation. As a rule, there was a previous discussion in private with Landau, especially in reports on one's own original work rather than on a journal article. Landau tried to ensure that the speaker was well prepared and competent. But, of all the seminar participants, it was really he alone who was invariably well prepared.

Landau himself was the only person who did not present reports at the seminar. He figured as purely a teacher, listening and correcting – and directing – his pupils. This was his school, where he taught physical thinking, perception, and approach. "It was quite strict," Yury Kagan recalls. "Landau made the speaker follow the direct and proper line. The speaker had to comply. The audience might not understand the entire argument; the seminar was not at all easy for the listeners. But later they realized how correctly Landau had stated the question. New turns, new directions of the problem, both forwards and sideways, both for the speaker and for those concerned with neighbouring topics."

Yes, it was exactly like that. Purely a teacher. But it would not be right to conclude that in his relations with his pupils he was only a "donor", a sort of "milch cow" (although there was indeed a current expression "to milk Landau"; few of the

theoreticians lost an opportunity to discuss their work with him – if they felt that Dau would not regard it as hopeless – or to pick up some ideas from him, or hear his opinion of interesting work from elsewhere).

Landau himself, however, was not the one who got least benefit. In his relations with his pupils, everything was so closely interwoven that it was often impossible to tell cause from effect. Although a great deal of effort and time was devoted to organizing the seminar, to preparation, and to discussions with those who were to speak, Landau too derived great benefit from it, and from his pupils generally. And this return was not just spiritual, almost ephemeral, like Rutherford's "my pupils keep me young" (Landau kept young without effort), but certainly quite material.

"By ear": to hear and learn what was new in physics, and to hear and understand the whole of theoretical physics, or any part of it. It is not surprising that this "Landau phenomenon" has long since become a legend. It is indeed difficult to imagine that anyone could in this way assimilate the huge amount of very complex and diverse phenomena, theories, researches, papers, ideas, conclusions, formulae – all the complicated and manifold mechanism that is modern theoretical physics: assimilate, sift, reject the worthless, and permanently retain the valuable part. He did not simply memorize it. Just as a first-rate surgeon has his instruments arranged so that what is needed comes immediately to hand, so Landau had this vast arsenal of knowledge always ready for action. Whatever was needed at any moment came immediately to hand. For Landau's universalism did not consist merely in knowing a great deal about many things in the various branches of theoretical physics. He had a grasp of theoretical physics as a whole. He saw it, imagined it, perceived it as one organically interrelated system. Anything that he learned was fitted into a particular cell, as in a honeycomb. One might say, into a particular cell in his memory, if this did not sound now like cybernetics terminology.

The seminars and the other conversations with his pupils, about their work and about journal articles, were the source of

Landau's information about everything that was happening in physics. "It is hard to imagine how he could have been so successful in so many fields of physics without his pupils," wrote Kapitza. "This work was achieved in the course of non-stop discussions and regular seminars at which Landau was the most active participant. . . . Landau gave to his pupils generously of his time and granted them great freedom in their choice of subjects. Their work was published under their own names. For his part, Landau gained much from his pupils. One of the peculiarities of his work in science was that he himself read no scientific literature; this was read by his pupils, who told him about it".

So, like a boomerang, what he had given returned to him. Both sides gained and were enriched.

"In the thirty years I knew Landau, I only once saw him reading a book. Everything he knew, he learned 'by ear' from his pupils, especially at the seminar," recalls Alexander Kompaneets. (In the list of physicists who had achieved the theoretical minimum, written out by Landau himself before the accident, Kompaneets was No. 1. He was the first to pass, in 1933.)

Kompaneets is referring, of course, to specialized scientific books and journals. Landau read, and read extensively, books on history and the arts. He was very fond of history and had an excellent knowledge of it. He usually read with interest the entire text. But in physics books there was much that he found unnecessary and even annoying – discussion, the author's arguments and calculations, the method of proof. Only the idea and the result was generally of interest. The intermediate part he traversed himself and in his own way.

This was not something innate in Landau. In his early years at school and university, and for some years thereafter, he read physics publications avidly and constantly. Nobody would have been allowed to *tell* him about them then. But in Kharkov, along with his various activities in setting up his school, with the recognition of himself as a teacher, came this method of dealing with the physics literature, which thereafter was gradually modified.

At first, when a new journal came in, Landau himself looked through all the papers and made notes of those which seemed interesting, in one book for theoretical papers and another book for experimental papers. Participants in the Institute theoretical seminars which he organized (there were two in Kharkov, one for theoreticians and the other for experimentalists) chose and reported on whichever papers they fancied.

Landau crossed out in the books both the papers already reported on and those which proved to be of no interest. At this time the term "pathology" originated. The verdict was final, without right of appeal; Landau simply lost interest in the paper, and often in its author also, sometimes permanently. The remainder of the "theoretical book" was a list of papers that he assigned to the "gold reserves" – work whose ideas or results were of more than passing value. These Landau kept in his mind, likewise permanently.

He maintained these records until 1941. But publications became so numerous that it was impossible to note everything. Only the "gold reserves" were still added to. Then came the war. . . .

So, 11 a.m. on Thursday. Everyone is seated. The seminar is about to begin. (Or someone looks at the clock and says, "Let's begin, it's one minute to." "We'll wait another minute," Landau replies. At this point, a panting Arkady Migdal would often dash in. In any event, after the passage of "Migdal's minute", at 11.00, the seminar began.)

What would a newcomer have noticed, present at the seminar for the first time? Primarily, the breadth of subject range, the seminar was normal," one pupil recalls. "Everyone knew that of theoretical physics as it was at that very minute, so to speak. "For physicists of my generation, the unusual breadth of the seminar was normal", one pupil recalls. "Everyone knew that you could speak about work in any branch of theoretical physics. And not only speak, but get expert advice; either during your paper, or in previous discussion with Dau."

Indeed, Landau by his personal example and his teaching practice taught our theoreticians to treat all of theoretical

physics as a whole. That was the way to perceive, know, and understand it, and to work in it. His pupils became accustomed to live with the whole of physics. And only when they had moved away from this custom, especially after the loss of Landau, did they belatedly realize and fully value what he had given them, and see with astonishment what had earlier seemed in no way surprising.

One of the Western physicists has recalled a curious fact. In 1930 three young men, Weisskopf, Peierls, and Landau, in Copenhagen with Bohr, made it their aim – for the future – to concern themselves with the whole of theoretical physics. Twenty years and more went by. Scientists from Europe and America began to come to the Soviet Union. The "trialogue" was recalled. They found that only Landau had carried out their sometime resolve. It was embodied in his publications, and with particular obviousness and impressiveness in the seminar. As the visitors said, they were flabbergasted; they had never come across a seminar that dealt with the whole of theoretical physics at once. The Americans, returning home, published in one of the journals an account of their visit. They said that the United States contained no physicist comparable to Landau.

The week's seminar begins. After a few minutes, Landau understands everything better than the speaker, even if the latter is discussing his own work, and not someone else's paper. This ability to grasp everything at a stroke, to detect all subtleties, to draw his own conclusions, was generally agreed to be unique.

The same happened at the seminars for the whole Institute, the "kapichniks", led by Kapitza. Reports on anything, any field of experimental or theoretical physics or adjacent topics bordering on biology, chemistry, technology – the same "procedure" always occurred: Landau was the first to jump up with a question. The question and his subsequent remarks left no doubt that he had not only penetrated already into the core of the problem, but seen its deep structure, its complexities, the conclusions to be drawn, and the relationship to other problems. This happened constantly, and the author of this book has seen it many times. Moisey Kaganov has given a terse but

clear explanation: "Landau had an impressive combination of quick reactions with informed knowledge and profound understanding. I have never seen anyone like him."

One might expect to become accustomed to this after witnessing it at every seminar. But there was no getting accustomed to it. Even when one was expecting this lightning reaction and its accuracy, it never ceased to be amazing when it happened before one's eyes.

And when various physicists, not only Landau's pupils but theoreticians from elsewhere, and experimentalists, described their work to him, in five or ten minutes he understood the problem fully, and was able to evaluate it and give the right advice.

"Landau knew everything, because he was interested in everything," was Kompaneets' summing-up. "There will not soon be another theoretical physicist with such extensive erudition. Living creative knowledge combined with absolute clarity of understanding of all sides of a subject" — that was the foundation of his remarkable gift.

At his seminar, Landau of course did not sit at any chairman's desk, in fact there was none. He sat in the audience, but always in the front row, so as to be able to run quickly to the blackboard. At Kapitza's Wednesday seminars he also sat at the front, for the same reason.

So the speaker at the theoretical seminar was usually talking mainly to Landau, although the whole audience talked to the speaker, interrupting him, asking questions, pointing out mistakes, and making sarcastic comments about various statements and formulae.

The seminar was always a working one. This was indeed its distinctive feature. It was the scene of continuous intensive thinking and learning. Even outwardly, there was always a clearly defined procedure: the sessions began and ended at precise times, and there were fixed vacations. Most important of all, Landau would never rest until the whole of the topic under discussion had been elucidated; he wanted everyone to understand it completely.

But this efficiency, to the stranger, looked like the incarnation

of inefficiency. Noise, shouts, questions, continually interrupting and annoying the speaker, exchange of tart repartee, no hesitation in calling out "nonsense, idiotic, crazy, pathology", and even more scathing comments. But the noise and apparent bedlam were of a special kind, directed only at the topic or the essence of the work or paper, or the whole problem. It was not allowed – no one remembers such a thing – to digress, to veer away from the question under discussion. Or to tell the latest story, as is so customary in gatherings, scientific and other. That was simply not possible in the seminar room.

"I knew, of course, about the seminar and its scientific quality before I attended it for the first time. Yet it still amazed me, in particular by its atmosphere," Kaganov recalls. "Those who assembled in the room, one had a distinct sensation, were living for theoretical physics. Having written this last sentence, I realize that I am not expressing myself accurately enough. . . . People of various ages and status and qualifications and appearance took part in the seminar, but all had one thing in common: what was happening there interested them more than anything else in life. The fervour with which they spoke, the chagrin which they suffered when dismissed from the blackboard (as quite often happened; giving a paper was a hard task), were not diluted by any subsidiary considerations. Science, and nothing else, ruled at the seminar. I can remember no instance where personal relations between the participants had any effect, or where the arguments that often broke out, and were rarely stopped, arose from liking or disliking the speaker, rather than from the problem in hand or the way of solving it. It was a completely democratic seminar."

One would like to write in elevated style about the spirit that prevailed in the seminar, but the "Cossack anarchy" which took over the meetings not only discourages elevated style but is simply incompatible with it. Nevertheless, all that took place, the whole life of the seminar, had as its implicit content the atmosphere created by Landau and was always consistent with this.

Yakov Smorodinsky emphasizes that an unusual and yet

leading feature of the seminar was the great interest in and respect for what others were doing. The sarcasm and acerbity might relate to the speaker's line of thought or to his errors, but never to the speaker himself.

Such was the result of the whole spirit of Landau's school and his attitude to physics. For instance, he simply had to listen to and take part in any serious and significant discussion of science; nor could he agree to anything he did not understand or thought incorrect.

It often happened, Kagan recalls, that there was a conversation partly about physics and partly not. Landau genially joined in. But as soon as some important point in physics came up, he immediately became plunged in silent thinking about the question.

This remarkable ability to drop everything instantly and ponder about a physics problem that had just been raised was noticed by many. At such times Landau thought of nothing but the problem concerned, and as deeply as if solving a problem of the universe, even though only a quite particular problem could be solved.

As well as the established order of seminar proceedings already described, there were other, unwritten, regulations. Whereas the established order gave the framework (Thursday, 11 a.m., break after an hour, and so on), the other part dealt with the inner life of the seminar and was more by way of "anti-order", since a great deal in the arrangements, the behaviour, the life and style of Landau and his school seemed to a stranger's superficial glance quite the opposite of what it really was.

"Nobody is allowed to give his paper" was the way in which those coming to the seminar for the first time (the others were used to it) expressed the force of this "internal regulation". And indeed, during a presentation, at any point and at any time, anyone was entitled to interrupt the speaker with a question, a comment, a correction, an expression of disagreement. And everyone, from Landau downwards, made extensive use of this privilege. The remarks from the audience were not accompanied

by any apologies and were far from showing extreme delicacy. Landau was quite unperturbed by this noise.

For the regular attenders, the procedure was quite standard. If an outsider, a visitor, gave a paper, he sometimes asked not to be interrupted. They managed to achieve this.

There was an unwritten code of behaviour at the seminar. The regular participants, especially Landau's pupils, knew exactly what could and could not be done. Unfortunately, purely theoretical knowledge was by no means always sufficient. First of all – if it does not sound too obvious – one had to understand accurately the work and what one was saying about it. The speaker had to think and grasp both the essence and the details, to have a clear idea of them, to be able to present them appropriately, to get them across to the audience and not bother too much about dealing with comments and questions.

Perhaps even more important was the quality and validity of the work being reported. If, for instance, it was found that the speaker's own work was not worth discussing, if the author could not show that it was new and valuable, or simply became confused and unable to explain it intelligibly, he was unmercifully hounded from the blackboard, as already mentioned.

Not only *what* was reported, but *how* it was reported, had great importance. Again we call on Kaganov: "The scientific intimacy and the strong interaction gave rise to a peculiar language of scientific communication, which was well understood by all theoretical physicists close to Landau (it is worth emphasizing the very high professional standing of the Landau school), and to which one had to become at least accustomed. It was necessary to know how to describe one's work. Some accomplished this easily; others, even if their work was entirely sound, were unable to grasp the subtleties of the language of Landau."

Besides knowing the "language", however, one had to be able to speak concisely and clearly. There was no place for emotion, or rather it might relate to the essence of the matter but never to one's own feelings. It was absolutely forbidden to expose these in public.

Even so, there was at the seminar a sort of basic benevolence, the tone of which was set by Landau. He would say, in one of the many forms of a frequently used expression, "The author is usually right," and this was the motto of the seminar. Though his own bite was quite sharp and painful, Landau always began by defending the speaker and usually took his side. "What's the answer, finally?" someone demands impatiently, weary of long and detailed calculations. "Hold on a moment," Landau restrains him. "Don't interfere. We need the exact solution here." But sometimes a speaker eventually became confused, or made a glaring error, or proved incompetent. Then, like a sentence of doom: "Alyosha, what's next?" Such apparently harmless words. Alexey Abrikosov, secretary of the seminar, got up. "The next paper is . . . "

All this strict procedure simply showed Landau's utterly serious and responsible attitude to physics – no more than that.

Ehrenfest saw a great similarity in the thought processes of Pauli and Landau, and also in their approach to problems of physics, in their creative style.

The general view is that they were similar in another way: their sharp critical faculty and acute assessments, both of their fellow physicists (without respect of persons) and of the latter's work.

Pauli, in such cases, was uninhibited in his expression and harsh in his choice of words. He was caustic, ruthless, and to the victim it seemed that he was simply rude. But this often very offensively expressed criticism did have important and useful content. Pauli uncompromisingly opposed errors, and only secondarily the physicists who persisted in them. The same was equally true of Landau.

Even Bohr, eye-witnesses recall, got the treatment from Pauli. "Be quiet!" the latter once shouted at him. "Don't make a fool of yourself!" "But, Pauli, listen . . . ," Bohr gently remonstrated. "No, I won't," came the reply. "This is bosh. I won't listen to another word."

And during one conference, talking in the lobby, Pauli cut

Landau short in just the same way, and when the latter tried to explain, or explain himself, he replied, "Oh, Landau, no, think it out for yourself." Those who knew Landau intimately say that it was a very unusual sight.

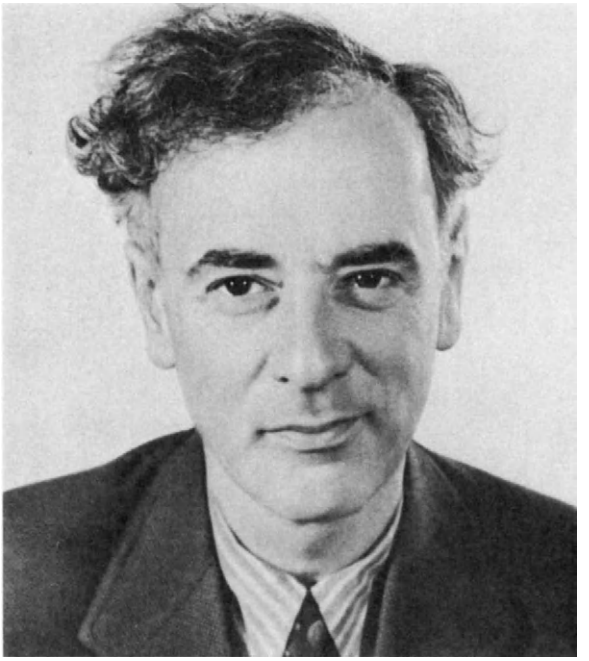
In general, it was known what behaviour at the seminar was simply not allowed the participants. God forbid that the speaker should utter a single word about how long it took to find the idea, or how difficult it was to arrive at the solution, or how the original choice of approach proved incorrect and unsuccessful, or impart any such "domestic details". Then, like lightning, the brutal comment: "Only your wife wants to hear about that." Or a sudden error in the calculations, unnoticed by the speaker, or which he is told about but cannot quickly put right – again a familiar cry: "Your mother ought to have taught you that."

What was strictly forbidden was to become annoyed either with Landau or with the rest of the audience. One had to give as good as one got, or better still make a witty retort. This was greatly appreciated, and put an end to the episode. If some unduly conceited person, or one who had not grasped the proper mode of behaviour in the school of Landau, showed himself aggrieved or hurt, the situation was made such that he eventually had to leave the seminar.

In all fairness, it must be added that Landau himself was not immune from sharp comments; and he did not take offence, but made rejoinders to them. It must not be thought that the outward parity of the relationship represented complete inward parity. Despite Landau's sincere and innate democratic view, there was in his relations with pupils a limit beyond which one could not go, resulting from the difference in scale, talent, knowledge, and capabilities – in all that constituted Landau the physicist. The teacher–pupil relations were somewhat like a chess grandmaster giving a simultaneous display against master players (of very high rank). "Though he behaved very straightforwardly with his pupils and was on familiar terms with many of them, all regarded him as immeasurably superior in all respects," Kompaneets recalls. "He was able to achieve everything that constitutes an ideal teacher, but for one thing: none of his pupils excelled him."

Although the expression "The author is usually right" became customary, in reality it was almost always Landau who was right. He very rarely made a mistake. And he defended his rightness to the end. If nevertheless he saw that he was in error, he always acknowledged it and immediately agreed with his opponent. He had great respect for anyone who could show him to be mistaken, and for anyone who could do something (perform a calculation, devise a method, obtain a solution) that he, Landau, had said was difficult. This attitude may have been the reverse side of the coin, a consequence of his own respect for himself as a physicist. He valued also an original approach in his pupils' work, their "scientific independence".

There were very few instances where Landau was wrong or in some other way was left at a disadvantage. It is probably for that



Landau in 1958

reason that the following episode is remembered. At one seminar, a graduate student was reporting on a journal article. "There is no exact solution of this equation," he said. "Impossible," Landau at once objected. The student had prepared his report a long time previously and had forgotten some of the details, so that he could not give a convincing proof, but he stubbornly repeated, "It's true; there's no solution." "Bring the journal here," said Landau, dispatching the apparently unfortunate speaker to the library, and rubbing his hands in anticipation. "Now I've got him on toast." The journal arrived. Landau looked at it and said slowly, in a tone of disappointment mingled with self-justification, "Um—m—m, from that point of view . . . "

Here is the story of the most resolute speaker. He was describing some phenomenon in semiconductors. He picked up the chalk and began to draw lines on the board. "Give us the formula first," said Landau. "Just coming to it," replied the speaker, again drawing coordinate axes. This dialogue was repeated several times. At the tenth repetition, Dau exclaimed, "Oh, draw then, blast you!" So the imperturbable speaker, with expressions of the greatest humility and respect, continued to have his own way. Landau of course was fully aware of the stratagem. But he accepted it. It was known as the "Pekar algorithm" after its "inventor".

Few got away with such methods, however. It was not easy to reach an understanding with Landau. "First impressions were very important to him both as regards people and as regards theories," says Kaganov. "Lack of success on first meeting (perhaps through saying something silly because of nervousness, or appearing incompetent in one's field) often put paid for ever to one's chance of a close relationship with Landau. Sometimes he was manifestly unjust to such people. A perfectly adequate physicist might at first give the impression of being a 'pathologist' in Landau's terms – and that was that."

Any such assessment was revised only with difficulty. "He was always an idler," Alexander Andreev remembers Dau saying of one physicist. This was not true, but the remark meant that Dau was unlikely to change his opinion.

Even pupils close to Landau might find themselves "excommunicated". There are many stories of his hardness and harshness. He excommunicated people not only for incompetence in physics but also for unworthy behaviour in general.

When, however, an error was tolerable, excusable, or accidental, Landau did his best to help deal with it. Smorodinsky recalls a physicist whom Landau scolded and who despairingly decided that if he wanted to go on living, which perhaps was desirable, he must give up his work. But it proved that Dau was the only one who lay awake and thought about the unidentified problem or the complicated question. The next morning he rang up the "victim", gave him the answer, and good relations were restored.

Usually Landau followed clearly all that a speaker said and was not at all distracted by the din at the seminar, or by the comments and questions. It did happen, very rarely, that the speaker confused not only himself but Landau too, who suddenly failed to understand something – in the formulation of the problem, in the calculations, or in the results. He then stopped the seminar and, outside, gave the speaker a dressing-down as to the need for a well-constructed script: "You really need a talking-to," Landau would begin rebuking him. "You're not a physicist, you're a . . . " After which the conversation continued on standard lines. Often it was continued in the evening by telephone. The calls kept coming, for further details and explanation, until complete clarity was achieved. The seminar was then reconvened, the paper given again in such a way that the point was understood not only by Landau and the speaker but by all others present. Everything was in order again.

The theoretical minimum

In describing the seminar, we have seen more than once that it was a special kind of thing, although seminars are a customary form of discussion in science, and a way of access to knowledge. It was not anything like this which Landau organized and

conducted. As to the theoretical minimum, one has to admit without any reservation that this creation of Landau's was entirely unique.

There are not very many people who like taking examinations. It is a nerve-racking business. Here too, indeed, Landau had arrived at a theory: those who want the marks do not like being examined, but those who want to acquire knowledge do like it.

But regardless of theory and regardless of liking, all of us with very few exceptions have to take exams, usually for some necessary purpose. Leaving school, entering college, gaining one's degree, and (for those who go far enough) presenting a thesis for a higher degree. Passing examinations always means gaining or winning something – not just satisfaction, but usually very tangible material benefits.

Those who passed Landau's theoretical minimum gained absolutely nothing of this kind. And passing it was the devil of a job. Landau's list of successful candidates contains but three-and-forty names. (There were many unsuccessful attempts.) Forty-three in all, from 1933 to 1961, chiefly from Kharkov, Dnepropetrovsk, Moscow, and Leningrad.

One asks, of course, what was this hurdle that so few were able to overleap? We shall try to answer the question, and first say only that it was not the kind of potential barrier that can be penetrated by means of the tunnel effect. There was no "percolation" here. Only surmounting. Only, in Pasternak's words, "over the barriers".

The theoretical minimum gave the impression of most resembling the minimum post-graduate qualification. In fact, they were very different. The latter was for some specialized subject. Landau's examination covered *all of theoretical physics,* together with the mathematics that is needed. Moreover, Landau's "minimum" was perhaps broader, deeper, and more complex than the "maxima" of many theoreticians.

The idea of the theoretical minimum, as we know, occurred to Landau during his time in Kharkov. He himself devised this method of training high-quality physicists, and himself worked out the syllabus of tests.

Originally there were two syllabuses, just as there were two theoretical seminars in Kharkov, one for experimentalists and one for theoreticians. Later, only theoreticians were given this training.

It is hardly appropriate here to go into the content of the syllabus: what had to be known in each branch of theoretical physics (generally speaking, all the most important things), and the order in which these were taken. We want to discuss principles, in particular the breadth and universality of the syllabus. It was as if, in this age of quite narrow specialization, a medical man, say an E.N.T. specialist, were expected to have a thorough (more than a student's) knowledge not only of the anatomy and physiology of the whole human body but also of the interaction of its various functions, its ailments, and its abnormalities. There is increasing evidence of very close relations and mutual influences of every part of the body, and only a supremely competent doctor is capable of achieving the principal aim, so to speak the ideal, of medicine: to cure the patient, not the disease. This ideal is rarely attained, the reason being that few physicians are adequate to the task of reaching the combination of universal learning and the ability to make active use of that learning.

So it is in physics. Usually, and for example when achieving the minimum post-graduate qualification, the theoretician enters upon some particular problem, some branch of physics. Landau's pupils entered upon, nay plunged into, all of theoretical physics as an indivisible whole. He trained and nurtured theoreticians of broad profile, able to attack various problems and to go, without great difficulty, from one branch to another.

Whatever the future specialization or type of work for each pupil, he had to master the minimum knowledge laid down by Landau, and be able to apply it with freedom. "Of course," Evgeny Lifshitz has written, "he did not expect anyone to be as universally well versed as he himself. But he thus manifested his belief in the integrity of theoretical physics as a single science with unified methods."

The main thing about the theoretical minimum, we are told

by one of Landau's pupils, was the sort of wide lateral vision that it gave, the ability to switch with ease to different, even quite new, problems. But it was very hard to learn thoroughly the whole of the theoretical minimum. It took up two to three years of one's life. Even then, many were unsuccessful.

In principle, anyone who wished could take the theoretical minimum. There was a fixed and very simple procedure. One simply rang up and applied. Landau then and there made an appointment. No one was refused, nor was there any mention of his own burden of matters to be attended to.

Neither Landau's views nor his actions were, to be frank, conventional here. Most important, they were constant with time, as physicists say. More precisely, external changes such as being elected an Academician did not affect them; he still continued to show a true inward democratic feeling.

Of course this is quite regular and not a reason for admiration. But we know of so many instances of the opposite. There was some nice, straightforward, attractive person, who became an Academician, say; and it was as if someone else had taken his place. True, he is now very busy, with many concerns and duties. But the time he can call his own has also acquired for him an added value (one would like to say, a "surplus value"). He who was yesterday a person well known and entirely accessible for ordinary contact has today become some kind of god. Access to him is either forbidden or else greatly impeded, with a secretary as an interposed obstacle.

None of this applied to Landau. Well might Shalnikov call him "the most unpretentious of men". This did not prevent him from liking, as he himself said, to show off, for instance with a clever or paradoxical remark, which for him was an amusement, a kind of jolly game, and in particular never referred to anything that should be taken seriously.

In his article "Landau's plain talk" (see Appendix), Evgeny Lifshitz published and commented on Landau's replies to letters from various people he did not know who were in some way interested in physics. They were all signed simply "Yours, Landau," or "Yours, L. Landau." Here are some extracts from these replies.

"I am sending you the 'theoretical minimum' programme, in which you can, if you wish, be examined section by section, by myself or by my associates. . . . My telephone numbers are also indicated in the programme. You need not be afraid of me – I never bite."

"I shall gladly help you. . . . I would recommend the following course of study. . . . After you have done this, please phone me (best between 9.30 and 10.30 a.m., when I am almost always at home, but if necessary at some other time of the day), and come and see me. I shall examine you and supply you with a programme for further study."

"Phone me" – and all were received for examination. "No certificates are needed to take these examinations," Landau used to say.

The examinations themselves followed an equally simple and fixed procedure. Landau invited the candidate into a room, gave him the work, and went away.

He later usually came in person and looked to see whether the problem had been solved. (The whole examination consisted of problems; he asked nothing about theory and set no purely theoretical questions.) If it had, Landau immediately set the next one. If not, he would say, "You are a little slow." (A remark hardly calculated to improve the output of one's grey matter.)

The surprising thing is that most of the problems were the same every time. Everyone knew this. They knew which problems were used. Landau knew that they knew. What sort of an examination was this? How could anyone fail?

"Very easily," Lifshitz tells us. "For one thing, the problems could be slightly altered. But principally, if a person knew how to solve all these problems, was able to solve them, then he *knew and understood* all that one needs to know and understand in order to work at ease in theoretical physics. And that was the purpose of the theoretical minimum."

But the demonstration of how easy it was to fail, even with this "most favourable treatment", is given by the figure already mentioned: 43 who were able to complete the theoretical minimum, in the space of thirty years!

First was always the test in mathematics. Landau constantly emphasized and expounded the necessity of this sequence in taking the theoretical minimum. In almost every one of the reply letters already quoted, we find the same words and the same arguments. "Bear in mind that a mastery of mathematics is especially important." "You must first master properly the techniques of theoretical physics. . . . Mathematical techniques are the foundations of our science." "You must start with mathematics which, you know, is the foundation of our science. The scope is indicated in the programme. Bear in mind that by 'knowledge of mathematics' we mean not just all kinds of theorems, but a practical ability to integrate and to solve in quadratures ordinary differential equations, etc." "As you have yourself understood, a theoretician must above all know his mathematics. What is needed is not all kinds of existence theorems, on which mathematicians lavish so much praise [Landau called this 'mathematical lyrics' – A.L.], but mathematical techniques, that is, the ability to solve concrete mathematical problems."

Landau knew very well what he wanted, expected, and got from his future – or potential – pupils. The theoretical physicist of today simply cannot exist without an unhampered mastery of mathematical techniques. Without this, neither talent nor intuition nor inventiveness will make him what Landau would have regarded as a proper scientist: either his ideas will need to be worked out by someone else or he will have to cooperate from the start with a "technical" physicist.

Landau himself was held to be a true champion in this respect. Physicists say that there was no other theoretician with such a masterly command of the techniques of his profession; for example, if all the leading lights were given some problem within the scope of existing theories, Landau would be the first to solve it. Within the limits of the possible, he understood the methods so well that he could do practically anything; mainly because he had an unusually strong and powerful mode of thought in physics, to which there were apparently no bounds. This was what enabled him every time to find, or divine, or devise, a technique adequate for the problem.

It would, of course, be absurd to suppose that he expected anything like this from young people when they first presented themselves for examination. Skill and ability had to accumulate and be perfected gradually. But even as pupils they, and especially the best of them, became masters of the technique of solution – though still naturally a long way behind their teacher. Even the "candidate-pupils" who had achieved the theoretical minimum had also learned more and more complex methods and encountered new fields of mathematics as they went through the successive examinations, whereas these had not been necessary at the first examination, even though it was in mathematics. This was foreseen in the syllabus itself: some areas of mathematics occur together with the areas of physics where they find application.

But, independently of the level and extent of knowledge, Landau's requirement was constant and fundamental. The mathematical equipment and technique must be so far mastered that technical difficulties did not impede the search for ideas and approaches to the problem, did not call for any great additional expenditure of effort, did not demand the principal attention.

There is an obvious analogy with the ballet here, although Landau himself would hardly have been gratified by the comparison: how could one compare the two, or even mention his beloved theoretical physics in the same breath as ballet, which he simply refused to regard as a form of art? However, most people would no doubt remain unshocked by such a comparison. When real talent in the classical ballet *par excellence* is not obstructed by matters of technique, when technical difficulties do not exist, so to speak, for that dancer, then nothing interferes with creating the image, opening out, finding and expressing the choreographic idea. There is then, as we see, a similarity, and the association is not accidental.

Of course, it quite often happens in physics that some very arduous problem is solved only by means of an appropriate advanced and refined mathematical technique, luckily found or specially contrived for that problem. But sometimes the problem

does not yield, and all attempts to find such a technique and solve the problem are unavailing. This, however, was not yet relevant to a person taking the first examination. The methods concerned were quite ordinary, well known, and widely used.

In one of the letters already quoted, Landau describes the minimum level of knowledge and ability that one should have before taking the first examination: "I would recommend the following course of study. First, learn to be able to perform correctly (and as rapidly as possible) differentiation, integration, solution of ordinary differential equations in quadratures; study vector analysis and tensor algebra (i.e., how to operate with tensor indices). The principal role should be played here not by the textbook but by a book containing a set of problems – it does not much matter which book, so long as it contains enough problems."

Landau contemplated writing his own book on *Mathematics for Physicists,* corresponding exactly to his requirements, but never had the chance to do so. Nevertheless, he took every opportunity to spread his views regarding the specifics of mathematics teaching for physics students, and in reviews he tried to influence the syllabuses and courses in mathematics for physicists at colleges and universities. "I believe", he wrote, "that the teaching of mathematics is ripe for a most serious reform. Those who undertake this most important and difficult task will deserve the sincere gratitude of the already trained physicists, but in particular of the numerous members of future generations."

If the contestant seeking entry to the school of Landau passed successfully through the initial tests, it was time to prepare for the remaining ones, in theoretical physics itself.

The sequence was: mechanics, theory of fields, quantum mechanics, statistical physics, mechanics of continuous media, macroscopic electrodynamics, and relativistic quantum theory. Seven examinations, with two in mathematics making nine in all. They covered all the chief areas and aspects of theoretical physics, and each of these had to be known equally well. "It is necessary to learn ALL the main branches of it [theoretical

physics – A.L.], and the sequence of study is dictated by their mutual relationship," wrote Landau. "As a method of study, I can only emphasize that you must perform all the calculations by yourself, and must not leave it to the authors of the books you have read." As already mentioned, the examinations did not consist in expounding the theory or in deriving formulae, but in solving actual problems in physics.

The difficulty of the examinations is shown not only by the well-known figure "43" but also by the long time taken by the majority to master the theoretical minimum, even though it was attempted mainly by the best of the young theoreticians. But Landau himself was not at all disturbed or worried by the long times, which in no way worsened his attitude, so long as the examinations were in fact passed. He wrote on this subject: "In practice the time ranges from two and a half months in the case of Pomeranchuk, who practically knew everything at the outset, to several years in other also successful cases."

Rumour has it that Pomeranchuk took even less time, about a month – so great was the belief in the outstanding capability and talent of Chuk, at that time 22 years of age. Evgeny Lifshitz completed the theoretical minimum even younger, at 19; he, like his teacher, was an "early bird", entering the university at 16 and finishing the course at 18, and completing Landau's theoretical minimum a year later. The young Isaac Khalatnikov also dealt with the minimum quickly, in two goes. For the first, he came from Dnepropetrovsk in the autumn of 1940 and passed half of the examinations, completing the remainder in the spring of 1941, shortly before the Soviet Union was at war.

There were other examples later of such precocity, which was always impressive. Thus the teachers and senior students at the Moscow Physicotechnical Institute knew at once when Sasha Andreev, who was still a student in mid-course, had already passed the whole of the theoretical minimum.

The work was laborious and difficult; much had to be mastered. The subject of study, the whole of theoretical physics, was a complex one, and seemingly unlimited. One had to know it in detail, understand it, and be able to use it. One's knowledge

had to be displayed in detail, and the "range of ability" was very clearly specified. It was also necessary to achieve considerable self-control, in order to have the strength to complete the long and arduous ordeal. For almost all the candidates were also working or studying at the same time.

The examinations in theoretical physics, too, were not necessarily just the next stages after mathematics, but might be higher ones according to requirements: a knowledge of mathematics was prescribed within certain limits, and easy unhampered ability to use it within those limits. Theoretical physics had to be known as a whole, with the interrelation of its parts.

This knowledge and ability acted as a springboard for subsequent work and as a threshold which a physicist might cross in order to be able to regard himself as adequately prepared to become a theoretician at the level of Landau's demands. He would simultaneously, and this was perhaps especially important, become a recognized, "legitimate" pupil of Landau's, a full member of the school. This "entry", however, was never in any way formalized. The only trace of it on paper is in the booklet where Landau wrote the names and dates of all candidates for the theoretical minimum, with a page for each year, and their subsequent distinctions (doctorate, Corresponding Member of the Academy of Sciences) up to the end of 1961. (Since then, of course, most of them have advanced further.) That was all. There was not even a certificate, his pupils comment. Certainly there was no piece of paper attesting that the examination had been taken and passed, and that someone had gained the title "pupil of Landau". Well, who would have issued such a thing; which would have been the appropriate office?

There was also no change, at least for the time being, in the physicist's official standing. Landau too, it seemed, gained no tangible reward from this additional great and constant labour. His extra burden was never in any way acknowledged. It was all a matter of pure enthusiasm – on both sides. One very sardonic physicist with a scorn for elevated language nevertheless said that the enormous effort in setting up the theoretical minimum and holding the examinations was the finest witness to Landau's selfless devotion.

Of course, Landau spent a great deal of time by himself giving the examinations to all comers. (This was his original arrangement, and lasted for many years. He alone was for long the whole "board of examiners". Only later, when the number of those wishing to try their skill became very large, did he begin to get help from his closest pupils and colleagues. Even then, however, he continued to give the first examination himself, in order to form an impression of the newcomer.) The main laborious task, though, was to plan a course of theoretical physics, to devise and develop syllabuses in every branch of it, to discover the proper sequence, to choose and distinguish what needed to be known especially thoroughly, and then to bring everything together and compile a design of all theoretical physics; this was indeed a mighty labour.

There was admittedly a foundation and a guideline, namely the books of the *Course of Theoretical Physics*. But, as the writing and publication of the various volumes occupied many years, and indeed was not quite completed in Landau's lifetime, by no means all who prepared themselves for the theoretical minimum examinations were able to use most of the volumes, and this had to be taken into account when constructing the syllabuses.

Although the whole effortful process of developing the theoretical minimum and holding the examinations was really upheld by pure enthusiasm, it would be wrong to suppose that no practical ends were intended or pursued. On the contrary, this was a sort of means of generating more pupils, whereby Landau found the physicists who were most gifted and closest to him in spirit; and a young theoretician, or a person who wished to become one, was able to study science in the most serious way.

"The passing of these examinations imposes no mutual obligations on anyone," said Landau. "Except on myself. If I notice a capable youngster, I regard it as my duty to help him make a start in science."

There was a kind of unspoken agreement. Some of the suc-

cessful candidates became Landau's graduate students; others simply became his pupils, with the various consequences that flowed therefrom: constant association, active participation in the seminar, discussions with Landau about their work, and sometimes co-authorship with him. It is not necessary for a theoretician to hold a regular appointment. He does not need special accommodation, a laboratory, apparatus, facilities. He works with books, journals, pencil and paper.

It was not even necessary to live in the same city as Landau in order to be a successful theoretical physicist, and his pupil. Landau always asserted his confidence in this possibility to all in whom he perceived a genuine interest in physics. We find this, for example, in those answers he gave to numerous letters. "You are apparently seriously interested in physics, and I am very anxious to help you. . . . I shall be glad to send you appropriate programmes, and after you have studied this material, I think you will be ready enough to start," he writes to a student in Penza, 500 kilometres from Moscow. "If you are seriously interested in theoretical physics, I shall gladly help you engage in what I also regard as an alluring science. I am sending you the 'theoretical minimum' programme, in which you can, if you wish, be examined section by section, by myself or by my associates."

On one occasion, Landau received a letter which said, "At one time Einstein did not refuse to help his student Infeld, and I therefore decided to write to you in the hope that you will not refuse my small request. . . . I am very fond of theoretical physics. . . . Forgive me for troubling you, but for me it is very important and, although it is perhaps not quite proper, but it happens in life, if the going gets rough, that there is not always room for propriety."

Such "improprieties" were in fact likely to attract Landau. In any event, he wrote: "I am glad to answer your letter. . . . I shall gladly help you. . . . If you pass this entire programme (this will take from one to three years, depending on your knowledge and diligence), then I shall consider you to be fully prepared for scientific work, and will attempt to help you, if you so desire, with arrangements for this purpose."

More answers: "If you have enough will, you can study theoretical physics independently – after all, it requires nothing but books and papers." "The fact that you ardently want to engage in physics is very good, for ardent love of science is the first token of success. Fortunately, theoretical physics is a science for which attendance at a university is not at all necessary. I am enclosing in this letter a programme. . . . If you are successful, I hope to be able to help you find an opportunity to work in theoretical physics even in the event that you graduate not from the Moscow State University but only from the Tula Pedagogical Institute," he wrote to a student in Tula.

"Foreign languages, unfortunately, are essential. But don't forget that you need no special ability to master them, for even very dim-witted Englishmen have a fair knowledge of English," was his humorous way of encouraging one young worker.

Again his innate goodwill is touching. And how sympathetically he reacts to declarations of a love for physics! But during the examination, though the goodwill might remain, it had to be pressed aside to make room for exactingness. There was a kind of "natural selection": if you could not pass the examination, then you must go away and take up some other activity, was the recollection of one pupil who had come successfully through all the tests. Those whose further work offered no prospects for science, and for themselves, were ruthlessly weeded out. Dau rarely erred one way or the other. He was particularly severe on those who were insufficiently diligent, and here he was never wrong.

But those who were able to stay the course gained much in return. The first fruits for the majority were probably self-esteem (fully deserved) and self-affirmation.

Just as anyone could, in principle, attend the seminar, so everyone could, in principle, take the theoretical minimum. There were no restrictions or conditions except to be at the proper level of knowledge and abilities. This all-embracing potential equality and genuine democratic system made the school of Landau a peculiarly attractive one.

In fact the selection was very rigorous, sometimes even

savagely so. But it could be said to follow a single threefold criterion: knowledge, abilities, and love of physics. All other aspects, such as might form the answers to a questionnaire, were ignored.

The special charm and attraction lay precisely in this informality, unofficialness, absence of red tape; in the passing of the examinations and the "enrolment"; in the whole life and activity of the school of Landau. Perhaps, as one pupil remarked, the theoretical minimum was useful by its very existence, which already created a certain micro-climate in the physicists' world. All felt the unusualness and attractiveness of these unofficial, unprescribed relationships. Moreover, as generally happens when there are no formal links, there was instead a different system of interrelationships and bonds.

Alexander Andreev recalls that at the Moscow Physico-technical Institute one had only to have passed the theoretical minimum in order to be exempted from examination. Why bother with empty formalities, why indeed? That examination was child's play in comparison with the theoretical minimum.

In brief, to belong to the school of Landau was a very high distinction, which provided the most direct road, in practice, to fruitful scientific work.

The *Course of Theoretical Physics*

The chief help in reaching one's goal was, as we know, the set of volumes forming the *Course of Theoretical Physics.* Every learner used these books, says Andreev – a recent graduate student of Landau's for whom almost the whole *Course* was available in published form.

Even the most resourceful popularizer would find it hard to give the non-specialist reader an idea of the *Course* and how it differs from similar works. The only possible way, and one that is likely to be of interest, is to quote some opinions. Let us hear what Landau's pupils have to say.

"Only Landau could have created such a course. Literally

without moving, without turning to any publications, at any moment, he was able to start work on a topic that had attracted his attention, in any branch of theoretical physics. He alone, therefore, was in a position to create an encyclopaedic handbook on the greater part of our science, every topic being described as if the authors had newly discovered it, and the approach being both more rigorous and more easily understood than that in other textbooks."

"It is a conceptual course of theoretical physics, a monograph, a universal and permanent foundation. It is Landau's true memorial. He alone could have created it; no other person. Books more striking than these will never be written in all the world, no matter what their subject. For instance, there have been all kinds of books entitled *Quantum Mechanics*, but not one like Landau's. And he has written upon the whole of theoretical physics."

"These books are most of all distinguished by their remarkable choice of material. They contain what is set down for all time. They are among the pearls of science. One Western theoretician, for example, takes *Statistical Physics* everywhere. 'Look it up in Landau' is a frequent remark. For this work is unique in selecting what is chief and most important."

"As regards the established field, it is all in the *Course*. If there is no answer there, it will not be found anywhere. Everything necessary is included. A remarkable selection! Problems that lead nowhere do not find a place."

"The volumes of the *Course* have other special features. The better you know them, the more you can get from them. The higher a physicist's level, the more profound and essential he finds them. They present a high barrier: they are difficult, and cannot be understood at all if one starts by knowing nothing. And yet — Landau's pupils' pupils found them less hard!"

"These books are written for the knowledgeable. Even a very skilful and famous physicist was unable to expound one passage. They are for grown-up people, and it is very difficult properly to master them all. From them, however, one can learn theoretical physics without being in Moscow or Leningrad, say; without a

teacher, without a spoken word; one can know enough of it to read journals, and that is not easy – to read papers in scientific journals. The books of the *Course* contain more than is read in our colleges."

"Another important feature is that they contain practically no mistakes. New things have to be added, but nothing needs to be changed."

"One must say that the reader himself has to know what he wants from Landau's books. They give no explanation of why a particular topic is being discussed and what practical benefits can be derived from it. Theoretical physics is presented in its most pure and rigorous form. Any digressions, Landau called 'lisping'."

"These books are pragmatic while being absolutely rigorous. At the end of almost every section, there are model solutions, to show what one has to do."

"The *Course* has attained world-wide fame and popularity. Landau and Lifshitz have written celebrated and epoch-making textbooks of theoretical physics used by students throughout the world and referred to by everyone who works in theoretical physics."

They have been published in many countries, on many continents, in many scripts – Latin, Slavonic, various characters. One would think that every physicist can read English. But still the *Course* is published in Hindi, Vietnamese, Japanese, Chinese. Latin America reads it in Spanish; Britain and the United States and Australia, in English. In Europe, almost everyone has it in his own language. They read it and study it. Well may one hear the remark "Look it up in Landau" so often – as an aid in searching, at a pause, as a debating point.

There comes to mind a comparison with the celebrated French *Encyclopédie.* This is no chance association (though it is admittedly not an essential one). Many of us have from an early age known the word *encyclopédistes* to be fraught with deep meaning: not only to take the sum of all knowledge of the time, to systematize it, and to bestow those riches on all, but especially

to have a meditated and bravely achieved attitude to the world and all that is therein. So, its volumes enclose not only an encyclopaedia of knowledge, but also, we know, a great fund of morality. The *Encyclopédie* was indeed to some extent the rallying-flag and the ideological base of the French Revolution.

Of course nothing like this could be expected in theoretical physics, one of the most abstract and unsocial of the sciences. Yet in these books too there is not only a complete tabulation of physical laws and knowledge, not only a discussion and explanation of phenomena, but also a particular view of them, repeatedly tested in thought and in life. Landau and Lifshitz' "encyclopaedia" has for this reason also helped in gaining familiarity with physics, and in working at physics, and (for all the abstractness of its content) has also acted to preserve the integrity of ideas in physics – in particular by its high standard, but also by its own integrity and the unshakeableness of its fundamental principles.

The labour of Landau and Lifshitz continued steadily, year by year, according to the established plan. Any interruption in the publication of the books was not the wish or fault of the authors: they did their part regardless of everything, for they felt a sense of responsibility for the development of physics in their country and a conviction that these books were requisite, nay essential; that without them there could be no strong, competent, progressive, and active school of theoretical physicists.

The chief distinguishing principle of the *Course*, in contrast to other similar works, is its unified approach, wherever possible, to different areas of theoretical physics; for example, the authors built up the whole of mechanics and also electrodynamics on the variational principle. This generality of approach to various problems in physics was indeed a distinctive feature of Landau's work, and one consequence of it was his astonishing ability, when solving some complex problem, to find and apply a mathematical technique from a totally different branch of physics seemingly unconnected with and unrelated to the one under consideration.

The volumes of the *Course* also contained, naturally, a fair amount of the authors' own work, discoveries, approaches to problems, derivations, and opinions. For instance, in one very burdensome year Landau reconstructed for himself the theory of shock waves. He made all the calculations mentally, without paper and pencil. When the volume *Fluid Mechanics* was later written, during the war, his work was given an important place in it, being made the basis for the treatment of shock-wave theory.

The creation of the *Course* followed two parallel paths. One was its further development by the continued writing of new books; the other was the publication of new editions of existing ones. This too needed a great amount of time and effort, because none was a mere reprint: the authors treated every volume as a new one. Everything was attentively revised, rewritten, supplemented by new matter to improve the forthcoming book, include the latest ideas, and reflect progress in the relevant area of physics.

Afterwards, all this work was undertaken – as it still is – by Evgeny Lifshitz, who is now helped by a younger pupil of Landau's, Lev Pitaevsky.

It is interesting to turn over once more the yellowing pages of the "Theoretical Minimum Syllabuses for Senior Scientists at the Ukrainian Physicotechnical Institute". They have had time to turn yellow, these duplicated leaflets distributed to the Institute staff in 1935. But our interest in them now is a special one. At the bottom of each page (dealing with one section of theoretical physics as Landau was already gradually arranging them: mechanics, statistical physics, electrodynamics, quantum theory, theory of continuous media) is a bibliography, and notes for each item in the syllabus. The list mentioned various authors, Soviet and Western; some books had been only partly, or not at all, published in Russian translation; and of course many different styles were represented, because different authors must mean different schools of thought, approaches, interests, emphases, forms of exposition, and everything else.

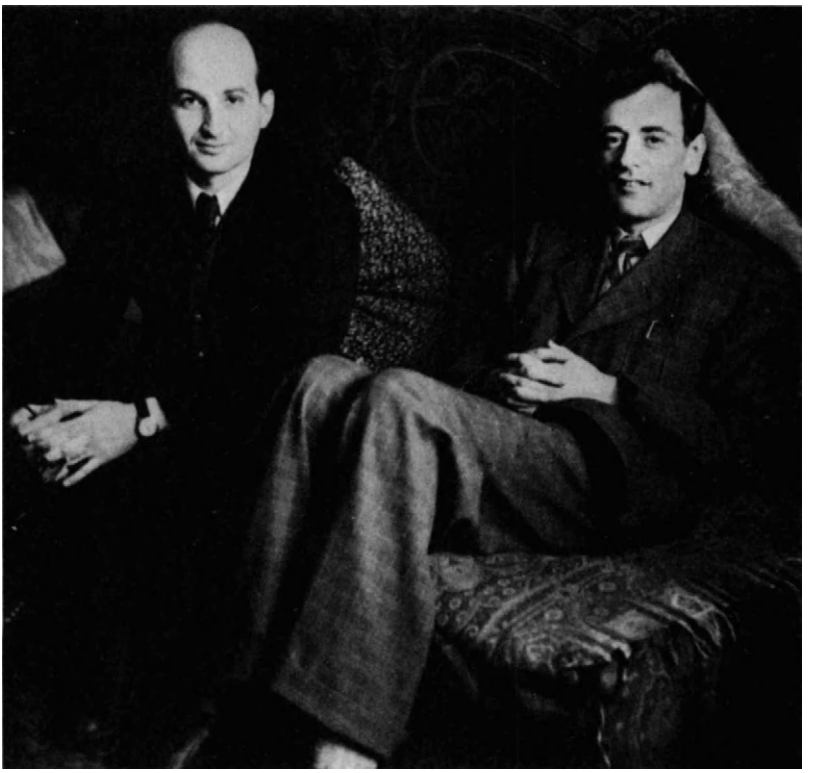
Even at that time Landau recognized the need to create his own course of theoretical physics, and this he set about doing. "It was there, in Kharkov, that Landau had conceived the idea and began to implement the programme for compiling a complete *Course of Theoretical Physics* and *Course of General Physics,*" Lifshitz recalls. "All his life long, Landau dreamed of writing books on physics at every level – from school textbooks to a course of theoretical physics for specialists. In fact, by the time of his fateful accident, nearly all the volumes of the *Course of Theoretical Physics* and the first volumes of the *Course of General Physics* and *Physics for Everyone* had been completed."

How was the *Course of Theoretical Physics* written? How did the authors share and carry out their joint work? Such a question inevitably arises when there is co-authorship, and almost as inevitably one recalls Ilf and Petrov's comic reply; they said that one goes round the editorial offices and the other stays at home to guard the manuscripts. Landau and Lifshitz had no need to guard their manuscripts – nobody else could have made any sense of them. Indeed, they paid little attention to editorial offices.

But to be serious, it must be acknowledged that one cannot readily understand why Ilf and Petrov kept their "production relations" strictly secret; the reason, no doubt, died with them. With Landau and Lifshitz, everything was open from the start. They made no secret of how their joint work was done.

First of all, there was a strange thing about Landau. Though a very fluent speaker, he became painfully helpless when he had to write anything. The mere need to put down in writing even his own ideas embarrassed and constrained him. "Excuse the delay, due to my extreme antipathy to the epistolary art," he wrote, and again "It is difficult for me to write letters and it therefore takes me a long time to get round to it."

This idiosyncratic attitude to letter-writing was well known, and of course everyone accepted it. It was the reason why any work done jointly with him was usually written up by his co-authors. "You have probably heard that I have utterly no



Lifshitz and Landau (1948)

aptitude for writing, and all my writings are with co-authors," he informed one correspondent. Moreover, even the papers containing only his work, without co-workers, were written for him by Lifshitz, from the middle 1930s until the end. The latter, who had to deal with this peculiarity of Landau's to a greater extent and for a longer period than anyone else, has explained it in the following way: "It was even difficult for him to discourse in an article on scientific work he did alone (without co-workers!), and all his scientific papers were written for him for many years by others. His insuperable desire for brevity and clarity of expression forced him to devote so much time to the choice of

each phrase, that ultimately the task of writing anything, be it a scientific article or a personal letter, became a torment."

Another point is that usually the books were many times rewritten; Landau never grudged time or effort for this, and here nothing held him back. In this respect Dau had zero inertia, Lifshitz remarks. On occasion, Landau termed his co-author a "Mark $n-1$ patriot" (fighting for the previous version). He himself was often ready to go on to "Mark $n+1$". But he never tired of telling all and sundry: "Evgeny is a marvellous writer."

This, however, refers more to the technique of working. True co-authors must have thorough discussions together of plans, ideas, and the whole text, must together seek the best solutions, together remove the complications that arise. That is, there must be a great inward closeness, the same way of thinking, the same approach to a problem, the same reactions.

And so there was. Without a doubt, Landau and Lifshitz had a very special alliance and friendship. The role of Lifshitz too was an exceptional one. Such a person was essential for Landau to be able to do what he did do, says Yury Rumer, and he gives as an example the *Course of Theoretical Physics*: "All will certainly agree that it could never have existed without Evgeny. He contributed many new scientific ideas as the course was being developed; he expended on it much selfless labour; he achieved thereby clarity and precision of treatment."

Landau needed a partner who not only perceived and understood his intentions but could think everything through with him, raise objections, be doubtful, disagree, search unwearyingly for the most finished way of expressing the matter, and not rest until it became clear. In brief, a partner who himself could rise to a very high level. And there was also a need for psychological compatibility, to use the current vogue-term. This too existed. For that reason, they achieved a great deal together with great ease.

For instance, they were so accustomed to sharing their thoughts that, when the Moscow Physicotechnical Institute was being set up, they started one course of lectures between them. Since journeys to the suburb of Dolgoprudny were long and

arduous at that time, they took it in turns. There was such complete interchangeability that, if one said where he had finished the previous lecture, the other could immediately go on from there.

They also liked to go on holiday together, setting off by car. They did not mind a lack of comfort, for they were not demanding as travellers. Landau simply took care to carry a supply of his favourite sunflower-seeds. "You're a 'discomforter'," was his usual reply to the reproaches of the pernickety Lifshitz that the car seat was covered with husks. He meant that his friend's desire for tidiness tended to diminish his comfort.

Evgeny Lifshitz is notable for his loyalty – active, and sometimes even aggressive – to all Landau's principles, and of course to Landau himself. This loyalty to principles continually spurs him on, whether preparing new editions of volumes published in Landau's lifetime, or new books planned by Landau, to accomplish once again this mighty task. It is hard to comprehend what such a task implies, and one realizes the full extent of Lifshitz' conscientiousness, his driving sense of responsibility both to the reader and, perhaps even more, to the shade of Landau.

The School of Landau

Landau was a born teacher and educator – divinely inspired, as the saying is. All things were happily combined in him: he had the wish, the passion, and the ability, to teach; and he was much concerned for the establishment at all levels, from the highest to the lowest, of a system to teach and train physicists.

Courses and textbooks were to be the foundation of all his broad plans. As well as the *Course of Theoretical Physics,* there is also the "easy" *Shorter Course,* published after his death; the *Course of General Physics,* originally written also by Landau and his co-authors before the war, but not published until after his accident; and *Physics for Everyone,* written with Alexander Kitaigorodsky and intended for "the broadest range of readers".

So no "contingent" of readers or students was neglected, from the skilled theoretical physicists to the schoolchildren and anyone with scientific interests.

There was also a ramified system of oral instruction: the successive generations of Landau's pupils and *their* pupils are continuing his traditions in the teaching of undergraduate and graduate students, using his methods, and striving to reproduce his style.

"Landau developed a rigorously thought-out system of education in science. . . . He gave attention to every stage of the intellectual growth of the scientist, from secondary school to the Academy of Sciences," said his pupil Alexander Kompaneets. And though he did not particularly try to train Academicians (he did sometimes give them preaching, if not teaching), he remained an authority for the great body of physicists, including Academicians, even before he himself became one.

Having mentioned this, let us summarize what happened. First of all, Landau, the author of many remarkable publications in almost every field of theoretical physics, the creator of an outstanding school, was still for some reason not a member of the Academy of Sciences. This injustice probably distressed and annoyed many, but in particular Kapitza and another great Soviet physicist, Academician Vladimir Fock, who wrote to Kapitza, "Landau should certainly be made a Corresponding Member, and I hope that this will soon be possible." At the beginning of 1941, they jointly wrote a report on Landau for the Physical and Mathematical Sciences division of the Academy. Here are some extracts from it.

"Lev Davidovich Landau, a doctor of physical and mathematical sciences, is one of the greatest of theoretical physicists. His work has been universally acclaimed, both in the Soviet Union and abroad. His theoretical researches cover a very wide area of modern physics, including nuclear physics, low-temperature physics, and solid-state physics. In all these fields he has put forward many original ideas. A distinctive feature of his work is a close connection with experiment; it always deals with the most immediate and urgent problems of present-day

physics. He is characterized by great rigour of thought, which often restrains the full sweep of his imagination. He has an excellent grasp of the mathematical techniques of modern physics. L. D. Landau has created around him a school of young Soviet physicists, to whose training he gives a great deal of time. He has trained several young scientists who have now attained advanced degrees and hold professorships. He also conducts a seminar at the university."

They also referred directly to Landau's role and authority: "Many of our scientists discuss their work with him. It is uncommon to find anything on theoretical physics published in this country which does not express thanks to L. D. Landau."

But the war came, and evacuation; the Academy of Sciences, like the rest of the country, refashioned its existence, work, and interests.

Lev Landau became an Academician in 1946, of course now by-passing the "Corresponding Member" stage – this was not a case, a figure, or a scale suitable for gradual promotion through the ranks.

Three of his pupils have become Academicians (Isaac Pomeranchuk, Roald Sagdeev, and Evgeny Lifshitz); here we regard as his pupils those with the "formal qualification" of having passed the theoretical minimum, since there are other physicists, including Academicians such as Arkady Migdal, Ilya Lifshitz, and Vitaly Ginzburg, who regard themselves as Landau's pupils but have never taken his examinations. There are many Corresponding Members among them; many more than at the beginning of 1962, when Landau wrote these pages still in existence, carefully listing names and dates, as if something urged him to cast up his accounts.

In general, however, his work did not in any way resemble a summing-up. Rather the contrary: everything moved steadily forward, everything was imbued with youth and all its attributes: energy, vigour, impetuousness, effectiveness, assurance of one's rightness, and intolerance of what seemed to be a profanation of science; and youth, as we know, is not and indeed cannot,

should not, be a time for taking stock. These are rather states at right angles to each other or, as physicists and mathematicians would say, mutually orthogonal.

Landau was youthful in character, nature, and behaviour, but especially youthful was his school in the age of its members and also in its spirit. Kaganov has given an apparently accurate and extended account: "Democratic procedure is not a simple concept. It depends on the nature of the relationships within an existing group, and on the ease with which one can join the group. In Landau's circle, it was very evident; I prefer not to use the word 'deliberate', because the straightforwardness of the relationships was a natural one, not shown off to anyone. Many were on familiar terms with one another and with Landau, and nobody was surprised at arguments (sometimes acrimonious) between scientists of quite different age and rank. I am sure that many were repelled by just this democratic and straightforward behaviour in the school of Landau. Those around Dau seemed like a group of close friends (and many in fact were friends). It is hard for a mature person to become a member of such a group. The school of Landau (when I knew him) therefore grew mainly by adding young people: new pupils of Landau, and his pupils' pupils. Young people, as a rule, more easily overcame the barrier of psychological incompatibility."

This then explains why young people more easily established themselves in the school. Conversely, Landau, like any teacher, could more readily shape the personality of a pupil who came earlier under his control, who had been less subject to other and extraneous influences. And in the school itself, of course, there was a collective mutual polishing and finishing-off, which also is much easier and less painful at an early age.

Was the school of Landau, then, a caste or not? To some extent, of course, it was. There was something of a closed society about it. But not of a limited society; and the closedness was very relative. One pupil has stressed the absence of sectarianism in Landau's school and seminar: they were just an assembly of participants, a group of like-minded persons, not organized, not having a formal structure.

They might be working in different Institutes, living in different cities, and only a few times each year visit Landau in Moscow, yet they were still genuinely his pupils. In this respect Landau resembled Bohr, although the latter's pupils came not from other cities but from other countries and even from other continents.

The mention of the school of Landau implies for many a necessary comparison with that of Bohr. But, Bohr being Bohr, to make assessments and comparisons one really has to put forward physicists, especially those who knew both men well.

"Landau was enchanted by Bohr's talent as a teacher, his personal charm and his personality as a scientist. In Landau, Bohr soon recognized not only a gifted scientist but, in spite of a certain abruptness and extravagance of manner, a man of great spiritual worth. Landau regarded Bohr as his only teacher in theoretical physics and I think he also learned his teaching and training methods from Bohr. The success of the school of theoretical physics which Landau subsequently founded in the



Bohr and Landau (1961)

Soviet Union undoubtedly owed much to Bohr's example," said Kapitza.

"He was not only an outstanding physicist but also a genuinely outstanding educator, a born educator. In this respect one may take the liberty of comparing Landau only to his own teacher – Niels Bohr." These are the words of Evgeny Lifshitz.

"The contact with Bohr and his celebrated Copenhagen school permanently fixed Landau's scientific aims, and taught him to distinguish what was truly progressive from what were merely (though sometimes clever) tricks. This scientific tradition he implanted in the Soviet Union among his pupils, they in turn among their own pupils, and so on to the third or fourth generation. We are all proud that our pupils are the grandchildren of Landau and the great-grandchildren of Bohr," said Kompaneets.

From Bohr, Landau also inherited his democratic approach, his dislike of servility, and his love of team-work, of thinking and seeking along with his pupils. And of course his special relation to and love for physics.

But Landau was, naturally, not exactly the same as Bohr; he could not be. Such personalities and all their derived actions, so to speak, occur only once.

At any given time, Bohr had everyone working mainly on a single shared problem, which was of course the most important problem at that time. Landau had a great variety of problems in progress simultaneously.

Another feature resulted, as one pupil said, from a "striking phenomenon": Landau was able to (and did) interact with experimentalists; and so for every pupil he took the place of experiment, standing at the start and finish of each piece of work.

Enough has been written about the differences between theoreticians and experimentalists, their different forms and methods of work, and the interaction between them. We shall add to such general considerations something relevant to the heroes of this book. The process of research may be roughly represented as being like a relay race in which theoreticians and experimentalists alternately carry the baton. So the pupil's

words are to be taken as meaning that Landau was always the intermediate link in this process, handing the baton from the theoreticians to the experimentalists and back, and doing so, of course, not mechanically but in terms of ideas, providing before each stage of the research his thoughts, conjectures, and arguments. This peculiar ability of Landau's was also the source of his constant interest in experimental work.

Such was the "life of the school". As to form, one example is that the methods of teaching and training were, not without reason, called "puppy methods". Throw him into the water: if he swims, well and good; if he sinks, no matter. For those who surfaced, Landau would never stint his time. He was ready to discuss their work for as long as they liked, provided that it contained a sound idea, his pupils tell us.

The "puppy method" is entirely in line with Landau's style and principles. But it is hard to imagine that he had the support of the infinitely gentle Bohr. And as a last illustration, again of the differences between them, here is a story from the collection *The Physicists' Jokes Continue,* which fits in very well.

"When Niels Bohr gave a lecture at the Physics Institute of the U.S.S.R. Academy of Sciences, and was asked how he had succeeded in creating a first-class school of physicists, he replied: 'Probably because I was never afraid to admit to my pupils that I was a fool.' Evgeny Lifshitz, who was interpreting Bohr's speech, transmitted this to the audience as: 'Probably because I was never afraid to tell my pupils that they were fools.' Animation in the hall. Lifshitz asked Bohr to repeat his words, corrected his error, and apologized for his accidental slip of the tongue. But Kapitza, who was present, gravely commented that it was not an accidental slip of the tongue. In fact it represented the fundamental difference between the school of Bohr and that of Landau, Lifshitz being a member of the latter."

But we must not suppose that "being never afraid" was the privilege, prerogative, or monopoly of Landau alone. Far from it. Every pupil had the right, and neither the leader of the school nor anyone else was immune from such treatment; remember Pomeranchuk's "O Master, what a load of tripe."

Landau's pupils acquired much from their teacher in both skill and behaviour. Their behaviour might sometimes seem slightly strange – now extravagant, now unduly abrupt – and many people did not understand or accept it, many indeed condemned it unreservedly, but it contained no deliberate wish to offend or shock. It was a natural reaction; though they did not, be it said to their discredit, make any attempt to regulate it. A reaction in defence of correct, valid, serious, and profound physics.

As to "abilities" or peculiarities of thought and work, there are surely several that are to be regarded as fundamental. First, the conceptual appreciation of theoretical physics, regarding it as a single science, a harmoniously constructed edifice, an integral structure with interconnected elements. Landau rightly believed that, however gifted a physicist was, if he did not know theoretical physics in its whole compass and unity, the defects could not be made good, and would hamper him all his life. Second, the high quality, one might say, of the formal thinking, and the acute critical faculties. Third, the refined professionalism, a principal criterion in the assessment of work, applied both in the approach to solving a problem in physics and in the capacity to solve it: in both the creative and the executive aspects of the task.

The loss of their teacher had a great effect on the school. He was the centre about which was generated an extensive force field of scientists in active high-level work. In the same way, he was like a powerful charge that generated another kind of force field – one of intense thought and creativity which became the source of outstanding investigations.

He was also the surety for reliability and quality. Without him, the former confidence in reliable backing has doubtless been lost, and so therefore has boldness and self-confidence. (Perhaps, indeed, this process was more subtle and sometimes not even realized, so that putting it into words like this is a somewhat rough and over-simplified treatment.)

He was also a constant source of ideas – readily, unstintingly, he tossed them to his own pupils and to other physicists with

whom he talked. (He was, however, annoyed when they forgot who had suggested the idea.) With his passing, the school lost this too, for ever.

(Though there is no comparison, yet by association, or as an aside, or for mental relief, it may be added that Landau just as readily gave or lent money. For instance, when a mutual benefit fund was set up at the Institute, he contributed the whole of the initial capital. He was also glad to lend to those who asked, but he disliked both forgetfulness and insistence. He believed that it was for him to decide to whom to lend and to whom to give outright.)

Teacher and pupils

Some of Landau's characteristics were certainly passed on to his pupils, "inherited" by them. Quickness of speech and quickness of thought. Self-confidence, decisiveness, even a certain inflexibility in judgements and assessments of anything, whether persons, occurrences, or scientific work. Perhaps this was because pupils, like friends, are usually chosen on account of compatibility – not consciously or deliberately, of course, but "natural selection" does take place.

Yet these characteristics might have existed in them already, from childhood or youth, and simply become more firmly established during their lives. Most of the pupils began to work with Landau as very young men, and they at once showed remarkable gifts, as had their teacher, thus bearing to some extent the mark of child prodigies (though Landau could never allow precocity to displace serious everyday work), which is rarely combined with diffidence.

Though many of Landau's qualities might evoke opposition from strangers, they were greatly admired by his intimates, especially his pupils, who saw them as a "garnish" to Landau's more important features as a scientist and as a person.

If we analyse and approach it without prejudice, this very intolerance of Landau's was the result not of snobbishness or any nastiness of character, but simply of his inward inability to

recognize that thinking, approach, assessment, taste, that is, both psyche and psychology, need not be at all definite and identical for all persons. He was fond of saying with a laugh, "My taste is, by definition, the best in the world." A sort of joke. But not everyone thinks of making that kind of joke.

Perhaps this too is a characteristic of youth, to suppose that what you understand others must also understand, that what you like is good and what you dislike is bad. It is usually only at maturity that you begin to realize that there are reasons for not putting forward your own views, tastes, and so on, as having absolute value.

One must also not put forward as absolute this characteristic of Landau's, especially for topics other than physics. For instance, when asked by a film actor whether he had liked a particular film, Landau replied, "No, I didn't. But that doesn't matter, so long as someone else liked it." Probably, if the topic had been some work in physics, the script would have read quite differently. Physics was always the most sacred and important matter for Landau and his school. No excuses were accepted or allowances made; everything was reckoned in full: devotion to one's work, results, and quality.

Landau had two characteristics, one of which was variously termed purism or rigorism, but was essentially an ultra-chaste view of science, allowing no liberties or speculations, only what is fully rigorous, pure, and conclusive. The other was a peculiar democratic approach – not to people but to physics itself. Any problem, from any field, with any degree of importance, was worthy of attention. The only condition was that the work done on it must be up-to-date and at a high level.

There is no general agreement whether these are good qualities in a theoretical physicist (and there are many instances to prove that they are not essential). Some consider that Landau's rigorism, directed not only towards others but in the first place towards himself, had the result that he might sometimes reject an excessively bold and, as he thought, unsubstantiated, "pathological" idea, or if not rejecting it, simply refuse to give attention to it, even when it occurred to him.

His equally serious treatment of all problems in physics, the complete and fundamental absence of what may be called scientific snobbishness, with only one test: "interesting or not?"; a contempt for those whose intention was to solve only "the great problems of the universe"; did this attitude not result in a dissipation of his creative powers? For instance, one of his closest and most gifted pupils, Isaac Pomeranchuk, still more of a purist than Landau himself, as regards what to work on as well as how to work on it, deprecated what he saw as the "omnivorous" attitude of his teacher. He himself took at any time only a well-defined area of physics, whichever he regarded as the "hot spot", the topic of investigation that had the greatest fundamental significance at that time. "I've recently been working on elementary particles. The journals are full of interesting things, but I haven't time for all of them," he once said regretfully.

Pomeranchuk tried to put similar blinkers on his teacher, whom he treated with touching concern, but also with a kind of strictness. He was displeased when he thought Landau was not working along the "main lines". On the other hand, how delightedly and effusively he could repeat, going from one seminar participant to another, "The Master has achieved his greatest work yet." That was on the conservation of combined parity. It was in fact along the main line of physics at the time, and represented a considerable step towards solving the problem of symmetry in the universe.

However, Landau himself, as we know, regarded the theory of superfluidity as his greatest work. Incidentally, Pomeranchuk made a large contribution to this new science created by Landau's efforts – the quantum macrophysics of the condensed state. He discovered extremely important and broad extensions of it. The terms "Pomeranchuk principles" and "Pomeranchuk method" have become current in this area of physics. One can even say that by his own efforts Pomeranchuk, that fanatical purist, effectively certified that this *was* one of the main lines in physics.

At first, Pomeranchuk was just another child prodigy, but to

his credit he not only continued to work instead of resting on the endowment of nature and destiny, but laboured onward without thought of himself, until his life ended. He was one of our most talented physicists, and achieved a great deal in science. Landau's pupils say that he was the most gifted of them all. Landau had a great respect and affection for him.

Arkady Migdal writes that the research worker's strongest incentive must be "curiosity, desire to know how Nature is constructed. The scientific success of another then brings him just as much joy as his own. This was the attitude to science of our excellent theoretical physicist Isaac Pomeranchuk, who even on his death-bed, regaining consciousness, asked about the latest discoveries in the theory of elementary particles and rejoiced at each new concept." To be dying, to know that one's end is near, usually brings complete renunciation of terrestrial things. Great must have been his love of physics, it must have been his life, if even his last moments of consciousness were devoted to it. But those who knew Pomeranchuk well were not so surprised. They say that he was a fanatic for science; a man of one pure tone; a man who achieved much and saw clearly the problems that had to be solved. This unanimity of opinion is, of course, not by chance; Academician Isaac Pomeranchuk – or Chuk, as he was called in the school of Landau – was indeed a great scientific figure.

The name Chuk was, however, rarely used to his face, unlike for example the name Dau, which was a frequent mode of address to his teacher by a large number of physicists and even some non-physicists. We know that Landau liked the name, and explained with gusto that his surname in French read "L'âne Dau" – Dau the Ass. He was once told that according to Brehm's *Thierleben* "dau" is the name of a particular species of wild horse, Burchell's zebra. Landau appreciated the "kinship" to this species of ungulates also, especially because the horse dislikes captivity and because the rumours that it produces inferior stock in captivity are false.

We might add some further such jokes and remarks by Landau. He liked any kind of "zoo-analogy". That gentle animal, the ass,

was not by any means assigned only to himself. "Yes, that is typical of the animal kingdom," he would say, meaning, "What an ass you are." Another favourite expression was, "If you don't work, you'll grow a tail." This was an inversion of Engels' idea that monkeys became human beings by working; Landau ironically foretold that idlers would evolve backwards into monkeys. Landau indeed was active against idlers, and was firmly convinced that one ought to work hard, especially when young. If the talk was, say, of the time devoted to work, the number of hours spent, then Landau would subtract any period spent in sitting and looking out of the window, which he claimed did not count.

Landau's attitude to physics was irreproachable throughout his creative life. However he behaved, however he appeared to those around him, one must always remember, above all, his attitude to physics, for this was the root of many of his actions.

Of course, like anyone else, he made errors of judgement. This has to be admitted, in the cause of truthfulness, and to make the portrait of him more authentic. His errors were usually in the same direction. It has already been mentioned, for example, that a single unsuccessful appearance at the seminar, or a poor and unconvincing account of one's work (which might itself be quite good), immediately caused him to react negatively, and the negative opinion was generally unalterable. (Though, here again, one cannot be absolute; there were exceptions.)

This was not the only way to "earn" a black mark from Landau. Just as he was closely concerned with, and deeply interested in, the whole of physics, striving at all times and places for quality and purity in physics, seeking to instruct both those at the foot and those at the top of the "ladder of learning", so his assessments and his sarcasm and his attacks were applied at various levels: to the "great ones" and the "little ones", to dilettanti and to quasi-professionals with pretensions to scientific titles and ranks, and even to acknowledged (sometimes deservedly, sometimes not), venerable, and distinguished physicists. The report by Kapitza and Fock mentioned above contained

the significant words: "Landau often speaks at scientific meetings and is extremely forthright in his criticism of the work under discussion. His inability to take account of the individuality and pride of the person criticized gives rise to displeasure."

Here are a few cases from his plentiful experiences of this kind. "I never met such an idiot! An utter idiot!" he yelled, rushing out of an important reception held in honour of one who was indeed outstanding – as a scientist. That man was, we might say, a little more self-assured and conceited than he should have been, and probably did not bother to conceal it. Hence Landau's reaction.

There was a Western physicist that he clearly underestimated. "Oh, *we* know what X can manage to do," Landau once remarked sceptically. In fact, X quickly showed that he could do a great deal. His style as a physicist was simply different from Landau's; in particular, he was less strong in technique, in the mastery of mathematical methods.

This was Landau's reply to some young beginners who evidently had too high an opinion of themselves: "You ask in what to engage, in the sense of which branches of theoretical physics are the most important. I must say that I regard such a statement of the question as absurd. One must have a rather ridiculous immodesty to regard only 'the most important' problems of science as worthy of one's interest. In my opinion, any physicist should engage in what interests him most, and not embark on his scientific work from considerations of vanity."

"He was deeply democratic in his life as a scientist (and in his life as a human being, for that matter); pomposity and deference to titles always remained foreign to him," wrote Evgeny Lifshitz. "Anyone, regardless of his scientific merits and title, could ask Landau for counsel and criticism (which were invariably precise and clear), on one condition only: the question must be businesslike instead of pertaining to what he detested most in science: empty philosophizing, vapidity and futility cloaked in pseudo-scientific sophistries. He had an acutely critical mind; this quality, along with his approach from the standpoint of profound physics, made discussion with him extremely attractive and useful.

"In discussion he used to be ardent and incisive but not rude; witty and ironic but not caustic. The nameplate which he hung on the door of his office at the Ukrainian Physicotechnical Institute bore the inscription:

L. LANDAU
BEWARE, HE BITES!

"With years his character and manner mellowed somewhat, but his enthusiasm for science and his uncompromising attitude towards science remained unchanged. And certainly his sharp exterior concealed a scientifically impartial attitude, a great heart and great kindness. However harsh and unsparing he may have been in his critical comments, he was just as intense in his desire to contribute with his advice to another man's success, and his approval, when he gave it, was just as ardent.

"These traits of Landau's personality as a scientist and of his talent actually elevated him to the position of a supreme scientific judge, as it were, over his pupils and colleagues." ("Dau said" was a constantly heard expression.)

"I liked him. We had common interests, and often chatted together. But I never discussed physics with him," said one physicist – not one of his pupils, but a very good physicist, whom several great and famous and honoured scientists regard as their teacher. But "I did not take the risk of discussing physics with him."

Another physicist – one of Landau's pupils, though originally by correspondence: "I liked him even before I met him. It was a tradition in the school of Landau."

Sometimes liking is "for no particular reason", or even "despite oneself". But, hardly so, if liking has become a tradition, for it cannot have been imposed as an obligation.

Why was Landau liked by his pupils? For his fascinating talent? Undoubtedly. And for his purely human charm, directness, integrity, intellect, his "specialness"? Of course these were involved also.

And for his attitude to physics, the atmosphere he created, one of reverence and service (to use elevated terms) towards

physics, which was also an atmosphere of thought, action, naturalness, wit, disdain for bombast and high-flown words; an atmosphere of hard work and easy breathing – all this was bound to evoke liking and acknowledgement.

But perhaps there was another reason also. Belonging to the school of Landau did not foreshadow an easy life. Quite the opposite: it demanded hard and constant work. This was a necessary condition, however, and by no means a sufficient one. In order to become a pupil of Landau's, one had also to be gifted. In his school, one had to know a great deal and be able to do a great deal. And it was because of this arduous fortune, because every pupil *could* – could do good work, could somehow overcome complexities and hindrances, inward and outward, could prove himself up to it, at home in big physics, could show that he was worth something and deserved to belong to the school, to this body of the elect (a body not chosen according to outward formal signs, or questionnaire answers); it was because of this that they liked Landau. Three words sealed it: liking, tradition, school. Belonging to the school of Landau helped one to acquire self-respect, to establish oneself, which everyone needs to do, and scientists probably more than others.

Landau's friends and acquaintances outside physics, that is, those whose relationships with him lay outside his work, and outside science, were not in a position to assess him fully, and so they could not derive the same pleasure from meeting him as fell to physicists and especially to his pupils.

There is usually an analogy to be found; one may even suggest itself. The more persistently it does so, and the more stubbornly it remains in one's mind, the more it seems trustworthy.

The Beethoven Quartet played Shostakovich's fifteenth quartet, one of his last compositions, written shortly before his death. Before the piece, Dmitry Tsyganov quietly and tremulously spoke a few words concerning Shostakovich, and asked that his memory be honoured. The speaker could not regain calmness; he pulled out a handkerchief, wiped away some tears, blew his nose.

The quartet was indeed stupendous, with some devilish

satanic power and infinite unendurable grief. Each movement is marked "adagio", and they are played without a break, such an unending melody of tragic parting, farewell to the world, leaving it for ever.

There was the utmost tension in the concert-hall. A woman broke down, left her seat, and almost ran to the exit. Tsyganov, however, was entirely absorbed when he was playing, though during the rests he again sighed and wiped his eyes, as could be clearly observed from the nearest seats.

Suddenly, everything became more understandable. Of course, there simply was a great personal friendship between Shostakovich and Tsyganov; and the death of a friend is always a severe loss. But there was more to it than this. Who was Shostakovich? A great composer, a genius, as anyone will tell you. A person's contemporaries rarely award such a description. Sometimes a certain lapse of time is needed in order to appreciate fully the creative work of a scholar or an artist; and sometimes it simply is hard to accustom oneself to the thought that one can, or even ought to, apply such a term to this person who lives and works alongside one, whose weaknesses, mistakes, and shortcomings one knows and perceives. Moreover, if we survey the many centuries of human history, it becomes obvious that the "density of geniuses" is exceedingly low. Then suddenly, one or two or three turn up alongside you, at the same period, in the same city. . . . This does not agree with our everyday experience, since we find it normal and customary to accompany the word "genius" with verbs in the past tense.

Yet a true genius was yesterday still among us and creating his remarkable works of art. He has only just departed, and now the pain of the personal loss of a friend is inseparable from the awareness of everyone's great loss. The quartet that is played seems like a final memory, a testament, a requiem.

Tsyganov came to know the fifteenth quartet; perhaps by reading the score, perhaps by hearing Shostakovich play it on the piano, and then rehearse it with the performers. At any rate, they were present at the birth of truly great creative art. They became more fully aware of it, more accustomed to it, saw

deeper into it; perceived it with ever greater refinement and intimacy. An "ordinary" genius was among them and working with them, involving them in his creative work, making them his fellow authors and participants. That was what happened.

Let us now envisage, for example, Landau's theoretical seminar; we have endeavoured to open a little way the door of that room (although, as we know, entry was unrestricted). And just as the quartet, or the symphony orchestra, first heard Shostakovich's new works when the composer performed them as one of their number, and did not merely hear, but were present when these works were being finished and polished, and above all when the composer was inspired before their very eyes: in exactly the same way, theoretical physicists were present at the mystery of inspiration that so frequently visited Landau, and saw the lustre and power of his thought, and experienced with him the surmounting of difficulties, the achievement of progress. (Though, to be honest, one cannot conceive of a more unsuitable word than "mystery" to describe the gatherings of this rowdy wisecracking bellowing assembly.) The joy of being present, the pleasure of understanding, were the further reasons why Landau earned the love and gratitude of his pupils.

Of course, there is a point at which the analogy with music becomes an antithesis. But this relates to "publication", not to creation. Anyone can come to the conservatoire and get to know great music. This is evident from the crowded concert-halls. But he can evaluate it and understand it only to the extent of his own capabilities, preparedness, and knowledge, whereas all can hear it and delight in it. It is not so in science, especially in a science like theoretical physics.

Landau appeared occasionally before a large audience of non-physicists. His lectures were invariably attractive and interesting. We shall see something of one such lecture. But it would be absurd to suppose that such lectures were in the least similar (as regards their content, of course) to those he gave for students, or to his expositions at the seminars and scientific committees and other scientific meetings. There was nothing in

common with music or the theatre, where appearing before the public is often a necessary condition and a stimulus to show the fullest extent of one's gifts.

For physicists, Landau's scope and profundity were most fully shown to a restricted circle, and often only in private conversation. They, if anyone, could form the most precise estimate of him. Those who did not observe and comprehend him when he was "the servant of the Muses" did not know the real Landau, however wise and shrewd they themselves might be.

There is one curious detail which seems to contradict this. In answer to the banal typically journalistic questions (which are sometimes indeed awkward to put in earnest) "What did he mean to you?", "What was the most important thing you gained from him?", and so on, to which the answers seem to be predetermined and known in advance, with only individual variations of wording, there comes suddenly an unexpected reply: "Dau played, in my life, a fundamental part in overcoming many complexes. He was intelligent, but he did not consider that complexes should be pampered. On the contrary, he believed that they should be examined and disposed of. A person ought to be happy. Although Dau and faith are incompatible terms, yet his creed was that man has the duty to be happy."

Again, "The chief thing that I gained from Dau was his teaching that in life, both public and private, one has to apply the same methods as in theoretical physics. I learned this and I am proud to apply the scientific method to all things in life. Landau had, as he would say, a scientific approach to everything. He was tremendously capable of this. He was asked to analyse this or that situation in people's lives."

One's first reaction to such revelations is a quite natural one: his pupils are not talking about the chief thing. Then you think, "Gosh, but it was a special, a unique gift, to teach oneself and other people to be happy." And though at first it seems strange and even somewhat flippant to discuss this as a serious topic, there is perhaps sometimes benefit to be had by thinking without prejudice about such matters. It may be that our general

position is simply the result of ancient, age-old delusions. Everyone usually makes the tacit assumption that every field of human activity demands labour, effort, and force. All except one, known as private life or human relations, in which everyone drifts along, secretly confident that it will all come right in the end. Yet this is probably the most complicated sphere of life, and rarely, alas, is anything in it capable of coming right by itself. There is a justified association with the second law of thermodynamics: "leaving it to itself" can only increase the entropy.

"A teacher of life" – such words are customarily taken humorously, and in particular this "mission" of Landau's is greeted with a smile. But we find that not only did he himself view it with great seriousness and genuine pride, but others too saw it as entirely serious and significant.

What he taught in the sphere of private life and human relations also had its specific features. Here too, scientific analysis prevailed, and frankness was not confessional and lyrical but rather informatory, giving the totality of facts needed to arrive at a rational solution.

Landau himself, despite his outward openness and directness, could be reserved and withdrawn in some important matters. There were doubtless some events or aspects of life which affected him in a special way and which he discussed *least* candidly with his pupils and his physicist friends, while to others, who either had no connection with science or were physicists not bound to him by permanent and firm "bonds of science", he might suddenly reveal thoughts and self-appraisals that told a great deal, experience sometimes very profound and (strange as it may seem) related in fact to his work, to physics, and to his place in physics.

What Landau said on such matters to others than his pupils has become fully incorporated into the traditions of his school. Not prestige and not self-esteem, but the position of chief of the school, of Master, and still more the style of relationship, mocking, ironical, without sentiment and effusiveness, was what tabooed many subjects and topics of conversation. "This is an

item in your biography" was one of Landau's favourite maxims, meaning that it should not be made public. His pupils eagerly took up this style. He could not afterwards discuss with them an "item in his biography". In his school he was legislator, and teacher of the Law, and he would not depart from the established rules of the game.

Kapitza said that only a great man can be a great teacher. Equally certainly, only a great scholar can be a great teacher: it needs both wide learning (which we know Landau had) and independence of thought (which mental quality too he possessed in full measure). If the condition stated by Kapitza is necessary in order to become the founder of a school, the other qualities are equally necessary for the shaping of an eminent scholar.

The Theory of the Superfluidity of Liquid Helium

Low temperatures, absolute zero, and quantum mechanics

THE WAY of knowledge is never short and easy for anybody. The pioneer scholar blazes a trail into unknown territory. The child takes well-travelled paths, but for him they are always new. Many roads there are, some lasting for centuries or decades, others coming into existence before our eyes, others again still to be devised. All are different, different for everyone. Only one thing do they have in common: they cannot be rushed along in an instant, without care or consideration.

Sometimes a great scholar will invite a person to join him in the search for truth, to see how a new path is marked out. Johann Kepler wrote: "For it is not only a question of how to acquaint the reader most readily with the essence of what is to be expounded. It is important above all to tell for what reason, by what clever device or roundabout way or lucky accident I, the author, was able to achieve what I have achieved. When Columbus, Magellan and the Portuguese navigators, who respectively discovered America, the Pacific Ocean, and the sea route round Africa, tell us how they lost their way, not only do we not censure them, but on the contrary we are afraid of missing anything they have to relate, so enjoyable is the reading of their narratives. And so I shall not be blamed if, out of regard for the reader, I proceed similarly in my own writing. When we read of the adventures of the Argonauts, we do not have to carry the burdens which they bore, whereas the obstacles and perplexities along the path of my own thought are fully perceptible to the reader. Such is the fate of any mathematical treatise. [May we interrupt Kepler to say: and of any work from numerous fields of modern physics, both theoretical and experimental – A.L.]

Different persons take pleasure in different things, and there are some whose greatest joy is to surmount all difficulties of understanding and embrace the sequence of my discoveries at a single glance."

Einstein was briefer and simpler when he first attempted to unfold and depict the structure of the Universe according to his general theory of relativity: "I shall conduct the reader over the road that I have myself travelled, rather a rough and winding road, because otherwise I cannot hope that he will take much interest in the result at the end of the journey."

These are splendid words. And the wish to take the reader with one along the laborious road, like a partner in the difficult search, is a splendid thing. Such also should be the aim of a book whose hero is a scholar. It is not easily achieved. It depends entirely on the reader's active sympathy.

The life and work of a scholar are of most interest to the man who is himself interested in knowledge, who is willing to spend time and effort in order to penetrate even a little way into the secrets of the world, to acquire new knowledge of the nature of things, to journey into those corners of the Universe that are not served by coach trips. But, although the coaches do not run there, these "preserves" are becoming accessible to more and more people, especially the young.

This is possible because there is a continuing process of collective increase of human wisdom, an ever-growing familiarity with new ideas and the phenomena of nature. Large numbers of people can now reach and become familiar with what not long ago seemed to the greatest intellects improbable and incomprehensible. This is the only explanation of many remarkable facts in the history of science.

In 1913, thirteen years after making his own great discovery, Max Planck, the father of the quantum theory, wrote to support the election of Einstein to the Prussian Academy of Sciences in Berlin. As well as highly praising him, he said: "The fact that in his discussions he sometimes went astray, as for example in his hypothesis concerning radiation quanta, should not be held against him, since nothing really new can be achieved, even in the exact sciences, without a venture."

Planck was in fact referring to one of Einstein's greatest works, in which he asserted the quantum nature of radiation. Some physicists (though admittedly a minority) rank this discovery even above the theory of relativity, regarding it as the true foundation of quantum mechanics.

Einstein in turn, though seemingly the supreme scientific revolutionary, all his life refused to accept what is perhaps the foremost idea of quantum physics: the probabilistic interpretation of processes in the microworld or, as he saw it, the violation of the law of causality: "I cannot believe that God plays dice with the Universe" – a half-ironical comment which he often made, expressing the scholar's deep perturbation at the collapse, as he believed, of the most unshakeable foundations of physics. In defence of these foundations, Einstein for many years continued in debate with Bohr, devising ever more objections and paradoxes, which he put forward to refute various first principles of quantum mechanics.

Bohr was in Moscow, shortly before he died. In discussion with scientists at the Institute of Physical Problems, he recalled this debate. "Today, when Einstein is no longer with us, I should like to say how much was done for quantum mechanics by this man with his perpetual and indomitable striving for perfection, for architectural harmony, for a classical completeness of theories, for a unified system, on which the whole picture of physics could be built up. At every new step forward in physics, seemingly a definite consequence of the previous one, he detected contradictions, and these became the impetus that moved physics forward. At every new stage, Einstein threw down a challenge to science, and without these challenges the evolution of quantum physics would have been much slower."

So science proceeds, both in its creation and in the understanding of it.

Now, in thinking of the members of the school of Landau, let us recall how one of them said that it is always easier for the pupils' pupils than for the first generation, both to grasp new ideas and to penetrate the complexities of the *Course of Theoretical Physics*.

The same thing is true, at various levels of difficulty of course, for the great majority of people. It is easier for them than for their predecessors to understand science, and it holds greater interest for them. Just as science comes to occupy more and more territory in the life of humanity (and perhaps just because of this), so it attracts the interest of more and more people.

It is, of course, possible to spare the reader unnecessary mental effort by assuring him that the author will not complain if some of the science gets left out. But it is better to remember and repeat Mayakovsky's words: "I am a poet. That is what makes me interesting." Asked how he would like to see Landau depicted in a book about him, one pupil replied: "As he was in seminars. Dau was at his most brilliant and fascinating when he was being a physicist."

To appreciate Mayakovsky as a poet, however, one need only read his verse. It is unnecessary, indeed pointless, to look at Derzhavin's writings first, or even those of Blok who is close to our own time.

It is not so with scholars and their discoveries. Here the previous history is essential. Otherwise, it is hard to understand the discovery or assess its significance and place in the edifice of science. In informing the reader of what preceded Landau's discovery, we shall often have to part company with the subject of this book.

Liquid helium was, in fact, first prepared in 1908, the year of Landau's birth; this is the starting-point in the story of discovery that we have chosen to relate. And a baby, however brilliant his future, has no place in the history of science.

From the beginning, before there was any thought of liquid helium or of low temperatures in the modern sense, helium seemed to scientists an unusual element. Landau used to say that, of all the chemical elements, helium is the most surprising, and this in many ways. Incidentally, the surprising nature of its properties is to some extent responsible for its surprising history. With this in mind, Landau strongly advised reading a book by a friend of his youth in Leningrad. In the preface to that book he

wrote: "This book, *Solar Matter,* the work of the talented and prematurely deceased physicist Matvey Bronstein, is something exceptional in the world's popular literature. It is so simply and attractively written that it will perhaps be equally interesting for every reader, from the schoolchild to the professional physicist. Once begun, it is hard to stop reading before the end is reached."

The reader, if he takes up the book, will find that he is already acquainted with the three physicists who contributed to it.

Its last chapter, "Twenty years later", was written by Alexander Shalnikov. It is to some extent a summary of what we have now to relate. Shalnikov has graphically described the behaviour of liquefied helium, and has done so with great precision, as one would expect, for he has been concerned with low temperatures for many years, and is closely familiar with liquid helium. The reader who wishes to know about helium gas is therefore recommended to study *Solar Matter.*

In the history of science, there are many instances of some place's becoming a kind of universally recognized headquarters for a whole field of science. Copenhagen, for example, was for several decades the generally acknowledged centre of theoretical physics, which every self-respecting theoretician felt obliged to visit. Similarly, Leiden, a small town in Holland near the mouth of the Rhine, became a great centre of low-temperature research. Among many others, the two Kharkov friends of Landau's, Olga Trapeznikova and her husband Lev Shubnikov, came here to work in the "Mecca of cold".

The most notable figure there was Heike Kamerlingh Onnes, who was for forty years head of the department of experimental physics at Leiden University. His name is associated with the steady descent to lower and lower temperatures and with a series of outstanding discoveries in that field.

The first of these was probably not really a discovery. It was rather the achievement by hard work of the goal of changing helium from a gas into a liquid. Liquid air, and its components liquid nitrogen and oxygen, had long since been obtained, and hydrogen too had been liquefied. Helium alone did not submit.

A theoretical explanation for this behaviour was found. Helium is well named a noble or inert gas. Its atoms are very symmetrical and attract one another only very weakly. Even in comparison with the other inert gases, helium takes an extreme position, because its atom has the most stable electron envelope, the innermost shell, fully occupied by two electrons. This is why helium atoms feel no inclination to approach anything, even their own kind, and indeed mightily resist doing so.

At last, after much labour and skill, helium was brought down to a temperature of -269 degrees, and Kamerlingh Onnes succeeded in liquefying it. This was in 1908. So physics entered the realm of the lowest possible temperatures.

Minus 269 degrees Celsius is about four degrees on the absolute or Kelvin scale of temperature. To be precise, helium liquefies at 4.2°K, that is, at less than five degrees above absolute zero.

The subject of temperature and the methods of measuring it – and therefore of temperature scales – is far from simple and has a profound significance in physics. One physicist has said that a complete answer to the question "What is temperature?" would fill a book and would be a good illustration of changing opinions and the progress of physics in the last four centuries. This is because, as our knowledge increases, what seemed to be simple facts acquire new meaning.

There are various definitions of temperature. It is said to be a measure of the mean kinetic energy of the molecules in a body, or a measure of the heat-content of a body, or an indicator of which bodies will gain heat from a given body, and which will lose heat to it.

A measure, an indicator . . . of heat, heat-content, energy, motion. Whereas the words in the second group have a definite meaning, those in the first group suggest some degree of arbitrariness or convention. Physicists constantly emphasize this. In a book entitled *Physics for the Inquiring Mind,* for example, we read: "Thermometers are useful and trustworthy as minor servants. Is there behind their useful servile scales a master temperature, a ruling scientific aristocrat? . . . Yet temperature

itself remains a man-made concept, with an infinite choice of types of temperature scale still open to us. Not all things we measure and use in science seem artificially defined like this."

This is similar to the view of Richard Feynman, one of the greatest of present-day theoreticians: "The mean molecular kinetic energy is a property only of the 'temperature'. Being a property of the 'temperature', *and not of the gas,* we can use it as a *definition* of the temperature. The mean kinetic energy of a molecule is thus some function of the temperature. But who is to tell us what scale to use for the temperature? We may arbitrarily *define* the scale of temperature so that the mean energy is linearly proportional to the temperature. The best way to do it would be to call the mean energy itself 'the temperature'. That would be the simplest possible function. Unfortunately, the scale of temperature has been chosen differently, so instead of calling it temperature directly we use a constant conversion factor between the energy of a molecule and a degree of absolute temperature called a degree Kelvin."

Physicists say that the terms "helium" and "low temperatures" have for them become almost synonyms. On the one hand, helium itself is a means of obtaining extreme cold. On the other hand, and most importantly, all the remarkable properties resulting from the behaviour of liquid helium come into play near absolute zero.

Absolute zero is one of the fundamental limits of physics, and involves a most unusual change in the behaviour of matter. And on the way to this limit, in the concomitant overcoming of the "arbitrariness of temperature" that has disturbed us all, there is likewise much that is interesting and relevant to the subject of our tale. It is therefore worth while to follow the path, if only rapidly.

When various means of measuring temperature were being sought, that is, when thermometers were being filled with various substances, reference points chosen, and scales variously calibrated (and the whole process occupied a great time), far-seeing physicists perceived more and more clearly the need to

find a scale that should as far as possible be independent of the properties of any particular substance, and of arbitrary choice by the measurer.

The problem was soluble in principle. The ideal agent for the thermometer was the ideal gas. In the mid nineteenth century, this notion came into the head of Kelvin, who constructed a gas thermometer with an ideal gas as the working substance.

The word "gas" comes from the Greek word "chaos" or disorder. The greater the disorder, the more nearly ideal is the gas.

The mechanism of this is obvious. In solids, such as crystals, the thermal motion of the molecules is highly ordered. They can only vibrate about the equilibrium position, the "site" which they occupy in the crystal lattice. Liquids show more freedom of movement, but the interaction forces are still very great. The molecules live very close together, with the same neighbours. In gases, the interaction forces that prevent chaos are much weaker, and in the ideal limit it is clear that, as the gas becomes more rarefied, the interaction between its molecules becomes less and less important. Each molecule lives practically in isolation, encountering only the walls of the containing vessel. The gas may then be called "ideal".

In fact, over a very wide range of temperatures, the molecular interaction is so small that it is neglected. ("Neglect" is a very usual word among physicists; once, in a sculptor's studio, Isaac Pomeranchuk remarked that both sculptors and physicists know what may be neglected.) The range of existence of a gas in which it may be regarded as ideal is therefore quite wide.

Kelvin, who proposed to measure temperature with an ideal gas, himself constructed the first gas thermometer. The working medium was ordinary air at constant pressure. When the air was heated or cooled, the change in its volume served as a measure of the temperature.

But Kelvin's achievement was not only to propose a very accurate and essentially non-arbitrary means of measuring temperature. His work had important theoretical consequences also.

If a real gas, such as air or hydrogen or helium, is steadily cooled, it behaves at first – and for quite a long time – as an

ideal gas, but eventually the interaction between its molecules comes into play. This interaction becomes steadily stronger as the temperature drops, until, at a certain value of the temperature for any particular substance, it forces the gas to become a liquid. There is then, of course, a great decrease of volume in comparison with that occupied by the gas in its "ideal" guise.

An imaginary gas which continued to behave in an ideal manner would contract to a point at the lowest possible temperature. Since a volume by its very nature cannot be negative, no further lowering of the temperature could occur; it would simply have no physical sense. For this reason, the low-temperature limit is called "absolute zero". The scale reckoned from absolute zero is called the Kelvin scale, or absolute scale, of temperature.

We can, however, imagine another way of descending to absolute zero. Let an ideal gas be cooled at constant volume. Its pressure will, of course, then decrease, to zero in the limit. It is not very difficult to guess that the pressure will be zero when the temperature reaches absolute zero.

What does zero pressure signify? The pressure of a gas is the total result of the impacts of its molecules against the walls of the vessel (and of course against the instrument that measures the pressure). As Landau once said, the continuous hail of molecular blows merges into the single force of pressure. This force is related to the speed of the molecules, and accordingly to their kinetic energy. The mean kinetic energy of the particles is simply proportional to the absolute temperature of the gas. "The mean molecular kinetic energy", to repeat Feynman's words, "is a property only of the temperature, not of the gas."

Returning along this chain of interrelated physical processes, we find that at absolute zero the kinetic energy of the particles is also zero, and therefore so is their speed of motion, and therefore (as we found earlier by a different route) so is their pressure. Thus zero pressure signifies the cessation of the "bombardment"; that is, the molecules come to rest, the random thermal motion of the particles ceases, the "chaos" that gave rise to the name of "gas" comes to an end.

This is what makes absolute zero exceptional and gives it a deep physical meaning. At absolute zero, the movement of the particles ceases. The particles — atoms or molecules — slow down, come to rest, "freeze".

Yet it turns out that the cessation of motion at absolute zero is not the whole truth. Quantum mechanics shuffles the cards here. But for us, as yet, it does not exist, and classical physics reigns supreme. At absolute zero everything sets hard, both living and lifeless nature are dead.

Absolute zero, being a limit, is unattainable. It can only be approached more and more closely. This follows from the theory. In practice too, the physicists constantly find that the closer the approach, the more difficult progress becomes. Each successive step, each fraction of a degree, is achieved with more labour. One physicist has given the following half-joking, half-serious explanation of the unattainability of absolute zero: "To cool a substance from 100 to 10°K (that is, from about the temperature of liquid air to that of liquid hydrogen) needs a great deal of effort and money. The same amount is needed to cool it further from 10 to 1°K, from 1 to 0.1°K, or from 0.1 to 0.01°K, so that absolute zero seems to be practically unattainable because of rising costs."

The unattainability of absolute zero becomes particularly striking if a logarithmic scale is used instead of any of the ordinary temperature scales (Celsius, Kelvin, Fahrenheit), where every division is one degree.

On the logarithmic scale, one division corresponds to a change (increase or decrease) by a factor of ten. If 1°K corresponds to zero on the log scale (since $\log_{10} 1=0$), one scale division will correspond to 10°K, two to 100°, three to 1000°, and so on. This is the upward scale. It extends downwards in a similar way: minus one means 0.1°K, minus two 0.01°K, minus three 0.001°K, and so on. Since the logarithm of zero is minus infinity ($\log_{10} 0=-\infty$), we find that absolute zero is far, far away; as regards attainability it has gone to infinity.

Physicists are fond of using logarithmic scales. As we have

seen, Landau used one in assessing the contributions to science made by various contemporary theoretical physicists. That was really only a joke. But log scales can be used, for example, to bring large magnitudes into relation with others that are tens, hundreds, or thousands of times smaller. In the case we are discussing, on the other hand, the log scale serves to remove absolute zero to infinity and make it unattainable – as indeed it is.

At Leiden, Kamerlingh Onnes continued his long and laborious descent to absolute zero – not in theory, but in practical experiments. In 1908, as we have seen, he was able to bring to a successful conclusion many years of attempts to liquefy the last gas, which had resisted so long and stubbornly. Helium, cooled to 4.2°K, became a liquid; boiling, seething, in a word furious, but a liquid.

The plan now was finally to tame the rebel – to force it to solidify. Kamerlingh Onnes moved on towards this goal, distant but (as he believed) attainable; for all substances eventually solidified into crystals when cooled to the appropriate temperature.

For helium, however, no "appropriate temperature" was found. Through almost twenty years until his death, Kamerlingh Onnes struggled without success. As his pupil W. H. Keesom recalled, "At the lowest temperature, 0.83°K, reached by Kamerlingh Onnes, helium . . . was still liquid. His aim was to cool liquid helium further to absolute zero and determine its state at that temperature, but he did not live to complete the investigations he had planned."

We know now that his failure was inevitable, and its cause lay in the physical nature of helium, which is such that helium would never have solidified, however close he had come to absolute zero.

At the time, however, Keesom, probably not seeking to explain the physical principle of the observations, but simply using the results of his teacher's experiments, decided to proceed differently. Having in mind the unsuccessful results of cooling

helium, he began subjecting the liquid to increasing pressure. Eventually, at pressures of the order of 25 atmospheres, it was possible to compel helium to crystallize. At normal pressure, however, solid helium simply does not exist. This in itself showed that the substance is out of the ordinary. It was a signal which invited attention. But though it was noted, it was not understood at that time.

Naturally, Kamerlingh Onnes could not understand or explain the enigmatic behaviour of helium, and doggedly attempted to cool it further and further. He was unsuccessful, because the reasons lay in the purely quantum properties of this peculiar liquid, in its "quantum" behaviour. But how was he to know this? At the beginning of the century, even the word "quantum" had an unfamiliar ring, and by 1926, when the Dutch physicist's life reached its close, quantum mechanics had passed only the first stage of its coming into being, the establishment of first principles, and was not yet capable of attacking all the paradoxes of physics. At that time, the classical laws were applied to helium. But the experimental results were in conflict with these laws.

Landau was always interested in the various physical transformations of matter, which are usually called phase transitions. With the use of phase diagrams to describe the behaviour of matter, he explained in the following way the unavoidability of crystallization at low temperatures: "The curve of equilibrium between the solid and the gas goes to the origin (with temperature and pressure on the coordinate axes); that is, at absolute zero the substance is solid at all pressures. This is a necessary consequence of the ordinary concept of temperature, based on classical mechanics, according to which the kinetic energy of the atoms becomes zero at absolute zero, and all the atoms are at rest. The equilibrium state of the body is then one in which the configuration of atoms corresponds to the minimum energy of interaction between them. Such a configuration, differing in properties from all others, must be ordered in some way, i.e. be a space lattice. This means that at absolute zero the substance must be a crystal."

The preceding pages have already prepared us for the necessity of parting from classical physics and turning to its successor, quantum mechanics. Actually "successor" is not quite the right term. It is rather a matter of delimiting the spheres of influence. In going to a new sphere, we must not forget that classical physics, like all the classics, is worthy of the deepest respect, even if it is defeated by some extreme situations.

It has long been a commonplace that quantum mechanics is the physics of the microworld. Yet all bodies, all substances, everything in the world, consists of elementary particles. Why then do we not observe and perceive, in everyday life and in physics experiments at school, the quantum properties of these particles? The reason is that the behaviour of matter and bodies as a collection of microparticles conceals and encloses the behaviour and properties of the individual particles.

Kapitza used to say that trying to detect the quantum nature of processes by studying matter at room temperature was like trying to investigate the laws of collision of billiard-balls on a table in a ship on a rough sea. Someone has called the thermal motion of particles, which hides their quantum nature, noise in the pure symphony of quantum mechanics, which makes it impossible to hear the music.

But let matter be cooled to low temperatures. The "noise" dies away, the motion is slowed. Then the quantum nature of processes begins to be discovered and revealed. So, in the words of Landau, "As the temperature falls, the energy of the particles decreases, the conditions for classical mechanics to be valid are eventually violated, and classical mechanics has to be replaced by quantum mechanics." He further explained the profound physical essence of this phenomenon: "At absolute zero, thermal motion ceases. This statement, however, does not mean that all motion of particles within the body ceases. According to quantum mechanics, it never ceases completely. Even at absolute zero, some vibrational motion of atoms within molecules or about the crystal lattice sites in a solid must continue. These zero-point vibrations are a quantum effect. The energy of such motion is characteristic of the 'quantumness' of a particular

object. Comparison of the energy of thermal motion of the particles with that of their zero-point motion may serve as a test of the applicability of classical mechanics: this is valid for describing the thermal motion of particles if the energy is sufficiently large in comparison with the zero-point energy. The clearest example of the zero-point motion, which continues to exist even at absolute zero, is the motion of the lightest particles, electrons, in atoms."

At low temperatures, various initially inexplicable features of the behaviour of matter began to appear. Long ago, it was noted that the specific heat behaves strangely. But this was found to be not strange when the behaviour is seen from the quantum viewpoint. Some metals and alloys showed superconductivity, their electrical resistance falling to zero, so that a current could continue to flow in them for an indefinite time. This too, it was subsequently found, was a purely quantum effect.

Helium the joker

All this seemed to have not the slightest relevance to helium, which still remained a liquid, and so, logically, had not yet exhausted its "classical possibilities".

In reality, the opposite was true. All other substances solidify before their quantum properties become at all obvious. Helium alone failed to solidify, just because, while still a liquid, it managed to become a "quantum liquid" (which is not a tautology; the words contain a deep physical meaning). A quantum liquid obeys its own quantum laws, and therefore need not solidify even at absolute zero.

A "quantum liquid" – is there not something unnatural in the sound of that phrase? In fact, quantum mechanics, by definition, governs the microworld. Its laws are obeyed by microparticles and certain combinations of them, or rather by their behaviour in microprocesses occurring in them and between them. A quantum liquid is of course a macro-object, visible to the naked eye, so to speak.

Thus, surprisingly, liquid helium in its macro-form, and not a group of microparticles, is a quantum object. That is the first contradiction or obscurity or absurdity. And it leads to another. We know that particles obeying the laws of quantum mechanics look and behave in a manner that can neither be imagined by us with our minds and eyes bred to the classics, nor be described classically, that is, "intelligibly". Liquid helium, however, need not be visualized; it can be seen, and its behaviour observed, with our own eyes.

If the appearance of liquid helium at first did not much perturb the Dutchmen who prepared it, its "odd" behaviour certainly roused their curiosity. In the first place there were strange departures from familiar regularities, which again and again made them check their instruments and methods of measurement. Then there was a "can't believe your eyes" feeling. Liquid helium proved to be a great conjurer. The physicists who watched it kept saying "It can't be!" and "This is magic!"

So there is a reason why "quantum liquid" sounds strange, why there seems to be a contradiction in the mere juxtaposition of these two words.

Kamerlingh Onnes, continuing his struggle to force helium to become a crystal, overlooked some strange and illogical features in the indications of his apparatus, or did not attach sufficient importance to them.

One signal, as it were, though admittedly not a very loud one, came from helium in 1911: for some reason, the density of the liquid had a slight peak at 2.2°K. Kamerlingh Onnes noted this fact, but no more. Perhaps his attention at that time was taken up with another phenomenon, another riddle: the disappearance of the electrical resistance in some metals cooled to very low temperatures. This was brilliantly resolved. In that same year, 1911, Kamerlingh Onnes discovered superconductivity; two years later, he received the Nobel prize, "for his investigations into the properties of matter at low temperatures, which led, amongst other things, to the production of liquid helium".

Many years passed, and physicists came to understand that

there is some similarity between the behaviour of liquid helium and that of a superconducting metal, or rather in the internal processes which take place in them.

The complete theory of superconductivity was achieved almost fifty years after Kamerlingh Onnes's discovery. Landau too played a very fruitful part at one stage of this long journey towards the truth. And in his first (and most important) study of helium he noted that there was something in common between superconductivity and the behaviour of liquid helium.

Helium continued to dominate the thoughts of the Dutch physicists. No available method of acting on it, no investigation, no measurement was passed by. Naturally, the various physical properties of liquid helium were measured. In every case, anomalies were found near the mysterious temperature of 2.2°K, the experimental points coming away from the smooth curve.

Again and again the density of helium showed a maximum at this temperature. There was evidently no error in the experiments. But once more no explanation was imagined.

At one point, another signal was heard. The specific heat of liquid helium behaved very strangely – at that same temperature, where it rose steeply, only to fall again almost as sharply on further cooling. The movement towards absolute zero was accompanied by such pathological behaviour of the specific-heat curve that Kamerlingh Onnes and his colleagues must have attributed it to some external causes: the complexity and inexactness of the experiments, the errors in the readings of instruments, and therefore in the results of the measurements. For this or some other reason, they published only "definite and reliable" values, for temperatures above 2.2°K, the position of the inexplicable maximum of the density of liquid helium. The other values were not even mentioned.

When the Leiden physicists in 1926, after the death of their teacher, came to look closely at the results of his measurements, they found that Kamerlingh Onnes had determined the specific heat over a wide range of temperatures.

In the last months of his life, he and his colleagues discovered

a further anomaly, this time in the behaviour of the latent heat of evaporation. It was then that the idea was put forward for the first time, at least in public, that some change in the liquid helium itself might occur at this temperature.

Now the Dutch physicists began to be haunted more and more insistently by the thought that it was not a simple matter, that this puzzle was to be treated with the greatest seriousness and attention, because, as Keesom suggested, "the finding of increased values might be due to the existence of a new phenomenon in nature".

Keesom and his colleagues repeated many times the measurements of specific heat, obtaining convincing and stable results. Near the density maximum, the curve does indeed go steeply upwards. At this point, the curve does indeed have a discontinuity and begins to go almost as steeply downwards. At lower temperatures, the specific heat of liquid helium is indeed higher than at temperatures above the "catastrophe point".

Mystified by this result and by the whole series of persistent and steady signals indicating some new phenomenon, Keesom and his colleagues tried to see or, as physicists say, determine visually whether anything happens to the liquid itself at this point. And they succeeded.

They found that over an interval of about two degrees helium remained "the same". When the temperature reached 2.2°K (or 2.19°K according to later and more exact measurements), it broke loose. There were striking changes: its external appearance was altered, and its behaviour became quite different. (This sounds like the description of a person at the beginning of a detective story – and a detective story it is.) It was reminiscent of a crushed rebellion. The vigorously boiling helium became quiet and transparent, the bubbles disappeared. It seemed to have suddenly become completely stilled and subdued. And it would surely solidify, as all "well-behaved" liquids do when cooled.

But not so. Under the quiet exterior, unlikely things were coming into existence. The marvels of unbridled behaviour were really only just starting.

Why did the very experienced Kamerlingh Onnes not notice these changes? We find that in fact it is not very easy to spot them, for example, to see whether helium is agitated or quiet. It is not like lifting the lid of the kettle to see whether it is boiling.

One must clearly understand that, whenever liquid helium is involved, any procedure becomes an almost insoluble problem. Even storing (and retaining) it is not at all easy.

A very common term in low-temperature physics and technology is "Dewar vessel", or simply "dewar". Liquid helium is kept not just in a dewar, but in a whole series of them like Russian dolls one inside the other. Liquid air is usually placed between the dewars, and the inner one, containing the one where the helium is, is filled with liquid hydrogen, the coldest liquid other than helium. This system of cooling is necessary in order to prevent the helium from immediately evaporating and to conduct various experiments with it.

In order to see the helium, viewing ports have to be made in the silvered walls of the dewars, and this of course somewhat impairs the thermal insulation. They are therefore used only when absolutely indispensable.

Perhaps this desire, simply to see whether anything happens to helium as it passes through 2.2°K, did not even occur or have a chance to occur to Kamerlingh Onnes. Only later did his pupils, perplexed by all the strange effects which steadily recurred at 2.2°K, arrive at the idea of looking for themselves to see how helium behaves at the critical temperature, and whether the passage through the "singularity" has any effect or not as regards its external appearance and its behaviour.

No doubt the transformation which the Dutch physicists observed was what at last put an end to their patience and unwillingness to commit themselves. All were agreed that helium above and below 2.2°K is not the same thing, but two liquids differing in some fundamental way.

Two heliums or "helium two"

But how can it be the same liquid helium, and yet a different liquid? Can a substance be the same and also different? Water freezes, and changes into ice. It boils, and changes into steam. Thus the same substance H_2O can occur in three phases, or three states of matter. These concepts do not always coincide: "phase" is broader than "state of matter".

Some crystals can occur in various allotropic forms. Their lattices have different structures, and this causes changes in physical properties and external appearance. The passage from one form to another is a phase transition, but not a change of state.

All phase transitions, whatever their specific nature, have a number of typical and constant features: they occur at a fixed temperature (if the pressure is constant, of course); the transition is discontinuous; the phases coexist at the transition point; heat is absorbed or evolved (according to the nature of the transition) throughout the "transition period".

Usually, in any thermal process where heat is supplied, the temperature rises, and if heat is removed it falls. In phase transitions, all the heat supplied (or removed) is expended on the phase transition itself; the temperature does not vary, even by a fraction of a degree. This was the origin of the term "latent heat" of fusion, evaporation, and various other transformations.

Though now realizing that liquid helium undergoes some important reincarnation at 2.2°K, the Dutchmen at first supposed that this must be a phase transition, although there was surely only one state of matter in which such a transition could occur, namely the solid state. The existence of different phases in a liquid or in a gas was unheard of.

Keesom and his colleagues, who knew all this quite well, nevertheless thought of the mysterious temperature as forming some kind of boundary. Liquid helium above that point was called "helium I", and below it "helium II".

On careful study and attentive consideration, however, it was impossible to ignore the anomalies observed. The processes

always associated with ordinary phase transitions did not occur. There was no simultaneous existence of both phases. There was no latent heat of transformation; the slightest heating always caused a rise in temperature.

This was a deadlock. On the one hand, a clear and sharp transformation, a sharp transition. On the other hand, violation of all the necessary conditions for a transition.

In a well-written detective story, the astute colleague of Sherlock Holmes or of Inspector Maigret reaches the scene of the crime at the vital moment. The Dutch physicists were likewise very fortunate. At their side was Paul Ehrenfest, a talented and acute theoretician; he was then living and working in Leiden. Of course he noticed all these "little games" of helium. Apparently, the puzzled experimentalists turned to him first of all.

Ehrenfest carefully examined the accumulated results, and agreed that there was indeed a transformation of one type of liquid helium into another, but without any latent heat of transition.

It is hard for scholars to give up their familiar ideas. Only very cautiously do they express unexpected results which go against the rules. Here are some phrases from the Dutchmen's papers: "Within the error of measurement . . . within the accuracy of present experimental techniques . . . the authors felt justified in assuming there is no latent heat connected with the transformation in liquid helium." And so on.

Ehrenfest was not so timid. He asserted outright that the process which occurs in helium at 2.2°K is certainly a phase change, a phase transition. But equally certainly it is not an ordinary phase transition. It is an entirely new phenomenon in physics, with its own group of properties.

Perhaps the most impressive property of all was the discontinuity — a very large one — in the specific heat as a function of temperature, at this point. Looking at the curve, Ehrenfest suggested calling the transition point the "lambda-point", because the shape of the curve was very like that of the Greek letter lambda (λ).

There were other features of the transition. For example, at

the λ-point the phases did not coexist, and no interface between them was observable. Whereas in an ordinary phase transition we have one phase just above the transition point and another just below, and both coexisting at that point, something strange occurred at the λ-point.

Thinking about the physical nature of the λ-point, calculating from the theory, and applying the principles of thermodynamics, Ehrenfest arrived at a large generalization of the long-standing concept of a "phase transition". What had always been called by this name came now to be termed a "first-order phase transition". The λ-transformation in liquid helium was described as a "second-order phase transition". It soon became clear that "λ-transformations", second-order phase transitions with similarly distinctive properties, exist in various substances having a great diversity of states and natures. For example, the Curie point in a ferromagnet is of this kind.

Ehrenfest, however, having made this important contribution to physics, still could not break away from an unhappy inferiority complex and feelings of inadequacy, which formed one reason for his fateful decision to end his life. Even in a purely scientific paper, Keesom tells us, Ehrenfest "wished he could better formulate and understand this characteristic difference between a second-order phase transition and an 'ordinary' phase transition".

Some years later, Landau described as follows, with his usual clarity, the properties of the second-order phase transition noted by Ehrenfest: "Whereas, at a first-order phase transition point, bodies in two different states are in equilibrium, at a second-order transition point the states of the two phases are the same. The absence of discontinuity of state in a second-order phase transition means that there is no discontinuity in quantities which describe the thermal state of the body: volume, internal energy, enthalpy, etc. Hence, in particular, such a transition is not accompanied by evolution or absorption of heat. Nevertheless, at a transition point there is a discontinuous change in the dependence of these quantities on temperature. . . . The presence of these discontinuities is the main characteristic of

second-order transitions." (Let us interpose here a reminder of the discontinuity of the specific heat curve in liquid helium.) "Second-order phase transitions", continued Landau, giving now his own ideas, "always bring about some new qualitative property of a body, with a continuous change of state. This may be a new symmetry property (related to the magnetic properties of the substance), or it may be the occurrence of what is called superconductivity – the disappearance of electrical resistance."

Thus a first-order phase transition can take place between any two different states. The substance may retain the same quality, and only quantitative changes are bound to occur. For a second-order phase transition, there has to be some new qualitative property. The substance must be the same or different; the new property must be either present or absent.

This primary feature of a second-order phase transition was what made the behaviour of helium strange and puzzling. Helium I is a liquid, and helium II is a liquid. What qualitative changes can be occurring in the liquid? It is not a crystal that might undergo a change of symmetry.

Here we have a chance to look into Landau's "creative laboratory". His fundamental and widely known paper "On the theory of phase transitions" (which of course occupies a place of honour in the Ten Commandments) was published in 1937. At that time, Kapitza was beginning his experiments with liquid helium – and soon afterwards made a decisive discovery.

But as yet the chief feature of helium II was unknown to physicists, and they had no way of guessing what happens to liquid helium at the λ-point or what qualitative change might occur in it. Landau gave much thought to this problem, and he looked for any likely answers.

Is helium II, he suggested, a liquid crystal? The basic property of liquid crystals is that the liquid molecules all have the same orientation. For instance, if the molecules are elongated, they may all have their axes in the same direction. But this seemed to Landau fairly improbable for the case of helium: in order that an asymmetry, or non-equivalence of directions, should occur,

not in a liquid with complex molecules but in helium, which consists of only one kind of atom, the atoms themselves must have an asymmetric electron shell. "Since this is a somewhat strange assumption, the hypothesis that helium II is a liquid crystal also becomes dubious," wrote Landau, arguing against his own proposal.

Landau was convinced that a second-order phase transition takes place in helium II. But what could be its physical nature? The phenomenon was so incomprehensible that no other ideas occurred to him, and not even a way to a possible solution was found.

So the question remained open. Until. . . . The time was not quite ripe. But a deep interest in the subject was already established in Landau's mind.

Still in 1937, following his work on phase transitions, Landau published a paper "On the statistical theory of nuclei". Such a complete change in his field and subject of investigation was, as we know, always characteristic of him.

"If we allow for the interaction of the particles in the nucleus," wrote Landau, "there is of course no reason to consider the nucleus as a 'solid body', i.e. a 'crystal'; it is to be regarded as a 'liquid drop' of protons and neutrons. In contrast to normal liquids, quantum effects are important in this liquid, since the quantum uncertainty in the coordinates of the particles within the nucleus is considerably greater than their distance apart. Although we have as yet no method for the theoretical treatment of 'quantum liquids', we can still derive certain properties of nuclei by means of statistical arguments."

It can be seen that the quantum liquid had a firm hold on Landau's attention. He was already inwardly mature, and ready to penetrate deeply into this new world of physics, with a frontal attack on its fortress. But experimental physics had not yet prepared the ground for the decisive advance of the theory. Its next move had to be awaited.

So helium, and only helium, was able to remain a liquid but at the same time become another liquid, special and peculiar.

And the outward manifestations of this particularity were only the blossom. The fruits came when the physical properties of helium II were measured, that had years before been measured for all manner of substances, both elements and compounds, in various states: specific heat, thermal conductivity, viscosity – in brief, the classical macroscopic characteristics.

Then, helium II made itself famous, in just these classical characteristics! We must remember, of course, that the measurements were made in unusual conditions, very close to absolute zero.

If the change in the external aspect of liquid helium as soon as it crossed the boundary to become helium II was strange, much more astonishing were the numerical results obtained by the physicists, in particular the Keesoms, father and daughter. In their paper, which greatly excited Kapitza when it arrived in the latest issue of a Netherlands journal, they wrote that the thermal conductivity increased by a factor of three million when helium passed the λ-point and became helium II. This meant that the best thermal conductors in the world, the metals copper and silver, were several hundred times inferior to helium II – a mere liquid!

Thermal conduction is a specific (not just any) process of propagation or transfer of heat. We may say that it is a pure microprocess, but one that has a macroscopic manifestation. The gradual equalization of the temperature in a body is the result of the transfer of energy from some microparticles to others.

In a solid, the particles are quite firmly fixed at particular points in the body, and can vibrate only about these "equilibrium positions". They thereby share energy with their neighbours, which in turn share with *their* neighbours, and so heat is propagated and the temperature gradually equalized. This is the mechanism of thermal conduction. If a solid is an insulator, there is a very slow transfer of energy from molecule to molecule. In a metal, of course, heat is transferred not by atoms or ions but by the thermal motion of free electrons scurrying through the crystal lattice. These electrons have

velocities of the order of a thousand kilometres per second, whereas atoms and molecules move at only about one kilometre per second. That is why electronic thermal conduction, as in metals, especially silver and copper, is so great, some thousand times more than in other substances. Liquids and, in particular, gases behave differently. Their particles do not have fixed positions. They can move more or less freely relative to one another and to the walls of the container — a beaker, a flask, or the oceans of the world; in a word, they can flow. The ability of a liquid to flow implies the presence of a form of heat transfer quite different from conduction (and usually in addition to this, not instead of it), called convection. This is a typical macroprocess, involving the movement of fairly large volumes of material. Heat transfer then occurs not by the random thermal motion of atoms or molecules or electrons but by the largely ordered flow of macroscopic groups of particles. For instance, the movement of quantities of warm air from a bonfire, a stove, or a radiator is of this type. Heat flow can also occur where there is a gradient of density or pressure.

In order to revert to pure conduction of heat, any such flow or movement of the medium concerned must be excluded. It must be uniform and at rest.

The Keesoms therefore proceeded quite correctly by enclosing helium II in a capillary. It could not get away from there: one would hardly expect any flow of helium, and therefore of heat, to be possible in a slender capillary tube. Evidently, if the helium is heated at one end of the tube, the first layer of atoms will begin to vibrate more strongly and will transfer some of its energy to the next layer, and so on to the other end of the capillary, which will eventually acquire the temperature of the hot end.

This will give a measure of the true thermal conductivity unobscured by other processes or forms of heat transfer. It is certainly the normal way of measuring the thermal conductivity of a liquid. Or, one should rather say, of a normal liquid; for, as was becoming more and more clear, helium II is certainly not a normal liquid.

"As the temperature falls below the transition point, liquid

helium suddenly begins to conduct heat in a way that seems quite supernatural for a liquid," said Landau in one of his popular lectures: "You have probably heard that liquids are very poor conductors of heat, and in particular that ordinary water conducts heat poorly. Other liquids have just as little thermal conductivity, with the exception of mercury, which, like all metals, is a good conductor of heat. Helium I, ordinary liquid helium, is also a poor thermal conductor. Yet, as the temperature falls to the point where liquid helium changes from helium I to helium II, it begins to conduct heat better than copper and silver, the best thermal conductors, and the change is an abrupt one. The property of enormous transfer of heat was of course noticed immediately, and showed that this mysterious liquid conceals many more surprises."

Another fact was lying in wait. The viscosity, like the thermal conductivity, is one of the properties of matter which physicists have long been accustomed to measure. "Viscosity", said Landau in the same lecture, "is the ability of a liquid to oppose movement. You can easily imagine how much harder it would be to swim in honey than in water. Accordingly, we say that honey is a much more viscous liquid than water."

Kapitza too was greatly interested in the viscosity of liquids and its relation to thermal conductivity. "Viscosity is the property of a liquid that governs its ability to flow – a kind of measure of internal friction in the flow of the liquid. For example, in a tube, a layer of liquid adjoining the wall is at rest, the next layer is moving with a certain velocity, the layer above it with a somewhat greater velocity, and so on, rather like a system of moving pavements that was once suggested. There is a slippage between these layers, accompanied by friction due to the fact that the atoms in each layer lag behind those in the next layer and by their forces of interaction oppose the motion. This causes a loss of energy which brings about the viscosity of the liquid. The picture just given shows that viscosity must increase with the extent to which the movement of atoms in one layer affects that of the atoms in the other, i.e. with the ease of propagation of heat in the body."

Indeed, the closer the relations between molecules, the greater is the thermal conductivity, and, for exactly the same reasons, the greater is the viscosity.

And the measurements in helium II? The viscosity of liquid helium was measured by Canadian physicists. They found that it has a very low viscosity, about a thousand times less than that of water (which in turn is much less than that of honey, as noted above). Most important of all, the lowest viscosity is that of helium II, which is several times less than for helium I.

A serious contradiction had arisen. Indeed, how should one combine what is incompatible? In one scale, the fundamental and immutable relationship: as the thermal conductivity increases, so does the viscosity. This is unshakeable, for it follows from the laws of physics and from the mechanisms of the two processes. It must be so. Not the reverse, as found for helium. And in the other scale, equally weighty facts: the experimental results of fully qualified and authoritative physicists. One might suppose some inaccuracy in the measurements, some errors creeping in. But not by factors of hundreds or thousands!

Kapitza, before beginning his own experiments, and merely examining published work, compared the results of the Dutch and Canadian physicists, and understood that the contradiction could be resolved only by a thorough reconsideration of these results, applying the age-old principle of "Doubt everything". He himself must re-measure all these properties. But in addition – and above all – he must know for sure whether these processes do in fact have the physical nature attributed to them, whether they are what they appear to be. This applies especially to thermal conduction.

The fantastically large thermal conductivity (thermal superconductivity, the Keesoms called it) made necessary a consideration of the true nature of heat transfer. Was the classical mechanism of thermal conduction actually present and being measured? or was some other type of heat transfer involved?

Kapitza suggested that heat was being transferred convectively by a flow of liquid helium. The high fluidity, or in other words

the low viscosity, of helium II confirmed this hypothesis. The picture seemed to be becoming clearer.

But as soon as Kapitza went on from the qualitative verbal explanation to actual calculations, he found that this viscosity, small in relation to the existing thermal conductivity, was too large to be compatible with convective heat transfer.

It now became necessary to cast serious doubts on the correctness of the Toronto results. There were two reasons for these doubts. Although one reason related to the technique of measurement, and the other related to the physical process of motion of the liquid, there was a basic connection between them.

To anticipate, we may say that what had been long known and countless times verified, and was as undoubted as $2 + 2 = 4$, was in helium II suddenly contorted by unexpected features and paradoxes that were completely unintelligible until Landau hit upon the truly fantastic explanation of the nature and behaviour of helium II. But more of this in due course.

So, the motion of a liquid. Viewing the process in a very rough and simplified manner, we should have to say that a liquid can flow in different ways: either quite quietly, with every particle moving in the same direction and preserving the existing order, or with eddies and similar kinds of "disorganization". Motion of the first kind is said to be laminar, and that of the second kind turbulent.

Turbulence of the flow may greatly increase the apparent values of the viscosity of the liquid. It was therefore necessary to devise a method of measuring the viscosity which would eliminate, or minimize, the turbulence of liquid helium.

As to the measurements themselves, there are two basically different ways of measuring viscosity. The liquid can be passed through narrow capillaries or slits and its rate of outflow measured; the higher the viscosity, the lower the rate. Or a solid, say a rotating cylinder, can be moved through the liquid in some way, and the viscosity that retards this motion can be calculated from the magnitudes of the forces that cause the cylinder to rotate.

The Canadian physicists used the second method. Kapitza suspected that their results were affected by turbulence, which often occurs when that method is used.

It must be stressed, however, that in "normal" liquids with properly conducted measurements the values of the viscosity measured by the two methods are exactly the same. So Kapitza's assertions contained some truth, but not the whole truth. The fact of the matter was far more complex and more significant.

Kapitza studies the behaviour of helium II and discovers superfluidity

Most ideas in physics arise from the need to explain what cannot be explained by existing theories. Landau's doctrine of quantum liquids arose partly in this way, partly not. In this way, because it explained what otherwise could not be explained. Not in this way, because there were no "existing theories". The revealing and the "discovery" of the actual behaviour of helium II still lay ahead. Accurate and reliable experiments were still to be done to lay bare the physical essence of the strange processes found to occur in it.

Kapitza achieved this in his acute and elegant investigations. Before him, there was no basis on which to construct a theory. A series of studies at the Institute of Physical Problems in Moscow revealed so many surprising phenomena that they immediately attracted attention and urgently called for explanation.

In the experimental stage also, complications and riddles successively arose, indeed formed a continuous procession leaving no opportunity to relax or to think complacently that the main difficulty was past.

Kapitza often declares with pleasure the need for contradictions as the motive power of science. Now his beloved contradictions repaid him with interest. They seemed to crowd round the laboratory doors and take turns to go on duty at each stage of the investigations, as if to nullify even the germ of an explanation.

The one thing that Kapitza's work did not have was tedious monotony.

Probably no undertaking can ever seem so attractive and absorbing as it does to the person actually doing it, carried away by it, living it. This is a case where to be a spectator, an odd man out, is not very pleasing; for it is not a play or a concert, and the spectator, even if understanding what and why, cannot find it as interesting as does the participant in the creative process.

Kapitza emphasized that even to direct scientific work, with good understanding of it but taking little part in it, is not something for the true scientist: "I am convinced that when even a very great scientist stops working by himself in the laboratory, he not only hinders his development, but is no longer a scientist." And: "Only when one works independently in the laboratory, and carries out experiments, even if often the most routine part of these, can one achieve genuine results in science. One cannot do good research by somebody else's hands."

Many a researcher has discovered for himself the truth of these words. For "the most routine" work is never done without thought and feeling. One would imagine it was just the readings of the instruments. But for the performers of the experiment it is the revealed picture of the phenomenon, perhaps glimpsed for the first time; perhaps expected, even accustomed; perhaps overturning all one's ideas; it may be any of these, but it is never a matter of indifference. This is why the experimenter's life is so full, not only of labour, but also of experiences.

Behind the measurements that he repeats, often lengthily, wearisomely, monotonously, the researcher is always seeking to discern the process, to divine its course and its outcome. He is rarely attended by satisfaction, always by hope, and often by disappointment. "Lady Luck" is there too, of course. But the results are more usually disenchanting. Even then, he must be extremely attentive, because the contradiction itself may sometimes conceal the seeds of success.

There are few cases in the practice of science where an idea and plan of experimental work, seemingly convincing and clear

in conception and design, have not undergone manifold changes in the course of the investigation. The results themselves pose new problems, bring about new directions, call for further experiments that had not been foreseen. You must continually reflect and replan as you go along. This cannot be delegated to your colleagues. You must see everything for yourself, do it with your own hands, feel it yourself. That is the only way to be in control of the investigation.

Kapitza started experiments with helium at the beginning of 1937. In the course of a few months he carried out his first and fundamental study, ending with the discovery of superfluidity in helium II.

This was followed by some three years' further careful study of the behaviour of helium in various circumstances; repeatedly checking and rechecking to be sure that all the wild escapades of helium were not accidental, not fictitious, not the result of experimental error; verifying that all these baffling effects represented the true physical nature of a phenomenon previously unknown, that they were accurate, reliable, and in need of theoretical interpretation.

There is evidence from that period in the form of numerous detailed records of experiments and measurements, successively carried out day by day. These diaries were made and are preserved by Sergey Filimonov, Kapitza's constant helper in all his work.

Kapitza had no scientist colleagues in these investigations (Filimonov was at that time a mechanic); he himself conducted them from start to finish. But he was helped by two outstanding craftsmen.

"Building apparatus is creative work too," Kapitza is fond of saying. "Nothing is so hampering and depressing in scientific work as waiting for the experimental equipment to be prepared."

Alexander Petushkov could do anything in the glass-blowing line – make dewars of any shape and arrangement, attach any kind of device to them, make anything move that was intended to move and anything stand still that was intended to stand still,



Filimonov and Kapitza (1940)

create a vacuum wherever required, and so on. Filimonov too seemed to find nothing impossible as regards the mechanical and electrical parts of the apparatus. Every idea and plan of Kapitza's was at once understood, taken up, and embodied in a working piece of equipment. Any modification of the experiment meant changes in the apparatus also, yet very often what was thought of in the evening was ready for use the next morning. There was truly nothing "hampering and depressing" for Kapitza here. He worked with Filimonov so closely and with such complete understanding that nothing more could be wished for. And Filimonov was also his constant helper in the experiments with helium II.

Every day Kapitza, or more usually Filimonov, completed the records of the experiments: a tiny outline diagram showing the main objects and principles of the work in progress; the initial information; and then columns of numerical results. And

if possible, if the results allowed it, conclusions were added.

The thick files of these daily sheets are weighty in both the literal and the figurative sense. Only on two occasions in more than three years' work do they show Kapitza writing in large letters with a coloured pencil "Experiment failed" (because an instrument gave incorrect readings) and "Experiment unreliable" (for a similar reason – something failed to operate). All the other hundreds of experiments gave dependable results.

Kapitza's first task and aim, then, was to determine accurately the viscosity of helium II. It was known that the greatest threat to accuracy here came from turbulence. The eddies that are easily formed in a liquid of low viscosity increase its actual viscosity. Hence, in order to minimize the turbulence, if not to eliminate it completely, the liquid is made to flow through narrow capillaries, the viscosity being found from its rate of passage.

"The problem of measuring the viscosity of liquid helium is a particularly difficult one," wrote Kapitza, "since the method must be such as to preserve laminar motion even with an exceptionally low kinematic viscosity."

Kapitza's apparatus was designed to retain the advantage of capillaries, namely their narrowness, which prevents turbulence. But he greatly increased the sensitivity of the method by allowing a much larger quantity of liquid to flow through his apparatus than could pass through the capillaries.

He began measuring the viscosity in the flow of helium through the narrowest possible slits, which it seemed even the most inviscid liquid could scarcely penetrate. And indeed, when the apparatus was filled with helium I, only a barely perceptible amount had squeezed through the slit after several minutes. But when helium II was in the dewar, a huge amount of it (in comparison with helium I) flowed through in a few seconds.

Kapitza, however, believed that he too would not succeed in completely avoiding turbulence and achieving purely laminar flow of helium through the slit. With more and more experiments, better and better techniques, narrower and narrower slits,

he saw more and more clearly that his truly remarkable low values of the viscosity were not yet the limiting ones. There seemed to be simply no lower limit. This idea pressed more and more strongly upon Kapitza's mind.

Concluding this stage of his investigations, Kapitza wrote that liquid helium II "has a viscosity at least 10,000 times less than that of gaseous hydrogen at low temperatures (when its viscosity is least) – until now supposed to be the least viscous of all fluids. I think that our upper limit is already sufficiently low to consider, by analogy with superconductivity, that liquid helium below the λ-point assumes a special kind of state which can be called a 'superfluid'."

Thus was uttered the word "superfluid". In seeking to discover the sources and causes of the evidently abnormal "thermal superconductivity" measured by the Keesoms, Kapitza actually discovered a new phenomenon in physics – the superfluidity of helium II. One might think of the comparison with Columbus, who set sail in search of the route to India, and discovered America. That might be a somewhat superficial comparison, in that, although the phenomenon of superfluidity was in itself unexpected, Kapitza's discovery of it was not by chance, and he quickly saw what America it was. Among physicists, of the twentieth century especially, there have been Columbuses who had the ability to reach their various Americas.

The discovery of superfluidity, a previously unknown property of matter, was the start of a whole sequence of studies by Kapitza on the properties of helium II. For even the strangely enormous thermal conductivity was not yet explained (and was not measured by Kapitza himself). Intuitively, it seemed that the explanation must depend on superfluidity, which, Kapitza thought, promotes the occurrence of convective flow and of turbulence in the liquid, both of which processes should greatly increase the heat transfer.

Kapitza was right in his initial assumption. But how immensely more complex, unforeseeable, and unpredictable were all those processes that he witnessed with amazement during his various experiments. To them we shall return in a moment.

"The most remarkable property of liquid helium", said Landau in a popular lecture, "was discovered by a Soviet physicist, Pyotr Kapitza. He showed that it has no viscosity at all. He performed a very simple but exceptionally important experiment. He observed the flow of helium through very narrow slits. They were so narrow that even a liquid with such a low viscosity (by ordinary standards) as that of water took many days to pass through them. Liquid helium II was found to flow through in a few seconds. Kapitza was able to show that the viscosity of helium is less than that of water by at least a factor of a thousand million. This is only an upper limit set by the accuracy of the experiments, the limit of viscosity observable by Kapitza. The viscosity of helium II was so small that he could not measure it. We may say that liquid helium has simply no viscosity. This phenomenon is termed superfluidity, and helium II is said to be a superfluid."

Landau also told of another surprising fact. (Helium was always showing physicists a new trick, and they could never see how it was done.) "Below 2°K", he said, "liquid helium has the strange property of moving inexplicably from one container to another. If two containers of helium are in direct contact, and the level in one is higher than in the other, after a time the levels will be the same."

(Everyone knows about communicating vessels, but Landau is referring to vessels that do not communicate in that sense: for example, two halves of a container, separated by an impermeable partition, with more helium in one half than in the other.)

"Kapitza's discovery of superfluidity", Landau went on, "at once explained the apparently almost mystical flow of helium from one container to another. All liquids that wet the containing walls cover them with a thin film, invisible to the eye and usually having no detectable effect. Liquid helium, because of the superfluidity, flows quite quickly out of the container along this film, a hundred-thousandth of a millimetre thick. Another property of helium was even more surprising. Kapitza showed that it is superfluid, i.e., passes instantaneously through any slit. Flow through a slit is not the only way of measuring viscosity.

Physicists have other methods, based on the resistance of a liquid to a body moving in it.

"If you want to measure the viscosity of water, you can do it in two ways: either allow it to flow through a slit, or move a body in it and determine the viscosity from the forces acting on the body. Both methods were used for helium, and it was found that, whereas for all other liquids they give completely identical results, both qualitatively and quantitatively, for helium they give what might be called opposite results. Liquid helium II flowing through a slit is superfluid, that is, it shows no viscosity at all. Yet it does oppose the motion of a body. So, whereas all ordinary liquids have one ordinary viscosity, liquid helium has two viscosities of quite different kinds: one infinitesimal or absent, the other entirely perceptible and measurable."

It was time to tackle the puzzle, posed by the Keesoms, of the "thermal superconductivity" of helium II, and to find out what this really was, and what was the cause of its enormous magnitude.

Initially, a simple, clear, and seemingly reliable picture was put forward. Helium II is a superfluid. Heat can and must therefore be propagated in it by convection. Also, because of the superfluidity, this propagation must be very quick.

This explanation, so long as it remained purely qualitative and verbal, was unobjectionable. But physics is an exact science. If convection is mentioned, it must be describable in quantitative terms. The rate of convective heat transfer is closely related to the rate of motion of the substance itself.

In order to test the numerical agreement of the rates of convection and heat transfer, Kapitza devised a precise technique for measuring the two quantities. For instance, to find the exact amount of heat transfer, he was able to measure a temperature difference of a few millionths of a degree.

Another mass of contradictions emerged. Kapitza obtained a value even higher than that measured by the Keesoms. The corresponding rate of convection had to reach a thousand metres per second.

"It was unthinkable to suggest", Kapitza said later, "that the helium in the capillary moves faster than a bullet. It can be shown that there is no source of energy suitable for such large convective fluxes. Our results seemed to lead to even greater difficulties if the convective mechanism of heat transfer was excluded."

But the mechanism of ordinary heat conduction by the transfer of thermal motion from one atom to another also did not at all agree with the figures, but contradicted them. It was clear that heat transfer in helium II took place by some peculiar and unknown means.

"Where now to look for the mechanism of this heat transfer, without any guiding idea?" said Kapitza, recalling that period. "Our results were in fundamental contradiction with all known theoretical concepts. We spent about a year trying to escape from this impasse. We had to grope our way forward, test all manner of physical factors that might affect the thermal conductivity. We tried the effect of pressure, gravity, time, and so forth, on the heat transfer in helium II. The results were negative: the thermal conductivity was unchanged, and remained just as great." Kapitza hoped that, by stumbling on some way of influencing the amount of heat transfer, he might get the keys that would unlock the mechanism of the phenomenon. Then suddenly, quite by chance. . . . One can "suddenly" be run over by a car or find a purse full of money. But when you are seeking all the time, with concentrated thought and attention, "suddenly" becomes expected and wanted.

Still, in fact, suddenly, quite by chance, they noticed that the "untameable" thermal conductivity was very sensitive (believe it or not) to extremely small fluctuations of the helium pressure in the supply pipe. Just as gas, and hot and cold water, are commonly piped to a laboratory, so liquid helium was piped to Kapitza. These fluctuations were of course transmitted to the helium in the capillary.

"Finally, one quite chance observation gave us a whole new line to follow," Kapitza related. "We found that the pressure fluctuations transmitted quite by chance from the laboratory

helium supply pipes to the helium in the capillary altered its thermal conductivity considerably. Although the fluctuations were very slight, they reduced the conductivity of helium II by a factor of tens."

Kapitza began to cogitate how these slight fluctuations could have such a great effect on the thermal conductivity of the helium. "The most straightforward explanation was this: we know that liquid helium II is a fairly easily compressible liquid, about a hundred times more compressible than water; so the pressure fluctuations compressed the liquid and could cause a flow of helium in the capillary where the thermal conductivity was being studied. We supposed that such a flow would affect the thermal conductivity. To verify the correctness of this explanation, it was necessary to carry out experiments to measure the conductivity of helium when flowing through the capillary. We then found that the thermal conductivity of helium II flowing in a capillary is indeed reduced by a factor of a hundred or even a thousand. These experiments also revealed that helium in the capillary flows easily when it is subject to a flow of heat, giving a relation between the flow of liquid helium and its ability to transfer heat, which was the key to further investigations."

So Kapitza described the strange and unexpected, but happily successful, end to the long searches and wanderings. This was how he escaped from the impasse and was able to go on to a direct study of all the aspects of the behaviour of helium that are related to heat.

The link between motion and heat, or between heat and motion, proved to be a very unusual one in helium II. Its exceptional nature is shown by a whole series of effects never observed elsewhere, and found in no other physical processes.

In looking at such effects or even reading about them, one can more easily imagine oneself watching a conjurer than sitting in a laboratory during an experiment, where many instruments are impassively recording everything they see; one wants to say "Don't believe them!" – one's own eyes.

Perhaps the most striking things were these. There are two connected vessels containing helium. Once they are connected, of course, the helium has exactly the same properties in both vessels, and in particular the same temperature. In the experiment, helium is made to flow through a narrow slit from one vessel to the other. One could then simply expect the amount of helium to increase in one vessel and decrease in the other. But not so.

"When helium flows through a slit, a strange thermal phenomenon occurs," said Landau. "If it flows from one vessel to another through a very narrow slit, the helium in the receiving vessel becomes colder, and that in the other vessel becomes hotter. This is called the 'thermomechanical effect', and was in itself most surprising."

Surprising indeed. The helium flowing out seems to carry with it an additional quantity of cold, which it transmits to its new container. The old container, conversely, retains some additional heat. Physicists simply could not understand this strange behaviour with heat – as if it could be separated from matter and given a life of its own to decide where to stay, where to leave, where to go.

"Kapitza was able to perform experiments that seemed even more surprising," Landau went on.

Here is an outline of one of the experiments that Landau had in mind. Helium is in a large dewar, in which is immersed a tiny flask (a bulblet, as Kapitza and Landau call it) also filled with helium. One end of a capillary is sealed into the bulblet, with the other end open. Various small objects (a vane, a mirror, a screen) can be placed on a balanced lever near the open end. From their reactions, both observed visually and unmistakably recorded by various instruments, it was possible to establish what was happening to the helium in the bulblet and the capillary in various situations, especially when it was heated.

Kapitza set a light vane opposite the aperture of the capillary. As soon as the bulblet heater was switched on, a jet of helium began to come from the aperture. Though it was invisible, its pressure caused a quite visible deflection of the vane. Then, by

moving the vane slightly, Kapitza showed that this jet has a precise direction along the axis of the capillary, with almost no spreading and no deviation.

"A most surprising situation," Landau said of this experiment. "It was not only surprising that heating causes a jet of helium to flow for no apparent reason. Still more surprising was the fact that the vessel does not become empty. If a jet of liquid is steadily taken from a vessel, there will soon be nothing left in it. Here, no change occured. The vessel was as full of helium as before."

Landau was not exaggerating. Everyone who had observed this experiment and seen the whole thing for himself was ready to declare that it must be an optical illusion. A jet of helium flows out of the vessel, continuously, without stopping; and however long the experiment lasts, you can see how the jet deflects the vane quite steadily, without slackening. When will it stop? When will the supply of helium in the vessel be used up? Never. It is quite clear that there is just as much helium left as there was to start with.

"It is a Biblical experiment, like the bush that burned with fire and was not consumed," said Landau. "Here too, the bulblet from which the jet flows does not become empty, but remains just as full as it was originally. This is one of the many paradoxical properties of liquid helium. At first sight, its properties seem completely absurd, like the story of the giraffe which evoked the exclamation 'There ain't no such animal!' This is approximately one's reaction to the properties of liquid helium. One feels that this cannot be, that these properties are logically contradictory. Of course, no logical contradiction exists here or in any other branch of physics. It shows only that the properties are due to very unusual causes that are quite strange to our minds. Later, I was able to devise a theory that explained some of the important properties of liquid helium."

Even before Landau set up his theory, Kapitza suggested the following explanation of the paradox, as a working hypothesis (such as is always needed in the process of research): for as long as the jet of helium is flowing from the capillary, helium creeps

back into the bulblet along a very thin film on the wall of the capillary itself. Because of the superfluidity, this takes place very quickly, and so the helium in the bulblet is not exhausted.

Kapitza used the same mechanism to explain the very large thermal conductivity of helium II. "There is reason to suppose", he said, "that the helium moving in a thin film on the surface is in a different physical state from that which is flowing in the opposite direction in the centre of the capillary. It is in a somewhat different energy state. In thermodynamic terms, it has a different enthalpy from that of free helium. This assumption seems to be quite sufficient to account for the large heat transfer in helium, observed in the capillary. The experimental picture is that helium absorbs heat when it flows into the bulblet along the inner surface of the capillary, and then leaves the surface and becomes free. This gives the appearance of a very large thermal conductivity."

The true process was even more unexpected than Kapitza supposed. Nothing like it had been previously known in physics. And nobody could have just guessed it beforehand. With all the accumulated results of the experimentalists to hand, Landau proceeded to explain theoretically the phenomenon of helium II.

Landau uses quantum mechanics, the physics of the microworld, to explain events in the macroworld

"I was able to devise a theory that explained some of the important properties of liquid helium," Landau afterwards related in the lecture theatre of the Polytechnical Museum. He added, having regard to the variety of training and occupations among his audience, "It would be impossible to explain to you the essential nature of this theory, even in outline. It is based on one of the greatest achievements of twentieth-century physics, namely quantum mechanics, a branch of physical theory that is extremely complicated both in its method and in the physical concepts that it embodies, and that is characterized by many of its ideas' being very imperfectly accessible to our perception.

This arises because our perception is rooted in everyday experience rather than in our intellectual power. We easily perceive what we have seen, but only with great difficulty perceive what we have not seen."

Landau's refusal to attempt a popular exposition of his theory was no accident. It resulted from his scientific chastity, so to speak – a fear, probably unconscious, of somehow cheapening the highest and most essential aspects of science. And from an indissolubly associated belief, the other side of the coin, in "our intellectual power".

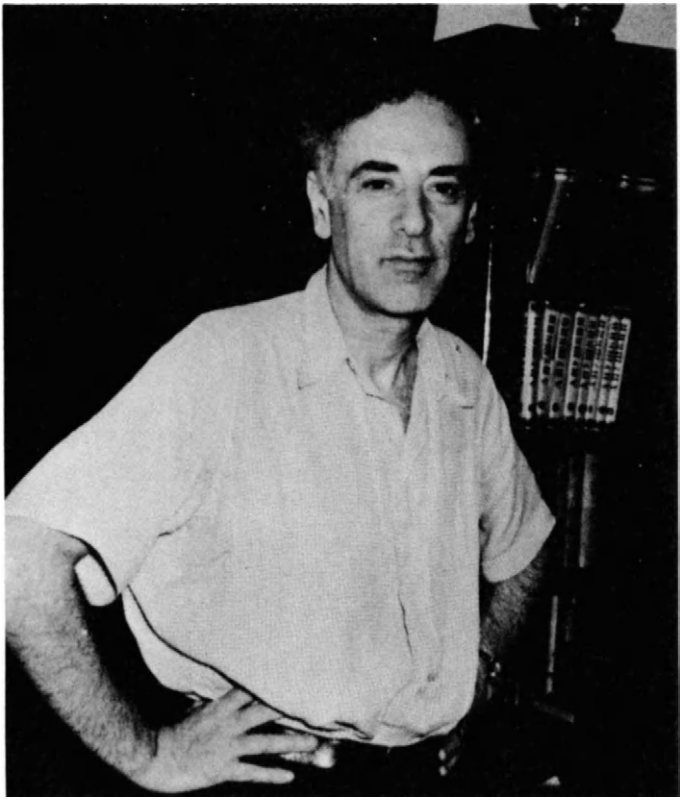
Landau is saying, as it were, "do not seek to cheat Nature by attempting to imagine what cannot be imagined; this is unbecoming, and it is you who will be cheated. Better to trust to reason, which will not fail to aid you. Do not unworthily rely on imagination and feelings, where they cannot help you. What belongs to the mind, and is not an illusion, has to be comprehended by the mind. Render unto Caesar the things which are Caesar's; and unto God the things that are God's."

The friends and pupils of Landau often heard from him such panegyrics to the force and power of the human brain, mind, and intellect. For example, Evgeny Lifshitz writes: "He told how amazed he had been by the incredible beauty of the general theory of relativity (sometimes he even would declare that such a rapture on first making one's acquaintance with this theory should be a characteristic of any born theoretical physicist). He also described the state of ecstasy to which he was brought on reading the articles by Heisenberg and Schrödinger signalling the birth of the new quantum mechanics. He said that he derived from them not only delight in the true glamour of science but also an acute realization of the power of the human genius, whose greatest triumph is that man is capable of apprehending things beyond the pale of his imagination. And of course, the curvature of space-time and the uncertainty principle are precisely of this kind."

For this very reason, apparently, Landau, though a great popularizer, flatly refused as a matter of principle to "popularize

the unimaginable", or gave only a phenomenological, "external", "descriptive" account.

However, since Landau did not actually forbid a popular exposition of his theory, we can risk attempting this, can venture at least to give a hint of its basic ideas. To do so, we probably ought to realize a little of the character of Landau's thought, the trend of his mind. The mystery of a scholar's thinking is a very complex topic, and it is not in any way simplified if the subject is a theoretical physicist; the more so if the subject is Landau.



Landau in 1960

Of course, it is all very tempting to penetrate into the laboratory of the mind and creativity of the scholar. But this is scarcely possible: not only, and not even mainly, because every scholar has his own process of thought with ungraspable differences and nuances, but because the greater part of the process is in the book sealed with seven seals. Not that the scholar, or the poet, or the artist is anxious to keep his secret. The mechanism of their creativity seems to be to some extent a secret for them too.

There is a documentary film of Pushkin working on a manuscript. The successive changes of wording in the drafts are shown. And you see for yourself how the ordinary, almost randomly chosen words are replaced by good ones, the good ones by excellent ones, and these finally by brilliant ones. This is his own secret, which none may discover, otherwise there would have been many Pushkins. The secret is that this changing of words seen on the screen, achieving the brilliant, does so only in one particular context; and such an optimum result is found only by the intuition of genius.

A youthful newly-fledged philosopher once asked a good physicist to explain the thinking of Einstein and Bohr. "If I could do that, I could probably do something like what they have done," was the answer.

Einstein's account of how and why he conceived the idea of relativity theory is well known. It was all due to his "late development". He was older than average when he began to think about the problems that one usually ceases to think about while still young: "What is time? What is space? . . . " So the theory of relativity was born.

As to Bohr, perhaps one should say that he was simply enlightened by the dawn when he put forward one of his celebrated postulates in constructing the model of the atom. Or that he had to find somehow an escape from the position of no escape, and so had the "crazy idea" of the electron that revolves in its own orbit round the nucleus without radiating electromagnetic energy. Moves and does not radiate! Bohr's postulate, firstly, contradicted the inviolable law of physics, that any

moving charge (or charged body) necessarily radiates electromagnetic energy. Secondly, like any postulate, it was unprovable, devoid of justification. Does not radiate, and that's that! The energy of a radiating body decreases, and a continuously radiating electron would ultimately just fall into the nucleus; in reality, this does not occur. Thus Bohr discovered one of the most surprising laws of the microworld. Such is the intuition of genius, the boldness of its thought. But to try to understand its mechanism . . .

It probably often happens that the simple realization of the immensity of the task and the impossibility of performing it by existing methods causes psychological difficulties that hardly anyone could overcome. And perhaps, in the combination of abilities both to abandon the existing methodology, the familiar approach to the problem, and to overcome this inner psychological barrier that impedes freedom of thought – in such a combination of scientific and human boldness; in not allowing oneself to fetter one's mind and imagination or be afraid of the unusual and unlikely ideas that arise; in all this, perhaps, lie the origins of what we call the intuition of the scholar.

To describe on quantum principles (that is, using the formalism of quantum mechanics) the behaviour of a liquid, which is a vast assembly of disordered particles, is a problem that, as the physicists say, cannot be solved in a general form.

In order to make an approach to the problem, it was necessary to find some special treatment, to guess which path would lead to the goal, to "see" the whole picture in a special light. Not in the ordinary classical way in which we all imagine a liquid as a random collection of closely packed molecules, like a flock of sheep in a pen, but in the new "quantum-mechanical view", where randomness is no longer random, since the "quantumness" of helium II generates an order of a quite special kind. This order had to be perceived and detected; and not just its existence, but its nature and character. The mathematical formalism had to be found which correctly describes the phenomenon and allows a valid theory to be constructed. Landau perceived, detected, and found what was needed.

This fundamental discovery was the sum or the sequence of successive constituent discoveries, ideas, guesses, refined mathematical calculations, and even experimental results applied by Landau – not only to account for the latter and to confirm the theory with them, but also to construct some parts of the theory.

Legend has it (and perhaps it is a historical fact) that the Egyptian king Ptolemy asked Euclid to teach him geometry not in the usual way but in a simpler and easier way. Euclid replied, "There is no special road to geometry, even for a king."

Learning Landau's theory of the superfluidity of helium II cannot be a pleasure trip. It calls for the application of both thought and imagination. It needs effort, naturally. Some tools also. The reader will be provided with a set of tools. But only his desire and inclination can provide the time and the effort.

The ideas on which Landau based his theory are fairly complex and unusual. The story here will be told at the level of ideas, and in no way attempt their mathematical embodiment. But the concepts underlying the ideas are also complex and unusual. So too is the terminology. We have to master all these to some extent.

When it became evident that the laws of classical physics cannot explain the behaviour of helium II, and in particular its superfluidity, the theoreticians turned to quantum mechanics.

Even before Landau, there were attempts to explain the superfluidity of helium on quantum principles. The physical content of the idea underlying these attempts was that some atoms of helium II are in a different quantum state from the other atoms in the liquid. Those who framed the hypothesis supposed that the atoms in this different quantum state, with zero energy, called the ground state, should move through the rest of the liquid without friction, i.e. behave as "superfluid atoms".

Landau showed that this view is invalid and self-contradictory. Firstly, he said, the separation of helium atoms into two different

types is physically impermissible: helium II is a single liquid, and all its atoms are indistinguishably alike. Secondly, even if there were two such types of different atoms, superfluidity could not result. The atoms in the ground state would collide with excited atoms, exchanging momenta with them, and therefore causing friction. Thus there would be a non-zero viscosity, perhaps considerable; and so there could not be superfluidity.

Landau started from quite the opposite position. First of all, it was necessary to forget the existence of individual helium atoms and ignore their individual behaviour. Helium II had to be regarded as one huge molecule filling the whole available volume, in the same way as a crystal is regarded in some problems; or, more precisely, as a single quantum-mechanical organism.

For example, in an ordinary gas obeying classical laws, we can imagine some molecule to be selected, and follow its motion and its collisions with other molecules, and describe the process. The classical approach is to account for the properties and behaviour of matter on the basis of the properties and behaviour of particles, and it was for long the only one available in physics. But whereas for gases it usually gives correct results, difficulties arise even for an ordinary normal liquid, because of the strong interaction of its particles.

For the quantum liquid helium II, such an approach is erroneous, and such a description is in principle impossible. The reason is that, according to the laws of quantum mechanics, all the atoms in such a body are in principle indistinguishable. If any of them change places, this in no way affects the whole assembly; there is no alteration in its state or in its mathematical description. Because of this fundamental indistinguishability of atoms, even in an imaginary experiment we cannot select one atom and follow its motion, behaviour, etc.

Moreover, in a liquid such as helium II, the interaction of atoms is so great that even the expression "the state of the (or an) atom" has no meaning, because the motion of every atom depends in some way on that of all the other atoms in the liquid.

Landau, ceasing to regard helium II as an assembly of actual atoms, "populated" it with new particles, which he showed to determine the state and behaviour of the whole liquid. These are the "quasi-particles" or "elementary excitations" – purely quantum constructs resulting from the laws and principles of quantum mechanics.

"Wave and stone . . . were not so different", says the line in Pushkin's *Eugene Onegin.* This poetic image, however, is intended to emphasize how different they are. The contrast of wave and particle, in science and in life, is a long-familiar one.

Only the creation of the quantum theory abolished the frontier. In quantum mechanics, they simply do not exist separately, as wave and particle; they are dialectically merged. A quantum object is a wave and a particle at the same time.

First of all, particles were extended to include waves. The first quantum object was light. This had to be regarded as having not only its inherent wave properties but also discontinuity, discreteness; that is, corpuscular or particle properties. The reason was complete desperation at the failure of all attempts to account for certain phenomena on classical principles.

Physics acquired a new concept, the "quantum" or "radiation quantum". Then the family of quanta began to grow and the radiation quantum was given the specific name of "photon", from the Greek *phōtos,* light, and *-on*, the ending of names of particles, as in "electron".

Radiation quanta, or photons, of course occur not only for visible light but for all electromagnetic vibrations, whatever the wavelength (or its reciprocal, the frequency).

Radiation, which by the laws of classical physics consists of vibrations, or electromagnetic waves, is regarded in quantum mechanics as a particular motion of photons. The electromagnetic vibrations may have any wavelength, depending on their origin. On the other hand, their behaviour, and in particular their effect on ourselves, depend on their "appearance", and therefore again on this wavelength or frequency. The higher the frequency, the more powerful is the radiation (other things

being equal); there is reason in the use of the terms "soft" (low-frequency) and "hard" X-rays.

Going from long to short waves, we have successively the radio waves, the infra-red, visible light, the ultra-violet, X-rays; and the γ-rays formed in the decay of atomic nuclei are also a flow of photons.

But the photons "belonging" to different parts of the electromagnetic spectrum are not at all the same. A photon is a lump or portion of energy, and the quantum energy increases as the wavelength decreases (towards high frequencies). The proportionality factor between energy and frequency is the famous h, Planck's constant, an important number (though very small in absolute value) and a sure sign of belonging to the extensive "quantum kingdom".

This kingdom was to be conquered by Planck's constant over many years – in the approaches to quantum mechanics, and afterwards as quantum mechanics started and evolved. The constant h itself was born in the last month of the last year of the nineteenth century, when Max Planck suggested that in certain processes radiation is emitted in portions or quanta. It was known that the energy of radiation increased with the frequency. Planck proposed that energy is proportional to frequency, and the proportionality factor is h.

Planck's formula cannot be deduced or proved. It expresses a given fact, one of the features of the behaviour of matter. Planck simply guessed this feature and the formula expressing it, just as Newton guessed the law of universal gravitation. If we say *brilliantly* guessed, the picture is complete. There are few such guesses in the history of science. "Today I made a discovery as important as Newton's," said Planck to his son. He said nothing like this outside his family, and was indeed an extremely reserved man. But Einstein said that Planck had put a large flea in the physicists' ear.

So the energy of a quantum is exactly proportional to its frequency. For example, an X-ray quantum is much more powerful than a light quantum, which in turn is stronger than an infra-red one. Thus every quantum and every particle

corresponds to a strictly defined point (which may be identified by either frequency or wavelength) in the electromagnetic spectrum.

A quarter of a century passed after the conquest of waves by particles. And then the waves unexpectedly succeeded in getting their own back, through a bold step taken by the French physicist Louis de Broglie. He announced (or speculated, one could equally well say) that not only does radiation have both wave and corpuscular properties, but also every particle of matter, even an electron, also has wave properties.

Just as Planck related the energy of a radiation quantum to its "wave characteristic", namely the frequency, so de Broglie related the corpuscular properties of the electron to its wavelength.

Taking as his starting-point Bohr's postulate that an electron revolving in a stationary orbit round a nucleus is stable and does not emit energy, de Broglie ascribed to the electron surprising properties. He viewed it as a wave, as a kind of oscillatory process. The wavelength of the electron thus "spread" over its orbit must have a definite value. The orbit must accommodate a whole number of waves. If this condition is satisfied, the orbit is stationary.

The rest was simple mathematics. From his condition and the Bohr postulate, de Broglie derived an expression for the wavelength of the electron, which proved to be proportional to Planck's constant and inversely proportional to the momentum of the electron, the product of its mass and its velocity.

De Broglie's great formula very soon ceased to be the property of electrons only. Any particle (proton, neutron, atom, molecule, nucleus, the "elementary particles") was treated as a particle–wave with its wavelength λ determined by its momentum.

(Two comments. One – we have already met the letter lambda, denoting the phase transition between helium I and helium II, but it has been used much longer to denote the wavelength. Nature, however, has so many phenomena and

processes that there simply are not enough letters in the alphabet. Quite often, therefore, the same letter stands for entirely different quantities. Physicists know which is which and do not get confused. The second comment is a very important one. If the de Broglie wavelength is much less than the dimensions relevant to a particular physical process, the quantum properties essentially do not appear, and the classical description is then valid; but as soon as λ becomes comparable with these dimensions, quantum mechanics takes over.)

Quantum mechanics has constructed a special mathematical formalism by which it describes the behaviour and interaction of such wave–particles. But, as we know, man cannot visualize these "centaurs" of the microworld, for the reason that there is nothing like them in the world that is visually perceived. On the other hand, we can easily imagine the mythical centaurs, even though they do not exist, because they are in fact creatures of our own imagination.

The microworld does exist, and so does this particle–wave duality, as the physicists call it. In some processes the wave properties are manifest, in others the corpuscular properties. But the thing itself, the same electron or photon, is neither one nor the other, but some third thing different from both wave and particle.

The photon was already fully established in our minds and our language, and not only in the work and vocabulary of physicists, when its younger brother the phonon came into existence. This is a quantum of sound. The need for it arose when physicists began to consider the pattern of certain processes that occur in solids, such as the thermal conduction and electrical resistance of metals.

The quantum approach, which revealed the essential nature of these processes, here began from the physicists' conception of a new kind of particles existing in a crystal. Each of these quantum particles corresponded to a certain wave process taking place in the crystal. As we know, the atoms in a solid execute thermal vibrations about their equilibrium positions, such as

the crystal lattice sites. Because the atoms are closely interdependent, their vibrations also are interrelated.

The assembly of such waves was identified with the assembly of "particles" propagated through the crystal lattice, each with a definite energy and momentum. By analogy with photons, they were called sound quanta or phonons.

Of course we cannot hear these sound vibrations, because their frequencies are quite different from the range of sound that is perceived by the ear. They are much higher. We should have to go more than thirty octaves above the middle of the keyboard in order to reach the range of thermal vibrations. Among all photons, too, the eye perceives only those belonging to the optical part of the spectrum, but the name "photon" applies to all quanta that are electromagnetic in nature. The same is true of phonons. They are all of the same kind, whatever their frequency, and all are propagated at the speed of sound.

Phonons were also called quasi-particles. Then the family of quasi-particles began to increase, but only through the arrival of new members. The quantum objects previously known, such as electrons and protons, are still called particles. This is not just a matter of custom or historical tradition. Particles may be regarded as individualists living by themselves or in a small group. Quasi-particles belong to an ensemble, a macro-object (and each kind of "particles" describes a definite process). It was to the quasi-particles that Landau turned when devising the theory of superfluidity of helium II.

Landau constructs the energy spectrum of helium II

So Landau's first step, first idea, was to replace a vast number of motions and complex interactions of all the atoms in a given volume of liquid helium by a small number of fairly elementary types of motion of quasi-particles.

Even though there are really no quasi-particles as actual objects that can be perceived, if only in thought, there are certain excited states of the whole liquid. If the energy of these

excitations is small, they may be regarded as a set of elementary excitations, i.e. a set of quasi-particles; and not just as this. Since helium II exists at very low temperatures, it contains few elementary excitations or quasi-particles, and the word "set" may perfectly well be replaced by "quasi-particle gas". In such a gas, the interaction of the particles will be extremely weak, and this makes the problem considerably easier and simplier.

But the quasi-particles differ from the "normal" particles not only in being unperceivable; there are other features, highly important, which must be borne in mind when constructing the theory. Indeed, in many ways they determine the theory.

These "quasi-particles" are well named in contrast to such particles as electrons, protons, neutrons, and atoms. This, as has been said, is not a matter of an accumulated tradition, but of a very important fact. In a closed volume, the number of particles, say atoms, remains constant, regardless of whether we, for example, heat or cool the substance. This is not the case for quasi-particles. The quasi-particle gas behaves quite differently from an ordinary gas, where the number of particles in a closed volume is constant, and only their energy varies with the temperature. When the temperature is raised, new quasi-particles appear. At zero temperature, they disappear altogether, and their number increases steadily with the temperature of the helium II.

This is not all. We know, for example, that any moving object, whether wave or particle, carries a definite energy and momentum. The quasi-particles are no exception to this. The dependence of their energy on their momentum is the determining mode of description of their "life" and behaviour.

There, however, the similarity ends. The energy of a quasi-particle may depend in a most fantastic way on its momentum. Yet this dependence is of primary importance. Each quasi-particle corresponds to a certain elementary excitation, that is, to a certain type of motion of the whole fluid. The nature of the relation between the quasi-particle energy and momentum therefore reflects the nature of this type of motion. The relation, a purely quantum one, is called the energy spectrum.

If we go back to the origins of the quantum theory, the reason for this name can be understood. The new physics was based on the idea that the absorption and emission of energy in various processes take place in strictly specified portions or quanta. This gave rise to the concepts of quantization of energy and the spectrum of allowed energy levels.

Thus the energy spectrum, which is the set of "permitted" values of the energy, is a principal characteristic of the state of a system obeying the laws of quantum mechanics.

But how to find the energy spectrum of helium II? Landau realized that one must first consider which quasi-particles are excited and present in helium, and then seek the form of the energy as a function of momentum for each type of quasi-particle.

Helium becomes a quantum liquid when the temperature falls and the thermal motion of the atoms is reduced to such an extent that the de Broglie wavelength corresponding to the thermal vibrations becomes large enough to be comparable with the interatomic distances. This occurs at about 2°K.

Landau began to construct his energy spectrum by going even further down the scale of temperature, in fact to absolute zero. Near zero, the thermal vibrations are much less still. Thus the de Broglie wavelength corresponding to a quasi-particle excited at such temperatures is much greater than the interatomic distances. By the laws of quantum mechanics, a long wavelength denotes a small momentum of the quasi-particle, since these are reciprocal quantities.

Landau assumed that these quasi-particles with long wavelengths and small momenta, present in helium II at very low temperatures, were just phonons, or quanta of sound. They are like the sound quanta excited in a crystal by the small thermal vibrations of the crystal atoms about their equilibrium positions. Landau also supposed, and later confirmed, that helium II did not and could not contain any other elementary excitations of very low energy, apart from the phonons.

It proved that Landau's speculation was the clew to the

labyrinth of explaining the superfluidity of helium. This is seen from the picture of the energy spectrum as Landau drew it.

Very approximately and purely verbally, that is without any strict treatment or calculation, simply following the line of Landau's thought, we can understand in the following way why helium must necessarily be superfluid.

First of all, we know how the energy of a phonon depends on its momentum: the energy is just proportional to the momentum. Thus the initial part of the energy spectrum is a straight line through the origin.

If you draw such a line, you will see that the angle between it and the abscissa axis is, so to speak, visible to the naked eye. Now this angle represents the ratio of the energy and the momentum of the quasi-particle. So, however close we come to zero at the origin, the ratio of the phonon energy and momentum can in no circumstances be less than a certain minimum value. Let us now move down to absolute zero. Then there are no quasi-particles in helium; it is entirely in the unexcited ground state. Even a single quasi-particle can be excited in it only under certain conditions. The point is, which conditions?

Imagine that we observe, say, the flow of helium in a capillary. A retardation of the flow, that is friction against the walls of the capillary or within the liquid itself, would mean that the kinetic energy of motion of the liquid was reduced.

On what can the energy of the motion be expended? On the excitation, the formation of a quasi-particle, which requires a certain quantity of energy.

Here, Landau found one fundamental relation: if the velocity of the helium is below a certain value, its energy and momentum are insufficient to excite a phonon with the necessary ratio of energy to momentum (as given by the initial part of the energy spectrum).

If energy is not expended on anything, it must be entirely conserved. We know that there are no "energy consumers" other than quasi-particles in helium II. There is in it no other analogue of friction or retardation. Hence, if helium moves sufficiently slowly, there will be no obstacle to its motion.

Nothing will retard it. And this is superfluidity.

It was the regular theoretical seminar. Landau was discussing the energy spectrum of helium II. He demonstrated the linear dependence of energy on momentum in the initial part of the spectrum, where phonons reign – sound quanta corresponding to "irrotational", or "potential", longitudinal vibrations of the liquid. He explained how the initial phonon part of the spectrum confirms the necessity for liquid helium II to be superfluid.

But, as we know, there are rotational as well as potential motions in a liquid. Hence, Landau said, as well as phonons in a quantum liquid, there can be elementary excitations that are rotational. They too relate to the behaviour of the liquid as a whole and must be quantized; that is, their energy has strictly defined values, not any arbitrary values.

Academician Igor Tamm, who was at the seminar, suggested that they might be called "rotons". In this way another type of quasi-particle in quantum physics acquired a name through Tamm's initiative.

For rotons, the dependence of energy on momentum is quite different from that for phonons, and their energy spectrum is therefore a complicated one.

Landau at first supposed that both phonons and rotons had their own energy spectrum, and that the overall spectrum of helium II consisted of two branches. Later, he reconsidered this idea.

An extremely important theoretical conclusion of Landau's was that not only phonons but also rotons cannot be excited at low flow rates of the liquid. Hence, although helium II contains quasi-particles of two types, under certain conditions the liquid is still superfluid – not completely so, as it is when at absolute zero, but quite perceptibly.

"Landau divined the form of the energy spectrum. To establish the energy spectrum of liquid helium simply from some general considerations and indirectly related experimental results was a triumph of scientific intuition and the power of scientific

imagination." So say the theoretical physicists. What lies behind these words?

Usually it is so. A scholar with the right qualifications, learned, concerned with serious modern knowledge, finds the right ways for its progress, and feels out the correct solutions to its difficulties. This is a natural process for a scholar in his own line, and his colleagues can observe and understand it.

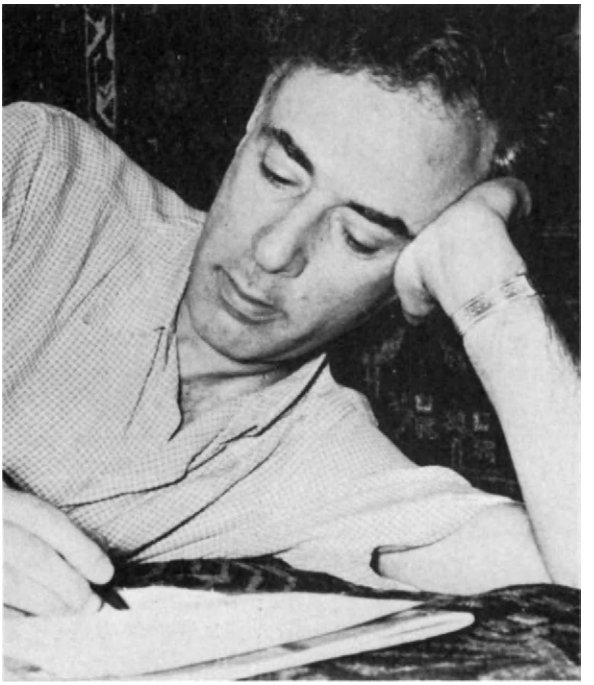
Much more rarely, we have the situation described by Mayakovsky: "Could you play through a nocturne on a flute of drainpipes?" Then, one speaks of intuition, enlightenment, the "sixth sense". Landau's discovery too was largely due to his "sixth sense": suddenly to divine how to construct the energy spectrum, where to make use of the theory, where to use the experimental results, and which ones.

The available theoretical arguments and calculations were evidently insufficient to construct the whole energy spectrum of helium II, to deduce such an unusual spectral curve. Yet it was a theoretical curve, which could not be derived from any purely experimental results.

It was therefore necessary to find, or to try to find, some appropriate combination of the two: to consider what can be drawn from theory and what from experiment, and to utilize the mutual assistance of these two tools of physics research. But how?

Landau had at his disposal, first, a series of paradoxes whose elucidation and explanation were unknown; second, a set of numbers relating principally to the thermal properties of helium II — results of measurements on which he could rely; third, some general considerations as to what was probable and admissible, and what certainly impossible. Most important of all, he had a special insight into physical processes, the special vision and understanding of them that is called the scientist's intuition.

Landau made use of measurements, mainly of thermal and thermodynamic quantities. He carefully studied all the values obtained in the experiments of the Keesoms, Kapitza, and others, and saw clearly which ones would be of use to him.



Landau at work

Moreover, with his "inner eye" – the keen mind of the theoretician – he saw that agreement between theory and experiment occurs only with the strange form of energy spectrum that he devised.

Perhaps it is impossible to find a single word that represents the mechanism of discovery. Found, constructed, guessed, calculated – each of these actions made its contribution to the creation of the energy spectrum. Therein lay the secret of creativity, which is not only hard to reveal to others, but incompletely known even to its possessor.

Such inspirations do happen in science, when fundamental and decisive relationships are perceived. "Einstein realized that *electrical* signals could not propagate faster than the speed of

light. He guessed that it was a general principle. . . . He guessed that it was true of everything, and he guessed that it would be true of gravitation," said Feynman. "Dirac discovered the correct laws for relativity quantum mechanics simply by guessing the equation. The method of guessing the equation seems to be a pretty effective way of guessing new laws."

Mozart was once asked how he composed music. He answered: "Sometimes, when composing mentally, I become more and more excited, and finally reach a state where I seem to hear the whole symphony from beginning to end, all in an instant! These are the happiest moments of my life."

There is an idea, which is quite justifiable and correct, that logic was the strongest constituent of Landau's unusual mental equipment, that his creativity was much more rational than emotional, that he surely never "became excited". Yet intuition did so imperiously enter into what was probably his most remarkable discovery.

How can we reconcile these seeming opposites? Perhaps Saint-Exupéry best reconciled the irreconcilable, in these words both sage and sly: "The theoretician believes in logic. He is convinced that he disregards reverie and intuition and poetry. He does not notice that these three fairies have donned masquerade garb in order to allure him, as if he were a lovesick lad of fifteen. He does not realize that his best discoveries are due to them. They have appeared before him in the guise of a 'working hypothesis', 'arbitrary conditions', or 'analogy'. How could he, a theoretician, suspect that, as he hearkens to them, he cheats his rigorous logic and is charmed by the Muses' song! Of course I admire Knowledge. But I also admire Wisdom!"

It might have been written specifically about Landau and this discovery.

Creativity. Scientific, artistic, . . . Looking at the stage, sitting in the concert-hall, we, the spectators, are present at the very process of creation — by the performers (the composer, the playwright, the producer have done their work previously and not in our sight). Most often, however, we make use of the ripe

fruits of creativity – looking at a picture, reading a book or article.

Nevertheless, any kind of creativity can in principle, and relatively simply, be viewed with detachment. The creativity of a mathematician or theoretical physicist, for example, is something quite different.

Yet, surprisingly, Landau's creativity, in some of its manifestations, was evident to those around him. Even for people close to him, friends, pupils, co-authors, this "creative" Landau gave an impression of the miraculous. Sometimes his mode of thought was quite evident. For instance, he might be given a problem, either complicated or from a field new to him, and everyone could watch how he "took off", to use the graphic expression of Alexander Kitaigorodsky. His eyes became fixed, and this extraordinary machine for calculation and decision began to function. What is usually an invisible process concealed from all, the process of thought, in his case became visible.

The work of a theoretician is by no means a spectacular enterprise. It is not like standing behind the artist and seeing how the strokes of the brush are applied to the canvas and the picture comes into existence, created before your eyes; or being present during experiments that can be described, often quite vividly: for example, how the curves on the oscillograph screen appear, break up, and flicker to and fro; or a spectrum flashes forth; or artificial lightning, a spark from a high-voltage generator, cuts through the air; or a plasma is ignited in some prototype of the thermonuclear reactor of the future. Many striking pictures can be seen when one is beside the experimenters in physics laboratories.

With the theoretician, there is nothing: no effects, no dramatic scenes, no beautiful sights; just the blackboard, chalk, and paper. The theoretician does not even have a laboratory; it would serve no purpose. And usually only his fellow theoreticians are capable of appreciating the content and significance of theoretical work, and also its august beauty.

Yet sometimes it may in a general way reveal its beauty even

to "mere mortals", provided that they are willing to make some effort, to expend a little energy. That beauty does not lie in external effects, but in the inward idea, often appearing as if by a miracle, and astonishing in its novelty and unusualness, in the logical turn of thought, in the surprising associations. Nothing can be more beautiful than human thought when it is new, stated for the first time, in some way paradoxical but also startling in its correctness, precision, and indisputable convincingness.

In this fashion, external visual impressions are replaced by a purely intellectual joy that can be so strong as to be distinctly sensed. Pushkin rightly said that to follow the thought of a great man is the most absorbing form of study.

The two-faced Janus, helium II

The surprising form of the energy spectrum both expressed and confirmed the surprising nature of the quantum liquid helium II, and in particular its superfluidity.

But the theoretical demonstration unavoidably raised a new problem: what is the physical mechanism of this strange process? What occurs, not on the graph, not according to the formulae, but in actual liquid helium?

The next step in Landau's theory was to answer this question. He did not attempt, as when aiming at the principal objective, to elucidate numerous riddles or the whole set of "eccentricities" of liquid helium; he set up a consistent theory; but in fact the paradoxes were in consequence resolved – a necessary, and a most welcome, consequence. What scholar is not gladdened by the brilliant resolution of what seemed unresolvable complexities, and by cogent proofs of the correctness of his idea?

In order to understand the further course of Landau's thought, let us recall how physicists were puzzled by the strange results of measuring the viscosity of helium II: not only its extremely small value, because of which Kapitza called helium a superfluid, but also the surprising fact that a different method

of measurement, for instance with a disc rotating in helium, showed the liquid to have a quite perceptible viscosity. This was the only case known to physicists where the two methods of measurement gave non-identical, and in fact quite different, values, not even of the same order of magnitude.

Landau now decided to investigate theoretically and calculate the behaviour of helium rotating in a vessel. In order to delineate (or perceive) initially in mathematical symbols the pattern of behaviour of the quantum liquid helium II, he imagined it to be rotating, and calculated all quantities appropriate to a rotating system. He considered the same gas of elementary excitations or quasi-particles (phonons and rotons) that characterizes the behaviour of helium II at temperatures a little above absolute zero. With this approach, as we know, the energy of helium may be taken as the sum of the energies of all the quasi-particles. Landau similarly assumed every other physical quantity to be equal to the sum of the values for the individual quasi-particles.

The first conclusion reached seemed quite reasonable: in a rotating vessel, the quasi-particle gas is carried along by the walls and rotates with the vessel.

Landau made a further calculation, and found that two quantities (the angular momentum and the moment of inertia), which usually go harmoniously together and vary in the same manner when the external conditions change, now suddenly diverged in an emphatic manner. This could only mean one thing: when the vessel rotates, there rotates with it not the *whole* mass of liquid within it; a part of the liquid is not caused to rotate.

The peculiarity of behaviour of helium II thus discovered by Landau was so unusual and surprising, even to its discoverer, that he found it necessary to describe in detail how he visualized the physical picture of the "life" of a quantum liquid.

"Thus we reach the fundamental result", he wrote, "that, when the vessel walls are in motion, only a part of the mass of liquid helium is carried along by them, while the other part remains at rest. One can therefore visualize liquid helium as

being a mixture of two liquids: one superfluid, without viscosity and not carried along by the vessel walls, the other normal, 'adhering' when it moves past the walls and behaving as a normal liquid. Here it is very important that there is no 'friction' between the two liquids as they move through each other, i.e. no transfer of momentum from one to the other. It must be emphasized that to regard helium as a 'mixture' of two liquids is no more than a convenient mode of expression for describing the phenomena that take place in helium II. Like any description of quantum phenomena in classical terms, it falls short of adequacy. In reality, we ought to say that in a quantum liquid such as helium II there can exist simultaneously two motions, each of which involves its own 'effective mass' (such that the sum of the two effective masses is equal to the actual total mass of the liquid). One of these motions is 'normal', i.e. has the same properties as the motion of an ordinary liquid; the other is 'superfluid'. The two motions occur without any transfer of momentum from one to the other. We must particularly stress that there is here no division of the actual liquid particles into 'superfluid' and 'normal' ones. We can, in a certain sense, speak of the 'superfluid' and 'normal' masses of liquid as being those related to the two simultaneously possible motions, but this does not mean that the liquid can actually be divided into two such parts. Bearing in mind all these reservations as to the true nature of the phenomena taking place in helium II, it is nevertheless convenient to use the terms 'superfluid' and 'normal' liquids as a concise description of these phenomena, and this we shall do."

So there is actually no mixture of liquids, there are not two liquids, the mass of helium is not separated into two parts. There is one liquid, homogeneous throughout, and as a whole it has this strange behaviour.

Can a man, say, or a locomotive, stand still and move at the same time? Obviously not. But helium II can stand still and move at the same time, can remain at rest and rotate.

Landau always realized how startlingly implausible this picture

must appear. He himself viewed it with some surprise – perhaps with the glad surprise of the true scientist to whom fate has granted the good fortune of encountering a new natural phenomenon, and who has not overlooked this phenomenon nor been afraid to record it, but rather has penetrated into it and finally revealed its secret.

Nevertheless, the surprise remained, and Landau sought to share it with his audience: "From the existence of these two masses of helium, the normal mass and the remaining 'superfluid' mass, there follows another result, equally monstrous at first sight: helium can execute two motions at the same time. Having two masses in the same place, in the same space, it can execute simultaneously two different motions at the same point in the liquid."

A quantum effect – we know that such was Landau's explanation. The effect is quite new and unlike others, or if like them, only in its unlikeness to the "normal world".

As soon as we come into the realm of quantum mechanics, the words "unimaginable", "cannot be pictured", "contradicts our senses", "does not fit with our ideas", and so on, will be our constant companions. Those who are concerned with the new physics are used to it. But even then. . . . They say that, during discussion in Copenhagen, one of the physicists remarked that merely thinking about these problems made his head begin to whirl. Bohr immediately replied, "If anyone says that he can think about problems of quantum mechanics without his head whirling, it only shows that he understands absolutely nothing about them."

However, the physicists had already realized that any description in words of quantum objects and quantum behaviour is bound to be inexact or, in technical terminology, inadequate, simply because words were made in the world familiar to us and are themselves associated with familiar ideas.

Only formulae are precise and adequate, only a mathematical description using the formalism of quantum mechanics that was specially devised in order to describe the behaviour of quantum objects.

"Physicists cannot make a conversion to any other language," said Feynman. "If you want to learn about nature, to appreciate nature, it is necessary to understand the language that she speaks in."

Landau, on the same subject, was fond of quoting the aphorism of Kozma Prutkov:* "Without knowing the rules of the Choctaw language, can you reach any opinion about it which is not baseless and foolish?" (We have now learnt a little about the "Choctaw language" of quantum mechanics; of course not enough to "reach opinions about it", but enough to understand it to some extent.)

Thus, in describing the phenomena of the quantum world in words only, and not in formulae, we are continually representing what, strictly speaking, does not really exist, because we have no suitable words for what does exist, and we never can have.

All this is entirely applicable to the "split personality" of liquid helium. The astonishing creation depicted by the pen of Landau is a product of the human mind. Helium II is not really like that; but not because something too complex and improbable has been devised that Nature could never create. Quite the opposite: because Landau's model is a simplified representation, in conformity with our ideas, our way of thinking, our classical view and language, of the real phenomenon, the quantum liquid helium II.

It is therefore natural that Landau laid stress in his theory on the strictly mathematical description of both the object itself and its behaviour, this description being not simplified but exact. The verbal delineation of helium and of all that occurs in it was given the (quite important) role of providing a convenient means to assist the exposition of a situation new in physics, as Landau himself constantly remarked.

Here it is appropriate to emphasize one very important circumstance. We must remember that helium, on passing below the λ-point, changes from an ordinary liquid into an unusual one that has two different aspects and lives simultaneously in,

*The pseudonym of a group of Russian writers in the 1860s. – *Translator.*

as it were, two different and entirely dissimilar worlds. On the one hand, it remains a liquid visible to the naked eye (we ignore for the present the strange features of its behaviour). On the other hand, it is a quantum object, obeying the laws of quantum mechanics. This combination is itself unfamiliar, because we are all accustomed to thinking that only representatives of the microworld obey the laws of quantum mechanics.

The "familiarity effect" is an important one. Words, statements, concepts, ideas, that have been heard many times ultimately lose their unusualness and paradoxicalness, even if they originally had these properties to a high degree.

Over a long time, physicists, and then all who were interested in the structure of Nature, gradually became accustomed to the idea that light is not only waves, but also a flow of particles, of quanta, and that atoms and electrons are not tiny spheres: all of them, like werewolves, in some processes (including laboratory experiments) behave partly like waves, partly like discrete particles. And the quasi-particles that have recently established themselves in physics are, by definition, not particles but excitations, motions of the whole crystal or the whole liquid, yet behave in many respects like true elementary particles. They are indeed something that gives rise to particular properties, which to our human sight cannot take any familiar material form. For that reason, we cannot imagine such a thing. All that is possible is to repeat the enraptured words of Landau, that vigorous opponent of elevated language, on "the acute realization of the power of the human genius, whose greatest triumph is that man is capable of apprehending things beyond the pale of his imagination".

But all this applies, once again, to the particle–wave dualism, which is already by now familiar not only to physicists but also to the schoolchildren of today and all who find pleasure in popular scientific literature. Or it applies to relativity theory, about which Landau was principally speaking in his comment just quoted (and about which also many popular books and articles have been written). Moreover, in the one case the objects under discussion are too small ever to be seen, and only the

traces of their "activity" can be observed, for example as tracks in a cloud chamber or as flashes of light, scintillations. The other is concerned with immense distances, the space of the Universe, stretching beyond the limits of what can be seen with the most powerful telescope.

This combination, unimaginable and invisible, assists the psychological acceptance of new and very complex ideas. It is much easier to accept something that we cannot imagine and do not see and cannot ever see, than to look at a giraffe and be sure that "there ain't no such animal".

Landau shows us just this kind of marvel. Helium is poured into a dewar and cooled below 2.19°K, thus changing from an ordinary liquid into helium II. It is in fact difficult to see, without some practice. The liquid is completely transparent and still, and the viewing port in the walls of the vessels is small (so that the inflow of heat to the helium II is minimized, and the liquid is insulated from the exterior as far as possible). But with practice it can be discerned, and there are also other ways of verifying that the dewar is filled with liquid.

Landau points to the dewar and says (and afterwards proves) that this macro-object (not micro), visible to the unaided eye, has a physical nature quite different from all other liquids – is a monstrosity.

Its behaviour can be described only by quantum laws and quantum formulae. But again, not in the usual way. Here, the formulae of quantum mechanics describe the behaviour not of microparticles, not of the atoms that make up helium II, but of a whole macro-object, the whole mass of helium that we can see.

This mass is a liquid, normal and abnormal at the same time. It is not that some of its atoms are normal and behave ordinarily, for example, transfer heat, have viscosity, and so on, while the others are not normal and stubbornly refuse to perform these functions appropriate to a macro-object. No, the whole of the helium is simultaneously both the normal and the superfluid component. The whole of the helium takes part simultaneously in two motions, normal and superfluid. And if the normal component rotates with the rotating vessel and the superfluid

component is at rest, then this means that the whole of the helium is simultaneously both moving and not moving.

In thinking how best to describe such a motion, one comes across another strange and unimaginable process or picture: the motion of a liquid through itself. Without any friction or resistance, without the slightest hindrance, this relative motion takes place. The whole liquid may remain at rest. Or, as one sees in experiments, it may take part in rotation, flow in a jet from a vessel, pass through a slit. We see that the whole liquid behaves in this way. But it is in fact not so: the two different simultaneous motions take place in it at all times. From a variety of processes that can be observed and measured, we can conclude that this paradoxical phenomenon does indeed exist. Moreover, it alone can account for all the strange results found in experiments, which so astonished and baffled the experimenters, and Kapitza too.

So the superfluid component, or superfluid mass, corresponds to the motion of the liquid without friction or resistance. The normal one corresponds to normal flow with an entirely perceptible and measurable viscosity. Now viscosity occurs when quasi-particles are present in helium. In other words, the superfluid component is helium without quasi-particles, and the normal component is an assemblage of phonons and rotons.

Let us now traverse the temperature range from absolute zero to the λ-point, where helium changes from a quantum liquid to an ordinary one. We know that the number of quasi-particles, unlike that of ordinary particles, is not constant, but varies with the temperature. Let us see how it varies and how the behaviour of the helium is affected.

At zero degrees there are no quasi-particles, and the whole of the helium is a single superfluid mass. As the temperature rises, quasi-particles are formed by the appearance of elementary excitations: first phonons, then rotons. This is the normal component, an assemblage of phonons and rotons moving in the liquid.

Although we must always remember that there is no actual

separation of the liquid into components, and each of the two states pertains to the whole liquid, being simultaneously present in it, for a theoretical description it is more convenient to refer to two masses or two densities of the liquid, normal and superfluid. We can then say that at zero degrees the ratio of the mass (or density) of the normal component to the total mass is zero, and at the λ-point it is unity.

The same liquid, homogeneous throughout, and yet this strange transfer of mass from one component to the other!

Here is what Landau wrote in his first paper on the subject. "The most important parameter which determines the properties of helium at any given temperature is the ratio of the masses of the superfluid and normal parts of the liquid. We define the density ρ_n of the normal liquid and ρ_s of the superfluid; the sum $\rho_s + \rho_n = \rho$ is the total true density of the liquid. [Density in physics is usually denoted by the Greek letter rho, ρ — A.L.] At absolute zero, the ratio ρ_n/ρ is zero. As the temperature rises, this ratio increases until it becomes equal to unity, after which, of course, it will remain constant. The temperature at which ρ_n/ρ becomes unity is the point of transition from helium II to helium I. In this way the phase transition in helium is connected with the disappearance of the superfluid part of the liquid. This disappearance takes place gradually, i.e. ρ_n/ρ becomes unity continuously, without a jump. The transition is therefore a second-order phase transition, i.e. a λ-point (not accompanied by the evolution or absorption of latent heat). The presence of a discontinuity in the specific heat is a direct thermodynamic consequence of a second-order phase transition."

It is indeed pleasant to read the authentic words of the author of such a discovery and now to understand their content. For they are in a language that is no longer completely strange and unfamiliar.

Most surprising of all . . .

It is difficult to exhaust the unexpected aspects that helium

presents to physicists. Perhaps the most surprising of all, however, is the relation between helium and heat.

"I remember", Landau said, "a very old story about a theory that was once part of physics, the theory of caloric, a fluid which of course never existed. It was thought that as well as ordinary fluids there was also a thermal fluid, that if a body is hot it contains more caloric. If there is less caloric, the body correspondingly becomes colder: that is, caloric was a fluid invented specially to explain these phenomena. Experiments showed that there is no thermal fluid, and that heat is the motion of fluid particles. We find that helium has retained something of the nature of caloric, of course in a very particular sense."

It is strange that at some stage in the development of science ancient errors or incorrect ideas are transformed into the truths and laws of the physical world. There has been more than one such metamorphosis. How is this? Chance coincidence – or vague, strangely transfigured presentiments – who knows? . . . The philosopher's stone, with which metals and other substances were to be transformed into gold, was the basis of the pseudoscience of alchemy. Yet the radioactive transformation was discovered at the end of the nineteenth century, and the bombardment of atomic nuclei yielded artificial radioactivity in the 1930s. The transformation of elements is now a scientific commonplace.

Caloric's turn came. Historically, it came into the field later than other errors, not until the end of the eighteenth century, and its "transformation" occurred very recently too, in Landau's theory of quantum liquids.

Caloric is a fictitious imponderable fluid existing independently of a given body or fluid, a substance that can be supplied to or removed from an actual fluid, thus making it respectively hotter or colder than it was before.

This idea suggested to Landau the use of an analogy with caloric to explain the strange relation between heat and helium II. He did this, of course, only in popular lectures, not in scientific papers. All such examples – the philosopher's stone,

perpetual motion, phlogiston, and so forth – are used only in historical or popularized scientific discourse.

Yet the analogy is a fine one. Helium II itself is a kind of unification of two liquids with totally different properties: the normal and the superfluid. We have seen how sharply they differ in their "interrelations" with viscosity. We shall now see how they behave in relation to heat. The role of caloric is taken by the gas of quasi-particles, phonons and rotons. These are related to energy, in fact to heat. Externally, without consideration of the physical essence of the phenomenon, the quantity of quasi-particles, which is the normal mass of helium, is, like caloric, directly associated with the temperature, in contrast to the superfluid mass, which has no connection with heat.

What difference does one degree make to an ordinary liquid? None at all (unless, of course, it is at the temperature of some transformation). One degree more or less makes hardly any perceptible difference.

But here it is not one degree (helium II has barely more than two degrees left) but every tenth, hundredth, thousandth of a degree that alters the liquid itself. It becomes in a way like the river into which, the ancients said, you cannot step twice. With every fraction of a degree the quantity of quasi-particles changes, and therefore so does the proportion of the normal and superfluid components. Thus the liquid not only becomes a little hotter or a little colder than before, but is in fact different.

At absolute zero, there are no quasi-particles, no "caloric", and the whole liquid is superfluid. As the temperature rises, so does the quantity of quasi-particles, and the superfluid mass gradually gives place to the normal mass. At the transition point from helium II to helium I, the superfluid component leaves the field. The whole liquid becomes normal, truly normal in all respects and properties.

Let us move away from the two extremes of zero and the λ-point, and choose some – any – temperature in this range, and look again at what happens in helium II. Since the superfluid motion is not accompanied by any transfer of heat, all the heat being transferred by the normal component, we can repeat

the words of one physicist: "In a certain sense, one may say that it is heat itself, which thus becomes independent in liquid helium, detached from the mass of liquid and able to move against a 'background' that is at absolute zero. It is worth while thinking about this picture, in order to appreciate how fundamentally it deviates from the ordinary idea of heat as a random motion of the atoms of a substance, inseparable from its whole mass."

One cannot look at such a picture, of course, but one can think about it, reflect upon it, and understand that is is not only astonishing but also beautiful. Above all, it provides the key to the behaviour of helium. It explains the paradoxes; it reveals the secrets. More precisely, by means of the two-component model, it explains everything to do with the strange "thermal effects" in helium II. The model itself allows an understanding of everything else related to the actual motion of helium and in helium, in particular the surprising scenario resulting from superfluidity.

Theoretician and experimenters

Having set up his theory, created a dialect of the language of quantum mechanics to describe a quantum liquid (a phenomenon previously unknown to physicists), Landau was able to express in that language what actually happens in helium II, how all its strange features change into an entirely regular, necessary, indeed the only possible, behaviour.

The key to the elucidation of the experimental wonders can be found from Landau's words in his lecture, summarizing the essence of his discovery and assessing that essence with much feeling: "The theory shows that the two motions of helium II must have quite different properties. The normal motion, associated with heat, is normal in every sense. It has all the properties of any normal motion, and in particular it involves viscosity. The superfluid motion, on the other hand, involves no viscosity. At first sight, this idea seems almost absurd. It may appear a fairly senseless argument, which, if it explains something,

does so only verbally, with no real result. But it is not so. The two motions which I mentioned and whose existence seems so strange can be observed directly in experiments."

Landau went on to describe the experiments. When Kapitza measured the viscosity of helium II from its flow through narrow slits, why did he find almost zero values – superfluidity – whereas, when the viscosity was determined from the friction of a cylinder rotating in helium, the values obtained were entirely measurable? The two results are always the same, for all liquids.

This, Landau replied, was because the two methods actually measure the viscosity of different components of helium II. The superfluid part of the helium flowed through Kapitza's slit, while the normal component, having viscosity, was able to pass through such a narrow slit only very slowly. This is why Kapitza discovered superfluidity in his experiments: the superfluid component actually has no viscosity.

The measurements with the cylinder are different. As it rotates in helium, it undergoes friction against the normal part of the liquid, which slows down its rotation. In this way, the viscosity of the normal component of helium II is measured.

The fact that essentially only the superfluid component can pass through narrow slits, whereas the normal component is almost incapable of doing so, enabled Landau to unravel the secret of one puzzling effect that Kapitza had observed. "This explains", he said, "the famous thermo-mechanical effect, in which the helium is cooled in the vessel which it enters through a narrow slit, and is heated in the one which it leaves. In superfluid motion, helium leaves without any heat. The vessel which it enters therefore has the same amount of heat but more helium, which is consequently cooled. In the vessel which the helium has left, there is the same heat and less helium. Naturally, the helium in that vessel becomes hotter."

Several other paradoxes found in Kapitza's experiments were to be explained. When helium is heated in a bulblet, why does a jet of liquid flow from the capillary, able to deflect a vane? and why does the bulblet not become empty, however long the jet flows?

"Let us take this remarkable experiment of Kapitza's with the jet of liquid helium," said Landau. "From the standpoint of superfluidity theory, the position is clear. The heating of liquid helium does not take place in the usual manner. Usually, heat passes from molecule to molecule without any general motion. In liquid helium, heating causes two simultaneous motions."

We have learnt what are the two motions to which Landau referred. One is normal, the other superfluid. And we can understand the process that has occurred. When helium is heated in the vessel, which like the vane and its suspension is immersed in liquid helium, a jet flows from the capillary and deflects the vane. This is a jet of the normal component, which carries heat, and which, like any normal viscous liquid, presses on the vane in the path of its motion. As long as the jet is flowing, a flow of the superfluid component passes into the vessel from the surrounding helium along the same capillary, in the opposite direction to the jet and, so to speak, "through" it. The superfluid mass of helium has no effect on the vane, and flows indifferently past it. This is why only the jet of the normal component is observed. But, as we know, the two components are equal in mass. This is the reason for the remarkable finding that the jet flows but the bulblet does not become empty. At every instant the same amount of liquid enters as leaves; the normal and superfluid flows exactly balance as to the mass of material transferred. It is more accurate to say that there is no overall movement of the liquid, and so the actual quantity of helium in the vessel remains constant.

"This fact explains also the enormous thermal conductivity of helium, its ability to transfer a vast amount of heat," said Landau. "In an ordinary liquid, where heat is transferred by molecular motion from one molecule to another, it is transferred slowly. In helium, there are two flows in opposite directions. The normal liquid flows from the hot end to the cold end, carrying heat; this heat is more than enough to explain the observed large amounts of heat transfer. In the opposite direction there is a flow of the superfluid: the two flows exactly balance as regards mass transfer, and so there is no actual macroscopic flow in helium."

It is worth remarking in addition to Landau's comments, and indeed emphasizing, that in this way physicists discovered and explained an entirely new phenomenon in macrophysics (which had seemed to be already thoroughly investigated): a new form of heat transfer, not ordinary thermal conduction, and not convective transfer, though partaking to some extent of the nature of both. Like classical conduction, the new form of heat transfer was not accompanied by a motion of the whole mass of the liquid. On the other hand, the normal component was involved in something resembling convection.

The final result was the very large value which had once led Keesom to call the process "thermal superconductivity", and Kapitza to suspect the presence of some concealed factor worthy of attention.

In this way, an exceptionally beautiful picture of the phenomenon was obtained, with all details harmoniously incorporated. Physicists, it should be said, are very sensitive to the aesthetic aspect of discovery, to the form with which it is invested by its author. One even hears it said that the form may serve as a further criterion of the correctness of the theory. In any event, the beauty of the work is particularly esteemed.

However, in contemplating the picture delineated above, and in learning more of it – a pleasure that we may now enjoy – we must still not forget that the actual physical processes occurring in helium are much too unusual to be capable of visualization or purely verbal description. There is no real separation of the quantum liquid helium II into two components. It is one liquid behaving in such a way as to seem two. The true – unimaginable – picture, a purely theoretical creation, is still more beautiful and harmonious.

Besides the verbal qualitative explanation of the phenomenon of helium II, quantitative properties can also be deduced from Landau's theory (and this is an indispensable requirement for any theory).

Landau, as has been said, did make use of certain experimental

results to construct the energy spectrum. Now, from the form and features of that spectrum, he calculated the thermodynamic quantities for helium II, obtaining excellent agreement with their measured values.

He also determined some non-thermal properties of liquid helium. The quantitative relation between the superfluid and normal masses was extremely important with respect to the theory. The fact that the whole of the helium is superfluid at absolute zero and normal at the transition point (2.19°K) did not reveal the dynamics of the process. It was necessary to establish how the relative masses of superfluid and normal helium vary with temperature in the range where helium II exists.

Landau made this calculation, and obtained the curve of temperature dependence of the ratio ρ_n/ρ mentioned previously. Experimentally, however, the result was not yet confirmed: no one had made such measurements.

After the war, Elevter Andronikashvili came to Moscow from Tbilisi in Georgia to work for a higher doctorate at the Institute of Physical Problems. He was greatly attracted to the subject of helium II, and decided to determine experimentally the relative concentration of the normal and superfluid components at various temperatures. This became the subject of his thesis.

But how should it be determined? The answer – a matter of principle as well as of technique – did not come quickly. The starting-point for the search was the situation already described which occurs when helium rotates.

"Imagine", said Landau later, "that a cylindrical vessel containing helium begins to rotate very slowly, so that the liquid must be carried along by the movement of the vessel walls. Since liquid helium is capable of two motions and its mass is made up of two parts, only one of these, the normal mass, will be carried along. The superfluid motion, which involves no viscosity, does not interact with the walls and is not carried along. When helium rotates, only part of it does so, whereas when any other liquid rotates the whole of it does so."

In this sort of way Landau formulated the principle of the experiments he devised, which Andronikashvili was to carry out. But how to determine what quantity of helium is at rest and what is rotating with the vessel?

Long reflection, calculation, and trial finally led Andronikashvili to construct an apparatus with which he was able to solve the problem. The properties of helium, in his words, "impose exceptionally stringent requirements on the construction of the apparatus, which is literally a skeleton".

Andronikashvili replaced the rotation of a vessel by the torsional oscillations of a stack of parallel metal discs. There were 100 discs of aluminium foil, each about 10 microns thick and 0.02 cm apart. The measurements were made over the temperature range from the transition point down to about 1.3°K.

When the stack rotated, the superfluid component passed easily between the discs, but the quasi-particles "stuck" to them, and were therefore carried along by the stack in its rotation. They then of course increased the total rotating mass, and so reduced the period of oscillation.

When the temperature fell, the quasi-particles forming the normal component became fewer, and the oscillations became faster. In this way, Andronikashvili obtained a quantitative relation between the ratio of normal and superfluid components and the temperature of helium II.

These experiments, unlike those of Kapitza, were performed to test the theory after it had been constructed. Landau was naturally much interested in their progress and results, and he became a frequent visitor to Andronikashvili's laboratory.

Together, they worked out the details of new experiments. Landau elucidated the complications that arose from time to time, and the results that seemed to the experimenters unexpected and strange. The staff enjoyed these visits. When Landau was there, everything was exciting and merry. Work alternated with witty repartee. There was banter at Landau's expense too, mainly on the subject of what would happen if he were allowed to get at the apparatus. He took these jokes affably.

In return, he had his "range of weapons", usually in response to ideas and suggestions that he regarded as incorrect – "pathology, nonsense, rubbish, bosh" and even more scathing expressions vigorously uttered.

Andronikashvili's refined experiments gave excellent agreement between the measured and calculated values, and thus a full confirmation of the theory.

"Remarkable results were found by Elevter Andronikashvili in his doctorate research," Landau said later. "The experiments showed that above 2.19°K all the helium is carried along in the rotation, but below this temperature only a part of it, which becomes smaller with decreasing temperature. Thus he was able to measure directly the part of the mass of helium that is normal and the part that is superfluid. The superfluid motion is not a theoretical fiction; it is something actually observed in the experiment. The quantitative results were also in excellent agreement with the theory. Thus Andronikashvili's experiment made it clear that the basis of the theory of liquid helium, however strange, does agree with reality."

In 1978, when physicists were commemorating the seventieth anniversary of Landau's birth, liquid helium could celebrate two anniversaries: the seventieth of its existence, and the fortieth of Kapitza's discovery of superfluidity. It was also the year in which the Nobel prize was awarded to Kapitza, "for his basic inventions and discoveries in the area of low temperature physics".

As we have seen, the discovery of superfluidity and other puzzling properties of helium II called forth Landau's fundamental theory which revealed the nature of a quantum liquid. The agreement of the theory both with Kapitza's entire range of experiments and with later ones by his pupils was impressively complete. This success was undoubtedly assisted by the close and continual association of the experimenters with Landau the theoretician.

Generally, the interaction and coexistence of theoretical and experimental physics is traceable in some form in all the great discoveries. But this is nevertheless quite a subtle matter, and

one that is sometimes not clearly understood. The interrelations of theory and experiment are usually evaluated differently by the exponents of these two methods of learning about Nature — as is probably to be expected. "Everyone thinks that his own work is the most important" was Kapitza's favourite aphorism. This he developed into "Most of the leading British scientists tend to attach the primary importance to experiment, regarding theory only as an auxiliary tool. More than 140 years ago, Davy said that one good experiment was worth more than the ingenuity of a brain like Newton's. This is often repeated even today. Such modern scientists as J. J. Thomson and Rutherford were fond of quoting it. It is to be treated as an exaggeration, of course, as a slogan of protest against the idolization of theory." Rutherford once said of theoreticians, "They play games with their symbols, but we, in the Cavendish, turn out the real solid facts of Nature." Actually, in the "offshore school" (to which Kapitza too belonged) in Britain, they did think this, in contrast to the "Continental schools" of Bohr, Pauli, and others.

Einstein said, "The scientist has to worm these general principles out of Nature by perceiving certain general features, which permit of precise formulation, in large complexes of experimental facts."

This is the role and destiny of the theoretician. But the great physicist had many times found how difficult the road is, and for that reason Einstein remarked with a wise smile: "We cannot envy the theoretical investigator of Nature. His labour is judged by an inexorable and not very friendly judge called Nature, or rather Experiment, who never says 'yes' to a theory, but at best 'perhaps', and usually just 'no'. When experiment and theory agree, it is 'perhaps'; when they do not, the sentence is 'no'."

Physicists, and Landau among them, have been fond of quoting Bohr's comment that agreement between theory and experiment means nothing, because among an infinity (the mathematical term "continuum" is used) of foolish theories there will always be some that agree with experiment.

Even so, there has always been an approval of theories by their self-confirmation in predicting new and unsuspected

phenomena which were afterwards indeed found in the place indicated and illuminated by the theory.

This is what Landau had in mind when he said in a public lecture: "Theory has not only explained the phenomena that I mentioned, which is never a fully adequate criterion of its correctness, but has also predicted various phenomena, all of which were subsequently discovered in experiments."

The real triumph of Landau's theory was the prediction of "second sound".

Second sound

What is sound, and what are sound waves? We open any encyclopaedia. "Sound is a vibratory motion, propagated as waves, of the particles of an elastic medium such as air or water. . . . The physical idea of sound includes both audible and inaudible vibrations of elastic media. . . . The sources of sound are bodies or systems of bodies whose motion relative to the surrounding medium brings it out of equilibrium."

That is the standard form. But, as we have many times found, laws are not written for helium II.

Using his theory as the basis for a mathematical picture of the motion of liquid helium as a whole mass, Landau again obtained a surprising result. The equations unequivocally state that in helium II, besides ordinary sound with its usual features, there should be propagated another very *un*usual type of sound. The chief difference shown by the equations is in the value of the velocity, and even more in its temperature dependence.

Whereas ordinary sound in helium, as in other liquids, is almost independent of the temperature, the other kind, known as second sound, does depend on it, in a very marked and interesting way. At and near absolute zero, the velocity of second sound is less than that of ordinary (first) sound by a factor of about 1½. It then falls rapidly to an almost constant value, becoming zero at the λ-point.

When Landau's theoretical prediction became known at the Institute of Physical Problems, attention immediately became riveted on this new phenomenon. There was a natural desire to test the theory as soon as possible, and to detect second sound experimentally.

The Leningrad acoustician Sergey Sokolov became the leading enthusiast. He suggested to Kapitza the idea of trying to measure second sound with an extremely sensitive instrument for acoustic measurements which he possessed. Kapitza asked Shalnikov to help Sokolov. They put the apparatus together, and began their experiments at the end of May 1941.

Sound vibrations were excited in helium by means of a vibrating piezoelectric quartz plate. They were detected by a receiver of similar material. It was expected that this would record two signals, one corresponding to ordinary sound propagated at the usual velocity, and the other to the much slower propagation of second sound.

The measurements continued until the Institute was evacuated to Kazan in the summer of 1941, but gave no positive results. Second sound was not discovered. The negative result was inexplicable. The reason seemed to lie in some fundamental point, and the strategy of search had to be changed.

The mystery continued to exercise the Institute's physicists, both experimenters and theoreticians, from Landau downwards. In 1944, Evgeny Lifshitz had the task of tackling this problem and resolving the question of second sound.

Looking at the situation from the theoretical standpoint, Lifshitz fully exposed the peculiar physical nature of second sound, and thereby explained the negative results of Sokolov and Shalnikov's experiments. At the same time, ways were opened for the experimental observation of second sound. Later, when the whole series of investigations was completed, Lifshitz explained the essentials of the phenomenon as follows: "It is well known that sound waves in an ordinary liquid are a process of periodic compressions and rarefactions, propagated through the medium. Every particle in the liquid executes an

oscillatory motion, moving about its mean equilibrium position with a periodically varying velocity. We know that in helium II there can be two simultaneous motions with different velocities. This gives rise to two different possible motions in the sound wave. If the two components of the liquid execute an oscillatory motion in the same direction, moving together, we have a sound wave of the same type as in an ordinary liquid. But there is also another possibility peculiar to helium II: the two components can oscillate in opposite directions, moving contrary, one through the other, so that the masses transferred in each direction are almost exactly equal. In such a wave, which is second sound, there are almost no compressions and rarefactions of the liquid as such. The vibrations of a membrane causing periodic compressions and rarefactions of a liquid will therefore lead in practice only to the generation of ordinary sound. This is the reason for the negative result of the experiment; the intensity of second sound was too small to be detected. Another conclusion now follows. The mutual oscillations of the normal and superfluid components are essentially oscillations of heat against the 'superfluid background' and must lead in the first place to periodic oscillations of the temperature of the liquid. It is therefore evident that such a 'thermal wave' must be most strongly emitted by a heater whose temperature varies periodically."

Thus it was found that the negative results of Sokolov and Shalnikov's experiments were not only correct, but the only possible ones. "We had the luck not to detect what was undetectable," Shalnikov later recalled. "It does sometimes happen to experimenters."

With a precise guiding idea after the work of Lifshitz, Vasily Peshkov, then one of Kapitza's graduate students, undertook the measurement of second sound.

Since second sound consists of oscillations of heat, not of density, it has to be obtained by exciting thermal oscillations in helium, which are then propagated through the liquid like a sound wave. Peshkov conducted a series of such experiments.

Heat oscillations were excited by supplying alternating current from an audio oscillator to a heater immersed in helium.

As we know, the quantity of heat in helium II is related to the amount of the normal component, i.e. to the quantity of quasi-particles. The number of these is in turn directly related to the temperature of the helium. The heat oscillations therefore appear to the experimenter as oscillations of temperature. By making the thermometer "travel" towards and away from the heater with periodically varying temperature, Peshkov clearly recorded the periodic oscillations of temperature in the liquid itself, that is, the existence of second sound.

The quantitative results were, of course, exceptionally important here, because Landau's theory and calculations gave a very unusual temperature dependence of the velocity of second sound, as well as its value for any particular temperature.

The results of the test of his prediction could not fail to arouse Landau's enthusiasm. He became as regular a visitor to Peshkov's laboratory as he was afterwards to Andronikashvili's. Again, his colleagues recall, his presence always brought animation, gave a new turn to thought, generated unexpected ideas. Here too there was the time-honoured exchange of repartee and jokes. Someone was bound to ask, "Dau, do you know a hammer from a soldering iron?"

Soon after Peshkov had completed his series of studies, Landau briefly expounded the essence of the matter in a popular lecture: "In liquid helium, unlike an ordinary liquid, two different kinds of sound can be propagated. Sound is an oscillation of the density of the liquid. Theory shows that in helium, as well as this, a special kind of sound can be propagated, because of the possibility of two motions. In helium, there can be sound in which there is no net mass transfer, but a mutual relative oscillation of the normal and superfluid parts. The part that contains heat oscillates in relation to the rest of the helium. This sound is known as second sound and was discovered by Peshkov, who observed its propagation in helium II. The propagation of second sound is easily distinguished from that of ordinary sound,

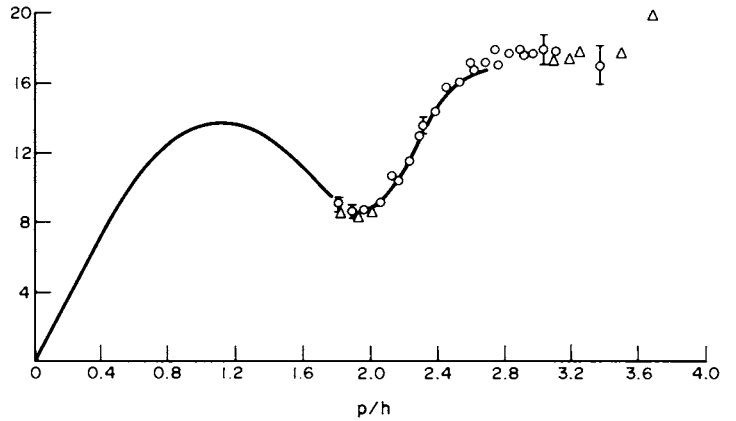
because the velocities of propagation are quite different, 240 metres per second for ordinary sound, 20 metres per second for second sound. Peshkov found that a special kind of sound is in fact propagated in helium. It is an oscillation of heat. If temperature oscillations are caused in an ordinary liquid, they rapidly die away. No second sound occurs. If the temperature is made to oscillate in liquid helium, however, the oscillation is propagated like sound with a definite velocity of about 20 metres per second. So this theoretically predicted phenomenon too has been experimentally observed."

In reality, however, the whole situation was rather more complex and interesting. Here is what Landau wrote in one of his scientific papers: "The velocity of 'second sound' in helium II has been measured by Vasily Peshkov with great precision. His results give an opportunity to perform a quantitative comparison of the theory developed by the author with experiment. Such a comparison gives full support to the general picture given by the theory, but at the same time it reveals a noticeable discrepancy between the calculated and observed values of the velocity (e.g. 25 m/sec calculated and 19 m/sec observed at the temperature of 1.6°K). Although this discrepancy is not very large, it is too large to be attributed to the inaccuracy of the experimental data on the thermodynamic quantities of helium II. For calculating the velocity of second sound, the formulae were used for the thermodynamic quantities derived under the assumption that the energy spectrum of the liquid consists of two branches – the phonon and roton branches. The direction of the observed discrepancy indicates in what way these assumptions must be altered."

Landau's cogitations led him to the conclusion that the energy spectrum of helium II should not be separated into phonon and roton branches; that there must be a continuous transition between these two types of quasi-particle, occurring of course not at the start of the spectrum, where the energy varies linearly with the momentum, but further on, where the linear dependence ceases and the momenta themselves become larger, i.e. short-wave instead of long-wave.

"With such a spectrum," Landau wrote, "it is of course impossible to speak of rotons and phonons as strictly different types of elementary excitation. It would be more correct to speak simply of the long-wave (small p) and short-wave excitations. It is to be stressed that all the conclusions concerning the superfluidity and the entire macroscopic hydrodynamics of helium II [that is, the two-component model, motion in helium, second sound, etc. – A.L.] maintain their validity also with the spectrum proposed here."

Henceforward, after this work of Landau, the helium II energy spectrum constructed by him reached its final form, as shown in the diagram.



However, despite the continuity of the spectrum curve, Landau as well as other physicists continued to refer to phonons and rotons, firstly because there are very perceptible differences between these two types of elementary excitation; secondly, because the nature of the excitations is such that most of them correspond to the minima of energy, i.e. to small momenta at the origin (phonons) and large momenta near the minimum on the spectrum curve (rotons).

So the co-operation and the mutual aid and enrichment of

theory and experiment yielded excellent results. The final stage (the prediction of second sound, its experimental discovery, and the refinement of the energy spectrum curve) became an example of true feedback between theory and experiment, the two ways of learning about Nature.

Of course, the concentrated and successful assault on the mysteries of helium II was helped by the fact, very rare in the history of science, that all the work was done within a single Institute, which became the place where superfluidity was first discovered and then explained theoretically, and where finally the theoretical relations were confirmed experimentally.

Moreover, just as Landau had the good fortune to be at the scene of Kapitza's discovery of superfluidity, so the experimenters were extremely fortunate to have Landau at their side, with his constant interest not only in the experimental results but in all the refinements, details, and unexpected occurrences, behind which Landau better than anyone else could glimpse something new and with real genius, as one of his pupils said, find a common language in which to do business with the experimenters at every stage of the investigations. So it was with the elucidation of superfluidity in helium.

Epilogue to a discovery

This is an epilogue not to the story of Landau's theory of superfluidity, but to his discovery itself. If you think about the essence of the matter, about the whole problem of quantum macrosystems, or the quantum mechanics of condensed states as it is also called, such an epilogue to Landau's discovery grows into a whole science, or rather into several great branches of physics. This process really began quite recently; the branches have still to grow further, to become mature, to gain strength and weight.

It is true that there may also be a direct epilogue to the theory of the superfluidity of helium II. In particular, there is the experiment by which the physicists obtained the energy spectrum.

Neutrons were passed into a vessel containing helium II. Bombardment of substances with neutrons or charged particles is a method of investigation very widely used in physics. The "bullet" particle interacts with the "target" particle, and the results of the interaction, measured by instruments or recorded on photographic film, give information about the properties of particles.

Neutrons incident upon liquid helium likewise do not pass through it indifferently without interaction. But with what do they interact? That is the whole point. Not with atoms or with individual particles do neutrons share their energy. When the instrument records that a neutron leaving the vessel has lost some of its energy and momentum, this means that they have been transferred to the liquid as a whole. That is, they have been used to produce a quasi-particle with just the energy and momentum that the neutron has lost.

When a curve was plotted from the values given by these neutrons scattered in helium II, it was found that it gives exactly the Landau energy spectrum; the experimental points fell on the theoretical curve, as the physicists would say.

The earlier experiments of Andronikashvili and Peshkov confirmed directly the correctness of the two-component model of helium II, and indirectly the form of the energy spectrum. The neutrons experimentally "plotted" the whole curve of the spectrum, and not only its shape but also the quantitative relation between energy and momentum were in excellent agreement with the theory.

The neutron experiment formed the last link in a "triad" or sequence stated thus by Feynman: "Guess – compute consequences – compare with experiment."

Helium, like most elements, has isotopes. Two of them are stable, and are therefore present in natural helium, namely He^3 and He^4. In fact, however, natural helium consists almost entirely of He^4, with only a very small quantity of He^3. "Ordinary" helium has two protons and two neutrons in its atomic nucleus. For a long time it was not just the ordinary but the

only helium known to scientists (the lighter isotope was discovered in 1939), and the theory of superfluidity was constructed for it.

The isotope He^3 differs from He^4 not only in atomic weight (which is 25% less because of the absence of one of the neutrons): there is a particularly marked difference in their quantum properties. The He^4 nucleus, the well-known alpha particle, contains four particles, an even number, whereas He^3 has an odd number. There is an important quantum characteristic called "spin", which may be either an integer or half an integer. Since each particle in the nucleus has a spin of one-half, the total spin of the He^4 atom is an integer, but that of He^3 is half an integer. Objects with integer and half-integer spins are differently described by quantum mechanics; they are said to obey different quantum statistics – the Bose–Einstein and Fermi–Dirac statistics respectively. They are therefore called bosons and fermions respectively. Atoms of the "ordinary" isotope of helium are bosons, but those of the rare isotope He^3 are fermions.

The fact of belonging to one or the other quantum group affects not only the behaviour of one individual atom or particle, but also the properties and behaviour of the whole liquid. Landau's theory applies only to a "Bose liquid", and superfluidity is the result of the "Bose type" energy spectrum which he formulated.

In the years 1956–8 Landau derived the theory of a Fermi liquid, including the isotope He^3 when in the liquid state; He^3 liquefies at 3.2°K, an even lower temperature than for He^4.

Although, it must be repeated, the quantum properties of these two liquids are entirely different, Landau's theory shows how they can be brought closer. Particles with half-integer spin can correct this "defect" by combining, for example in pairs. Landau had in mind just this possible joining of atoms into groups when he wrote that "any liquid consisting of Bose particles necessarily possesses superfluid properties. The converse theorem that a liquid consisting of Fermi particles cannot be superfluid is in general form not true."

Later theoretical calculations showed that for He^3 such

combinations, and therefore a transition to a superfluid phase, can occur only at very low temperatures, much less than a fraction of a degree.

Ultimately, the transition to another phase in which this isotope He^3 becomes a superfluid would occur. Even then, it would not be exactly like He^4. First of all, it would have certain unusual magnetic properties that do not exist in helium II. Roughly speaking, superfluid He^3 would be similar both to the superfluid isotope He^4, helium II as it is usually called, which we have already met, and to a superconducting metal, and it would also have certain features as yet not understood by physicists and not having any analogues.

Theoreticians and experimenters in several countries are now extensively investigating quantum liquids. In particular, there is great interest in the properties of such liquids when combined as solutions, especially the He^3–He^4 solution. Here there is a surprising combination and interaction of the features of both components: superfluidity, phase transitions, acoustic vibrations, thermodynamic characteristics, and so on.

Thousandths of a degree; thousands of millions, even tens of thousand millions, of degrees – or, in the notation customary in physics, 10^{-3} °K and 10^{10} °K. Referring to what? To superfluidity. The first figure is the temperature at which He^3 was made superfluid; it is thousands of times less than the corresponding temperature for He^4. But this descent to ever lower temperatures is not surprising when we are concerned with quantum liquids, quantum macrosystems.

And how do thousands of millions of degrees come into our story? They are the temperatures in the interior of the stars, for example those known as neutron stars. Such stars have a mass about the same as that of our Sun, but their radius is only some 10 kilometres. The density of matter in them is therefore enormous, of the order of 10^{14} g/cm^3, or in everyday words hundreds of millions of tons per cubic centimetre. It is a density comparable with that found in atomic nuclei, and neutron stars and therefore sometimes called giant nuclei.

Astrophysicists consider that a neutron star is not a homogeneous object, the same at all points from the centre to the surface, but has a complex multilayer structure. There is, however, good reason to suppose that, in regions forming a considerable part of the star, the neutron liquid is in the superfluid state, at temperatures of the order of thousands of millions of degrees.

One type of neutron stars consists of the pulsars, which are rotating magnetized stars. The superfluidity of the neutron liquid in a pulsar must affect its properties. So says the theory, and there are some observations which confirm it.

However, the investigation of this class of quantum objects – not indeed macro-scale, but what might be called cosmo-scale, objects – is still in an early stage, and many interesting discoveries are yet to be made, as also in the "quantum ranges" of behaviour and existence of solids, and perhaps too in the processes that take place in atomic nuclei. There is a possibility that they too will prove to have some superfluid properties.

All these ideas and approaches to objects so various in nature, size, and position in the universe became possible upon the basis of Landau's theory of superfluidity. The words were well chosen that formed the citation at the award to him, in honour of this work, of the Nobel prize in physics: *For his pioneering theories concerning condensed matter, especially liquid helium.*

Dau Away from Physics

"THE BEST birthday anyone ever had!" exclaimed Landau to almost everyone he saw. The celebration of his fiftieth anniversary was a joyous and brilliant occasion.

There are probably many resonance processes in human relations and feelings. They include all "personal" festivals, and anniversaries such as Landau's. It is very agreeable to be present at these commemorations. One can try to describe what happens, but the real atmosphere of the celebration cannot be conveyed, because an anniversary is an occasion of special resonance. There is immediate response and appreciation for every joke, witticism, verse, song, and turn. They may not be the highest flights of poetry or gems of wit. It does not matter. They score direct hits, and are greeted with applause and laughter from the audience that on this day is so responsive (though usually very critical and sarcastic).

But what even a stranger must perceive is the level and strength of affection for the recipient. One wise old teacher, on her eightieth birthday, after listening to all the fervent and heartfelt speeches crowded with superlatives, said that she did appreciate what had been said, but the simple fact that so many of her pupils had *come* to the party showed the sincerity of their feelings.

Nowadays, indeed, both ceremonious and emotional speeches at birthday parties have been put aside, at least among physicists. The reading of formal addresses and salutations is also unpopular. "Leave your speeches with the porter," read a notice in the entrance-hall of the Institute of Physical Problems on Landau's fiftieth birthday. Solemn gatherings are now replaced by merry parties. But all that is done in preparation for such an occasion, the enthusiasm and inventiveness and hard work that go into

the composing of verses and poems, the preparation of albums and special playthings, is evidence of the real affection felt for the one whose birthday is to be remembered.

It is said that at the start of the day Landau was a little unhappy and perplexed. Perhaps he was affected by this imposing "round figure" date in his life? But he was soon caught up in the general gaiety. Or consoled by a telegram which read: "Everyone's fifty now, except the kids." And of course he was deeply moved by the presents.

Those marble tablets with "Landau's Ten Commandments" arrived from the Institute of Atomic Energy. Ilya Lifshitz, a great stamp-collector, made up a postage stamp depicting Landau. His portrait also appeared on a medal with one of his favourite sayings engraved:

Another fool says so.

On the model of Effel's "In the Beginning", an album was prepared on the creation of the school of Landau. Those present could read the "biography" written for the occasion, under the title "The true life-story of Academician L. D. Landau from his creation on 22 January 1908 to the present day, compiled from the recollections of eye-witnesses, rumours, and other reliable sources, and information from the house management committee." There were also many other gifts and comical devices.

Of course nobody forgot that the cause of the festivities was a very great scientist. But such celebrations are "between the lines", or else in jester's garb. Human characteristics are in the foreground. So for us too, let Landau the physicist give place now to Dau away from physics.

Perhaps the key to Landau's personality and actions may be his profound conviction that every person should and must be happy. That is his duty to himself, to life and, if you will, to society. He should be happy at work, in love, should live a full and satisfied life. Landau liked to give his version of Judgement Day: the Trinity will ask, and you must answer, how you lived your life, whether you were happy.

Landau not only proclaimed this belief, but very persistently "worked on himself" so as to become a truly happy person, to overcome within himself, and if possible outside himself, everything that prevented him from being happy. Referring to Landau's abnormal shyness as a young man, Evgeny Lifshitz writes: "This trait caused him a great deal of suffering and at times – as he himself confessed in later years – led him to despair. The changes which occurred in him with the years and transformed him into a buoyant and gregarious individual were largely a result of his characteristic self-discipline and feeling of duty towards himself. These qualities, together with his sober and self-critical mind, enabled him to train himself and evolve into a person with a rare ability – the ability to be happy. The same sobriety of mind enabled him always to distinguish between what is of real value in life and what is unimportant triviality, and thus also to retain his mental equilibrium during the difficult moments which occurred in his life too."

Of course, in the modern age some words do not have their former ring, and are usually spoken ironically. But there are surely not many people who would not wish to be happy. Almost certainly they will not enlarge on this, but rather complain that they are unlucky, that their lives have not been successful, and in particular that something essential failed to happen.

Landau, on the other hand, found in himself boldness and courage, not fearing ridicule or sarcasm, but daring to say and even to announce: "I want to be happy; I must be happy." But not only "I" – everybody; or even more strongly, "Man has no right not to be happy, he must be capable of building the life that is given him to be lived well."

He also gathered ideas, gave advice, showed a real and active desire to help, to teach, to explain; was confident that reason and logic, the operative means of science, should be effective here also. Anything irrational or contrary to logic he declared to be a far-fetched – or fabricated – complication. Things may well be difficult. But all psychological complexities and "over-indulgences" are really so much camouflage for one's disinclina-

tion to understand oneself and behave in a proper, logical, reasonable manner. Such was Landau's credo. He made every effort to follow it and to impart it to others. He would say with a smile that he was, alas, unable to complete his most important theory: how one should live and how to be happy.

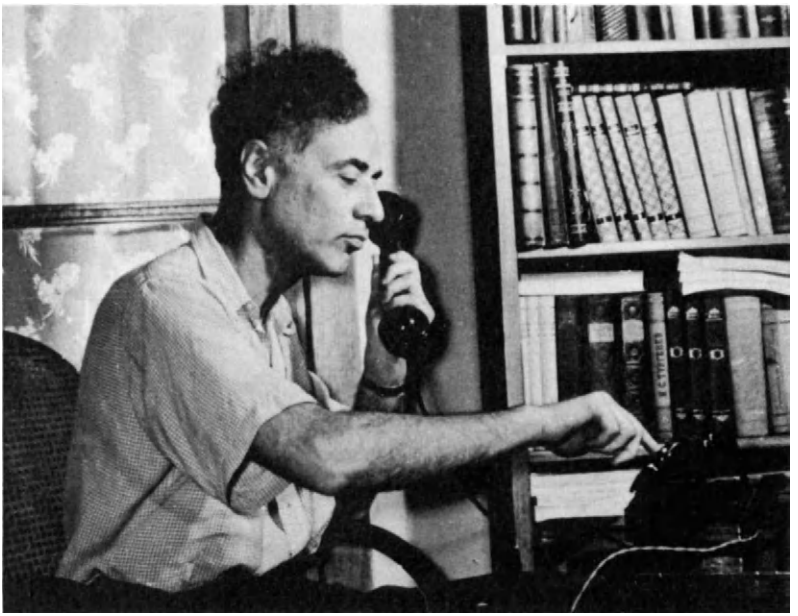
Probably he overlooked a detail of some importance. In physics, he was face to face with science alone. There he was a king and a god, of course up to a certain point, because even kings and gods at times prove impotent. In life, in human relationships, there have to be other people, on whom much may depend.

Moisey Kaganov, recalling his conversations with Landau, writes: "He was concerned really deeply, I should even say painfully, with the 'eternal themes'. His utterances were not conventional. Many were frightened off by the 'theoretical physics' clarity with which Dau tried (often with some success) to resolve complex problems of human relationships. He was deeply convinced that in most cases the complexity of relationships is fictitious (he always made a sharp distinction between 'complex' and 'difficult'), and he tried to get to the materialist core of the conflict. By temperament, Dau was an instructor, in life as in science. He felt that people ought to be taught how to live; and he did teach them. Landau had a hard but essentially happy life. He was surrounded by devoted pupils; recognition and fame were his while he was still alive. . . . Dau always avoided giving a direct answer to the question, 'What is happiness?' He said that everybody knows for himself what it is. Happiness is a very personal category that does not allow a general impersonal definition. Though aware of all the difficulties of life and the complexities of the modern world, Landau remained an optimist; gloomy forecasts did not appeal to him. This was particularly evident in relation to scientific forecasting."

"Dau was very fond of giving advice," those who knew him will often say. Whether or not to get married, whether or not to get divorced, whether or not to make a change. . . . Each time, Landau would concern himself with the details, and benevolently give out his authoritative and sometimes peremptory judgement, his "advice".

In physics, for example, Landau much more readily and often said "Think it out for yourself," "Solve it yourselves," whereas in everyday "problems" he was freer with advice. Moreover, in order to pronounce judgements and give advice, one must of course have information. This was the reason for his style of behaviour, his fondness for asking "indelicate questions", which shocked many people, especially those who knew him only slightly or not at all. They often failed to realize that Landau was asking not out of idle or foolish curiosity, but with a lively interest and good will, and again with a readiness to give reasonable, "scientifically based" advice.

Landau always had a great and sincere interest in people, in their fortunes, in the details of their lives; in all sorts of people, including complete strangers. In a conversation, even a first one, he immediately wanted to know how life was turning out for



Landau at home

the person he was talking to. Hence the shocking questions. They arose not from triviality or desire to give offence, but simply from genuine interest – though there may sometimes have been a desire to put the other person in an awkward position and make him lose his composure.

This was, however, more a sideline of Landau's character and behaviour. In the main, unusual though it may be, the traits of Landau the man were to some extent inseparable from those of Landau the physicist, and indeed were determined by the latter.

A slightly different formulation might be more correct. The two aspects developed from the same source and basis. Clarity and exactitude of the initial assumptions and data; rigorously systematic treatment; and an attempt to "trivialize". The last of these needs explanation. Landau often said, for example, that the most common shortcoming is greed, which tends to be the cause of failures, break-ups, even bankruptcy, both in private life and in one's work. But one does not wish to acknowledge such an unattractive vice; one tries to conceal it by referring to all kinds of complications of relationships and human psychology. It is necessary to see through these "complications" to the real elementary facts underlying them. This was what Landau meant by "trivializing".

The word "gramophone record" is associated by many with the image of Landau "outside physics". For example, his friends recall that one of his later favourites was this "record concerning greed", one of the defects that Landau most despised. And by greed he meant not only avarice. For him, it was a much broader concept, including careerism, and in general the capacity for improperly conducting one's affairs, the desire to make a profitable marriage, and similar aims and actions.

"It must not be supposed", said Landau's friend Moisey Korets, "that Dau talked in aphorisms. It was not like that. During conversations, a formulation was worked out, and this was what he liked to memorize. So we got the 'record'."

Korets recalls that even in the Kharkov days Landau explained to him the classification of conversations, not of course those

about science or business. Type 1 was the most attractive and the only one he really cared about and needed – a creative conversation in which the two persons think together. But not everyone is capable of holding such a conversation. On the other hand, it is not always suitable to remain silent when a creative conversation does not arise. Hence type 2, the "record". Everyone should have his supply of "records". It is simply necessary to ensure that the same record is not played twice to the same person. Lastly, type 3, the "noise", which generates acoustic vibrations in the atmosphere. Women are particularly gifted at engaging in this type of conversation, and in order to mix freely with them it is necessary to learn both to receive and to generate "noise". (Perhaps "society gossip" may be equated with "noise".) Dau, however, believed that mastery of this type of conversation is a great art, and was always unhappy that in his dealings with "the ladies" he himself could never achieve it and usually had to fall back on one of the first two types, most often replacing "noise" by "records".

Despite his principles, he probably did not escape some development of his tastes and preferences. Yet there were some things he went on repeating. "I'm a realist," he was always saying. He was very fond of Stendhal, especially *The Red and the Black.* He liked Dreiser (more than he did Hemingway), especially *The "Genius".* He explained why he liked *The Count of Monte Cristo* but not *The Three Musketeers.* The revenge of Monte Cristo was a just retribution for wrongdoing, and the novel was therefore based on justice. Milady of the *Musketeers* was indeed a horrible, cruel traitress, but she was not to blame for being so, since she had herself previously been treated in a horrible, cruel, and unjust way.

Only very ingenuous people regard the heroes of fiction in the same manner as they do their own fellow beings. But is this not really just the scientific approach of the theoretician? The initial premise being false, everything deduced from it must be rejected as invalid. There was little of the accidental in Landau's literary tastes. They were governed by a perfectly logical set of opinions.

If we try to identify his general approach, it seems that he rejected the literature of (in his view) excessively complicated and ambiguous feelings and relations, and also that which involved what he called "pathology" (one of his pet words, as we have seen). In literature, as in science, he was all for naturalness and definiteness. But this does not mean that he perceived artistic works rationally, purely intellectually, and without emotion. On the contrary, he was impressed by strong and undivided feelings (for example, he regarded the love story in *For Whom the Bell Tolls* as one of the masterpieces of world literature), but they must be pure and clear. Though having this attitude to Hemingway's novel, he disliked and rejected much of that writer's work, mainly because of its complication, impressionism, and the undertones present in almost all that he wrote. Landau liked ballads, for example, where there is a clear content, topic, and action. He also said "I'm a Simonov man," thinking especially of Simonov's wartime lyric poems.

Even a simple listing of his favourite works, in poetry and in prose, and of what he regarded with indifference or with positive, even furious, dislike, would be interesting. He was very fond of Kipling and Gumilev's poems of "courage". His list of likes also contained, on the other hand, much of Lermontov, the works of Heine, and Pasternak's *Hamlet*.

Did he always, stubbornly or good-naturedly, whether challenged or not, defend his tastes? With full awareness that he must be what he was? Or with regret that some things which gladdened others were not for him?

He said a little about himself in an interview with the weekly *Nedelya,* entitled "Frankly speaking": "I am afraid I shall disappoint someone, but I watch films in a purely 'childish' way. I worry about what will happen to the hero, I love the nice people, I hate the villains. This is if the picture is interesting, of course. If not, excuse me, but I shall tiptoe out of the cinema as quietly as I can. If a film bores me, I am not interested in the director's 'discoveries'. I cannot even bear to hear the expression.

"I would rather think afterwards about the director's work. If I liked the film, I think of its makers' names with pleasure

and gratitude. But if the director persistently reminds me of his existence, and so slows down unnaturally the action of the film, he only annoys me. The action is what I want most at the cinema – continuous action from which I cannot detach myself for a moment.

"Among recent Russian films, I was greatly impressed by *The House Where I Live* and *Ballad of a Soldier.* They showed outstanding originality on the part of the director, without obstructing the natural flow of the narrative.

"I hate boring films. Forgive my acerbity if I describe such productions as 'toffee'. I cannot stand the slow pace of the narrative, the heavy style of acting, the endless uneventful 'dead' scenes.

"One of the most hazardous causes of boring films is the attempt to spin out a short novella or tale to the length of an ordinary feature film. The screen versions of Chekhov, for instance *The Butterfly* and *The Lady with the Little Dog,* are particularly disastrous, and principally for just this reason.

"Different films, even by the same artist, need not be of equal value. Grigory Chukhray followed a great film, *The Forty-first,* and a notable one, *Ballad of a Soldier,* with one that I found pretty boring, *Clear Skies.* I ought to mention, however, that even there one feels grateful for a certain innate nobleness of the authors.

"In this conversation I must willy-nilly 'pass judgement' (only on my own behalf, of course) on many productions into which much intensive effort has been put. I should not wish their authors (if they do happen to notice my pronouncements) to feel that I am setting myself above them like a judge or a schoolmaster. Although my opinions are somewhat categorical, I am very far from seeking to obtrude my taste in the arts on anyone else. I can assure you that, if I were the manager of a cinema, I should willingly screen even films that I thought were very bad, if there was an audience to whom they gave pleasure. . .

"Does the cinema have a great educative role? One cannot imagine a scoundrel's being impressed by the argument that virtue is better than vice, however effectively this is presented.

In the same way, it would be naïve to suppose that watching a detective film will arouse criminal instincts in a person. However, if a film by the strength of its effect makes a person concerned about the fate of others, he thereby becomes a better person, though maybe only very slightly better.

"May I end by mentioning a topic where I am perhaps reasonably competent: the cinematic representation of scientists' life and work. Unfortunately, the successes in this area, either in films or in literature, can be counted on the fingers of one hand. Among books, I think the best so far (apart from Chekhov's *A Dreary Story*) is Sinclair Lewis's *Arrowsmith,* which gives a vivid picture of the mind of a scientific worker. Among films, even the famous *The Baltic Deputy* gives a totally incorrect account of the scientist's work. I am not intending to take the standpoint of the professional who carefully searches for specific minor inaccuracies; that would be valueless. It is sad to see a wrong representation of the environment, of the nature of the relationships. . . . Writers and directors still have a very slight and poor knowledge of the world of the scientist."

These comments on the cinema can probably be applied to literature also. But whereas in literature Landau was an ultra-orthodox "realist", in the fine arts he took a much broader view, and was not afraid to like and appreciate a painting "after Delacroix".

Everyone says with amusement that Landau could not endure opera and ballet. Here it is easy to draw up a scheme of his principles. It was "pathology" for people to sing their thoughts and feelings, and even more so to dance them. Words and speech are available for that purpose. Such "unnatural" behaviour on the stage was, as mathematicians say, orthogonal to his realist nature. They were simply incompatible. His attitude to music was clear also. He divided it into soft and loud, saying that soft music was better as not interfering with thought, but that he himself was "musically blind".

Such was Landau outside his work, outside physics.

Four more years of creative activity passed after that festive anniversary.

Landau once met a friend he had known in Leningrad, after an interval of many years. He asked her with a little anxiety, "I haven't become dull?" Her candid and joyous reply was "Not in the least!"

Not in the least did he "become dull" as a scientist either, to the last hour of his active and creative life.

Appendix: Landau's plain talk to students of physics

By E.M. Lifshitz, first published in *Nauka i zhizn'* (*Science and Life*), No. 9, 14–22, 1971; this translation by J. G. Adashko, first published in *American Journal of Physics* 45, No. 5, 415–422, 1977

IT IS now (1976) almost fifteen years since a tragic accident put an end to the brilliant activity of Lev Davidovich Landau. Those choosing to embark on a career in theoretical physics can no longer receive a valediction from the man whose door was open to anyone seeking his advice in science. The cast of this unusual man's mind also recedes in the past and becomes shrouded with legends. Even the most vivid recollections of those who were fortunate to be included among his close pupils and friends cannot give a full account of the uniqueness, lustre, and fascination of his personality.

Any remembrance is bound to be subjective and include some part of the personality of the narrator. Only the actual words, the direct manner of speaking, unparaphrased by the chronicler, are capable of giving an insight into a man's soul.

A man's vivid idiom can be deduced after his death from his letters. But Lev Davidovich wrote letters with great difficulty, and infrequently. It was difficult for him to set his thoughts down in writing (thus, his answer to one of the suggestions that he write a popular article was: "You have probably heard that I have utterly no aptitude for writing, and all my writings are with co-authors"). It was even diffficult for him to discourse in an article on scientific work he did alone (without co-workers!), and all his scientific papers were written for him for many years by others. His insuperable desire for brevity and clarity of

expression forced him to devote so much time to the choice of each phrase, that ultimately the task of writing anything, be it a scientific article or a personal letter, became a torment.

It is therefore all the more remarkable and characteristic of his high sense of duty, that Lev Davidovich always (albeit not always immediately) answered anyone who turned to him for advice or help ("I have delayed my answer not for any fundamental reason, but simply because it is difficult for me to write letters and it therefore takes me a long time to get round to it"; and again: "Excuse the delay, due to my extreme antipathy to the epistolary art").

For many years he dictated these letters directly to a typist at the office of the Institute of Physical Problems, pacing the floor and carefully delivering each phrase. We are indebted to Nina Dmitrievna Loshkareva, for many years the secretary of the Institute, for preserving copies of these letters, although they were "personal" rather than "official".

Much has been written about the fact that Landau was not only a genius as a physicist, but also a born teacher. The combination of both qualities in one person to such a degree is not a frequent occurrence in the history of science. In this respect, there are grounds for comparing Landau with his own teacher, the great Niels Bohr. Although they had little in common in their emotional make-up and traits of character – Bohr's extreme gentleness was not at all similar to Landau's ebullience and harshness – they did share something much more profound: absolute refusal to compromise when it comes to science, combined with good will towards people, readiness to help anyone trying to find his way in science, and the ability to rejoice in someone else's talent and someone else's scientific success.

It is therefore natural that a prominent place in Landau's correspondence is occupied by answers to young people's questions how and what to study. These answers not only demonstrate the quality of Landau's soul, but repeatedly reflect his views on the training of future physicists. These views should be of interest to the new generation of young scientists.

A student of one of the technical colleges in Penza writes to Lev Davidovich complaining that he spends much time in the physics laboratory and on self-education in mathematics and theoretical physics, but finds himself at a loss when confronting the vast material that is still to be learned: "I am only at the start of my journey, I do not see clearly the paths that lead to science, and I beg of you to help me get organized, to choose the right direction. You can help me very simply and very greatly: please send me a plan, some sort of programme that tells me what to study and in what sequence." Landau answers:

> Dear Comrade B.
>
> You are apparently seriously interested in physics, and I am very anxious to help you. It is very well that you understand that to work in science there is much for you to learn.
>
> As to what you should study, this depends very strongly on your future plans. The point is that modern physicists come in two classes — theoreticians and experimenters. The theoreticians use pens to write formulae on paper, and experimenters work with instruments in laboratories. Naturally, the education required for these two specialties differs. It is clear that the theoreticians need a much more thorough and profound theoretical foundation, although experimenters, of course, must also know quite a lot.
>
> Therefore, think over this question and write to me of your intentions. I shall be glad to send you appropriate programmes, and after you have studied this material, I think you will be ready enough to start.
>
> With best wishes,
>
> L. Landau

Worker L. writes to Lev Davidovich: "In a week I shall be leaving Moscow, and will be extremely grateful if you could give me some advice as to what and how I must study to become a theoretical physicist, and whether it is worthwhile for me to aim at this. . . . My training corresponds to approximately three years of the Mechanics and Mathematics Division of the Moscow

State University, but I am already 25 years old, and have to work." He writes of the problems he had tried to solve, the difficulties in the understanding of the principles of physical theories, and how he attempted to get round these difficulties; he mentions also that he finds it difficult to learn foreign languages."I beg of you, Lev Davidovich, please write to me if there is any hope of my becoming a physicist. And if this is the case, besides sending your famous programme and any advice you may wish to give me, please tell me how long it usually takes to complete your programme, so as to enable me to assess once more my own capabilities. I know, Lev Davidovich, how precious your time is, and I shall consider it a great honour if you answer me."

Lev Davidovich writes:

> Dear Comrade L.
>
> I shall attempt to answer your questions.
>
> It is of course difficult to evaluate beforehand your ability in the field of theoretical physics. But it is not the Gods who fire pottery. I believe that you will be able to work successfully in the field of theoretical physics if you really want to. It is most important that this work be your real interest. Vanity considerations can in no way replace real interest.
>
> Obviously you must first master properly the techniques of theoretical physics. This by itself is not too difficult, since you already went through part of the mathematical training, and mathematical techniques are the foundations of our science. Twenty-five is not too old (I am twice as old and have no intention of quitting), and a worker's labour, in any case, could not do you any harm.
>
> The only thing you must not do is to direct your effort to a solution of major scientific problems. If you just keep on working systematically, the solution of the problem will eventually come by itself. Financial difficulties, of course, can be a hindrance, for it is not easy to work on an empty stomach or when you are very tired. Foreign languages,

unfortunately, are essential. But don't forget that you need no special ability to master them, for even very dim-witted Englishmen have a fair knowledge of English. You have reached the correct conclusion that you must think less of the fundamentals. The most important thing to master is the technique of working, and the understanding of the fine points will come by itself.

To sum up, I can say that you will become a theoretician if you have a real interest and an ability to work. I am attaching the programme to this letter. As to the time involved, this will depend on the extent to which you are burdened with other matters, and on how much you actually know at present. In practice the time ranges from two and a half months in the case of Pomeranchuk, who practically knew everything at the outset, to several years in other also successful cases.

With best wishes, yours, Landau

A student of one of the technical colleges also writes of being attracted to theoretical physics, and how exasperated he becomes when faced with the many books and articles which he still finds it difficult to understand. He tells how he once attended Landau's seminar at the Institute of Physical Problems (admission to which was always open to all), but understood nothing, and could not bring himself to address Landau. Here is Lev Davidovich's answer:

Dear Comrade R.

If you are seriously interested in theoretical physics, I shall gladly help you engage in what I also regard as an alluring science.

Naturally, you are bewildered by the tremendous mass of material and don't know where to start. It is clear that a theoretical seminar would at present be unintelligible to you and it is still too early for you to attend it. I am sending you the "theoretical minimum" programme in which you can, if you wish, be examined section by section, by myself or by my associates.

You must start with mathematics which, you know, is the foundation of our science. The scope is indicated in the programme. Bear in mind that by "knowledge of mathematics" we mean not just all kinds of theorems, but a practical ability to integrate and to solve in quadratures ordinary differential equations, etc.

My telephone numbers are also indicated in the programme. You need not be afraid of me – I never bite.

With all wishes for your success

L. Landau

One more appeal to Lev Davidovich: "At one time Einstein did not refuse to help his student Infeld, and I therefore decided to write to you in the hope that you will not refuse my small request. I, too, am a student, only in the second year of the Radio Engineering Department, but I am very fond of theoretical physics. You are probably very busy, but if you can spare a few minutes for me, I shall be most grateful. It is absolutely essential for me to acquire a profound and comprehensive knowledge of most branches of theoretical physics, and therefore also of the necessary higher mathematics. . . . Forgive me for troubling you, but for me it is very important and, although it is perhaps not quite proper, but it happens in life, if the going gets rough, that there is not always room for propriety." Lev Davidovich answers on New Year's Eve:

Dear Comrade K.

I am glad to answer your letter. You are quite correct in believing that to work in theoretical physics you must first acquire knowledge of this field. I shall gladly help you with this.

As you have yourself understood, a theoretician must above all know his mathematics. What is needed is not all kinds of existence theorems, on which mathematicians lavish so much praise, but mathematical techniques, that is, the ability to solve concrete mathematical problems.

I would recommend the following course of study. First, learn to be able to perform correctly (and as rapidly as

possible) differentiation, integration, solution of ordinary differential equations in quadratures; study vector analysis and tensor algebra (i.e., how to operate with tensor indices). The principal role should be played here not by the textbook but by a book containing a set of problems – it does not much matter which book, so long as it contains enough problems.

After you have done this, please phone me (best between 9.30 and 10.30a.m., when I am almost always at home, but if necessary at some other time of the day), and come and see me. I shall examine you and supply you with a programme for further study. If you pass this entire programme (this will take from one to three years, depending on your knowledge and diligence), then I shall consider you to be fully prepared for scientific work, and will attempt to help you, if you so desire, with arrangements for this purpose.

This is all. With wishes for a happy New Year,

yours, Landau

Since Moscow residents were always able to communicate with Lev Davidovich directly, it is natural that letters came to him mainly from other cities. Many asked: Is it possible to become a theoretical physicist by studying neither in a special Physics Institute nor in a University? They found themselves confronted by a dilemma: should they continue to study in their own college or should they attempt to leave it and continue their studies independently?

Landau's answer to one such doubting student of a Pedagogical Institute is:

> It seems to me that you are creating an unnecessary dilemma for yourself. The fact that you will graduate from the Pedagogical Institute will in any case come in handy, and continuing your studies at the Institute will hardly interfere with your work. If you have enough will, you can study theoretical physics independently – after all, it requires nothing but books and paper.

A student from another pedagogical institute received, to an analogous query, the following reply:

> The fact that you ardently want to engage in physics is very good, for ardent love of science is the first token of success. Fortunately, theoretical physics is a science for which attendance at a university is not at all obligatory. I am enclosing in this letter a programme, which when followed will provide you with enough knowledge of theoretical physics to permit further independent work. Bear in mind that a mastery of mathematics is especially important. The main branches of mathematics are mentioned in the introductory part of the programme.
>
> If you can and want to, come to Moscow, where my coworkers or I can examine you in the various divisions of the programme (there are altogether nine, including mathematics). If you are successful, I hope to be able to help you find an opportunity to work in theoretical physics even in the event that you graduate not from the Moscow State University, but only from the Tula Pedagogical Institute.
>
> This, properly speaking, is all. I sincerely wish you all success. Remember, that most important in science is work, the rest will come by itself.

A passionate attachment to science, an enthusiasm not impelled by some extraneous motives, was valued by Lev Davidovich more than anything else, and these characteristics invariably aroused his sympathy and desire to help. The tone of his replies, however, becomes less sympathetic if he does not discern immediately this passion in the communication. Thus, a graduate of an out-of-town university, who expressed a desire to work at the Theoretical Division of the Institute of Physical Problems, but reported in this connection only that he is able to obtain a Moscow residence permit, received the following answer:

> Unfortunately, I cannot encourage you greatly. We are afraid to buy a pig in a poke, and choose graduate students only after they pass their theoretical physics examinations

in the form of the "Theoretical Minimum". The appropriate programme is attached. The examination can be taken at any time. If you crawl successfully through the potential barrier, then you could probably be accepted without a Moscow residence permit, since the Academy of Sciences provides hostels for out-of-town graduate students.

The "Theoretical Minimum" programme referred to in all these letters was first developed by Landau back in the 1930s, while at the Ukrainian Physicotechnical Institute in Kharkov, when students started to gather around him and his school of theoretical physics came into being. This programme was subsequently continuously revised, but the pedagogical principles on which it was based remained unchanged.

Lev Davidovich was the enemy of any superficiality and dilettantism: independent scientific work can be undertaken only after a sufficiently comprehensive study of the principles of science. In accordance with his deep conviction that theoretical physics is a unified and indivisible science with unified methods, he insisted that those who wish to become his pupils master first the principles of all the branches of theoretical physics. These principles were contained in seven successive sections of the "Theoretical Minimum" (mechanics, field theory, quantum mechanics, statistical physics, mechanics of continuous media, macroscopic electrodynamics, and relativistic quantum theory).

A characteristic feature of Landau's own scientific creativity was its almost unprecedented breadth; it encompassed all of theoretical physics – from hydrodynamics to quantum field theory. In our era of ever-narrowing specialization, such versatility is becoming an exception; with the passing of Landau, physics has lost possibly one of the last great universalists. Of course, he did not demand of anyone to be a universalist to the same degree that he himself was. But he regarded knowledge of all the branches of theoretical physics – at least within the scope of the "Theoretical Minimum" – as mandatory for all theoreticians, regardless of their narrow specialization. Again and again he repeats:

> As to your question concerning the study of theoretical physics, I can state only that it is necessary to learn ALL its main branches, and the sequence of study is dictated by their mutual relationship. As a method of study, I can only emphasize that you must perform all the calculations by yourself, and must not leave it to the authors of the books you have read.

It is interesting that at the same time Lev Davidovich believed that one person is incapable of simultaneously doing justice to both theoretical and experimental work in physics. To a group of students who advanced the opinion that a real theoretical physicist should be also an experimentalist, Lev Davidovich wrote:

> Those who believe that theoretical physicists can also be experimenters apparently imagine that theoreticians are supermen. Theoretical and experimental physics are now so greatly different, that one person cannot possibly engage in both. The only exception of the last few decades was Fermi but, recognizing that he was a genius, this exception only confirms the rule. Being engaged in different aspects of physics, theoreticians and experimenters supplement each other and are mutually related, but neither guides the other.

The examination on the "Theoretical Minimum" was always, if we can say so, efficacious: it was not necessary to derive some theoretical formula, but to be able to use one's knowledge to solve a posed concrete problem. At first, Lev Davidovich marked all the examinations himself. Later, when the number of applicants became too large, these duties were distributed among his closest collaborators. But the first examination, the first acquaintance with every new young person, Lev Davidovich always left to himself. Anyone could meet him for this purpose – all he had to do was to phone and express his wish.

Of course, not all those who undertook the study of the "Theoretical Minimum" had the ability and the persistence to complete it; there were many drop-outs. The list of those who

passed this trial from 1934 to 1961 (Lev Davidovich kept this roster personally) contains only 43 names. The efficiency of this selection can be assessed at least from the following formal data: eight* of those who passed have already become members of the Academy of Sciences, and sixteen more are Doctors of Science.

It is seen from the cited letters what great significance Lev Davidovich attached to mastery of the mathematical techniques. The degree of this mastery must be such that mathematical difficulties should, if possible, not divert the theoretician's attention from the physical difficulties of the problem – at least when it came to standard mathematical procedures. This can be accomplished only by sufficient training. Yet experience shows that the present university curricula and approaches frequently fail to provide this degree of training. Experience shows also that, if a physicist begins to study mathematics after he has started independent research, he finds it too "dull". Therefore, Lev Davidovich first subjected every examinee to a test in mathematics in its "practical", computational aspects. The following was required: ability to evaluate any indefinite integral (expressible in terms of elementary functions) and to solve any ordinary differential equation of standard type, and knowledge of vector analysis and tensor algebra. The second examination on mathematics included the principles of the theory of functions of complex variables (residue theory, the Laplace method). The assumption was that such disciplines as tensor analysis, group theory, etc., will be studied together with those divisions of theoretical physics in which they find application.

Landau's views on the mathematical education of physicists were set forth by him with great clarity in response to a request for his opinion on the mathematics programme in one of the colleges teaching physics. With his characteristic directness, he advances the opinion that these programmes must be planned with full account taken of the requirements of the physics departments – and with the advice of those who know, from

*Now eleven. – J.B.S.

their daily experience with scientific work in physics, what is required for this work. He writes:

> Unfortunately, your programmes suffer from the same shortcomings as all usual programmes on mathematics, which turn half the study of mathematics by physicists into a tiresome loss of time. For all the importance of mathematics to physicists, what they really need, as is well known, is computational analytic mathematics. Mathematicians, on the other hand, for inexplicable reasons, palm off on us logical exercises as a mandatory stock in trade. In this particular programme this is directly emphasized in the form of a special introductory remark. It seems to me that the time has long come to teach physicists what they need for themselves, and not save their souls despite their own desires. I do not care to dispute the venerable mediaeval scholastic opinion, that by studying unnecessary subjects people somehow learn how to think logically.
>
> I believe categorically that it is necessary to exclude from the mathematics courses designed for physicists all existence theorems, excessively rigorous proofs, etc. I shall therefore not stop to discuss separately all the numerous items in your programme which strongly contradict this point of view. I shall make only a few supplementary remarks.
>
> Vector analysis is taught in your programme as part of the study of multiple integrals. I have nothing against such a juxtaposition, but I hope that this is not done to the detriment of the absolutely essential formal knowledge of the formulae of vector analysis.
>
> The programme dealing with series is particularly overburdened with unnecessary things, which drown out those few useful data that are absolutely essential to know concerning the Fourier series and the Fourier integral.
>
> It would be correct in my opinion, to make the so-called mathematical physics an optional course. One should

not require experimental physicists to be able to handle such topics.

The need for a course in probability theory is quite doubtful. Physicists learn all they need to know about probability in courses of quantum mechanics and statistical physics.

Thus, I believe that the teaching of mathematics is ripe for a most serious reform. Those who undertake this most important and difficult task will deserve the sincere gratitude of the already trained physicists, but in particular of the numerous members of future generations.

Being deeply interested all his life in problems of teaching, Lev Davidovich dreamt of writing books on physics at all levels – from school textbooks to a course on theoretical physics for specialists. Actually, almost all the volumes of *Theoretical Physics* [written with E. M. Lifshitz, awarded the Lenin Prize in 1962 – *Editor's note*] were completed in his lifetime, as well as the first volumes of *Course of General Physics* and *Physics for All.* The *Shorter Course of Theoretical Physics,* which is based on his ideas, started being published after his death. He also planned to write textbooks of mathematics for physicists, which should be, in accordance with his ideas, "operating manuals" for teaching the practical application of mathematics to physics. He did not live to realize this programme.

Nor did he get round to writing school textbooks, although he was vividly interested in schools, and willingly addressed schoolchildren, and answered their letters.

Thus, the pioneers [boy scouts] of one of the schools in Tula wrote: "We know how little free time you have, but we hope nevertheless that you will find a few minutes to answer us. We wish to organize a gathering on the topic 'Education is a treasure, labour is a key to it,' since not all the pioneers of our class understand why they need an education. Many of them study their lessons not systematically, but only to get a pass mark. We would like a letter from you, since your words will be very convincing for our pioneers." Here is Lev Davidovich's answer:

Dear children,

It is very difficult to write of the obvious. After all, you know perfectly well that education is now essential for any occupation. The uneducated man will always be something second-rate.

In this context, I was very distressed to find a grammatical error in your letter. This shows that you children read very little, that you are not versed suitably even in your own native tongue. Therefore read much more – it is, after all, also entertaining – and remember that you need education not for the school but for yourselves, and that being educated is far from dull, but on the contrary, interesting.

With best wishes,

L. Landau

Lev Davidovich answered also those unfortunately too numerous persons who believe it possible to revolutionize science (including refutation of relativity theory) without having either knowledge or abilities for this purpose. In such cases, however, Lev Davidovich did not consider it necessary to show any sympathy and to mince words when expressing his disapproval. Here are a few examples of such answers:

I must say that your manuscript is lacking in any interest. Modern physics is a tremendous science, based primarily on a large number of experimental facts. You are patently almost completely unacquainted with this science, and you attempt to explain physical phenomena, about which you know little, with meaningless phrases. It is clear that nothing can come out of it. If you are seriously interested in physics, you should not engage in discoveries, but first learn at least a little about the subject.

Modern physics is a complicated and difficult science, and in order to accomplish anything in it, it is necessary to know very much. Knowledge is all the more needed in order to advance any new ideas. It is obvious from your letter that your knowledge of physics is very limited. What

you call new ideas is simply prattle of an ill-educated person; it is as if someone who never saw an electric machine before were to come to you and advance new ideas on this subject. If you are seriously interested in physics, first take time to study this science. After some time you yourself will see how ridiculous is this nonsense that came out of your typewriter.

The arguments advanced by you are unfortunately absurd to the highest degree. It would even be difficult to explain where the errors lie in your letter. For God's sake, before you start expounding on the universe, acquire at least the most elementary ability to read a physics text; all you do now is to put yourself in a ridiculous situation.

Your remarks consist of naïvetés that are of no interest whatever. It is clear, that if you wish to work in this field, there is much study ahead of you – to become acquainted with the subject. You would hardly sit behind the wheel of a car without knowing how to drive. Physics is by no means easier.

This brief sampling from Lev Davidovich's letters is appropriately concluded with a passage on the motives for work of a real scientist. Recognition of the results of his work to one degree or another is important to any scientist; it was important, of course, also to Lev Davidovich. There is no doubt, however, that the greatest inner incentive for himself was not a desire for fame, but inexhaustible curiosity, an all-consuming passion to know nature. And such a passion he valued primarily also in others. For this reason he always condemned the tendency to work only on "important" problems:

You ask in what to engage, in the sense of which branches of theoretical physics are the most important. I must say that I regard such a statement of the question as absurd. One must have a rather ridiculous immodesty to regard only "the most important" problems of science as worthy of one's interest. In my opinion, any physicist should engage in what interests him the most, and not embark on

> his scientific work from considerations of vanity. Of course, one must take care to avoid problems that are not wisely formulated and are therefore devoid of scientific interest.

One must never work for the sake of ulterior motives, for fame, with an aim at making a great discovery – nothing will come out of it anyway. Landau never missed an opportunity to repeat this simple truth.

Index

Abrikosov, A. A. 52
Andreev, A. F. 64, 69
  on Landau 55, 69
Andronikashvili, E. L. 26, 173–5, 184

Bacon, F. 2
Baku 16–17
Bohr, N. 20–3, 52, 100, 141–2, 161, 176
  and Landau 23, 81–3, 200
Born, M. 23
Bronstein, M. P. 19–20, 25–6, 102

Cambridge 23, 31–2, 176
Copenhagen viii, 20–1, 47, 102
Course of Theoretical Physics ix, 27, 40, 66, 69–77

de Broglie, L. 23, 147
Dirac, P. A. M. 21–3, 39
Drobantseva-Landau, C. 30

Ehrenfest, P. 23, 28, 117–8
  on Landau 28, 52
Einstein, A. 21, 99–100, 141, 176

Fermi, E. 20–3
Feynman, R. P. 104, 156, 162, 184
Filimonov, S. I. 128–9
Fock, V. A.
  on Landau 78, 89–90

Ginzburg, V. L. 79
  on Landau 22

Heisenberg, W. 21, 23, 139
Helium 101–87
  isotopes 184–6
  Kapitza's experiments on 128–38
  liquefaction of 102–3
  liquid 112–87
  superfluidity of 11, 14, 131–8, 159–66, 170–5
  II 116, 121, 144
  II, energy spectrum of 151–5, 182

Institute of Physical Problems *see* Moscow
Institute of Theoretical Physics 37, 39
Ioffe, A. F. 17–18, 26

Kagan, Yu. M.
  on Landau 43, 50, 55
Kaganov, M. I. 49, 51
  on Landau 47–8, 55, 80, 191
Kannegiesser, G. (Lady Peierls) 19
Kapitza, P. L. 18, 24, 36, 97, 110, 123, 176
  and the Institute of Physical Problems 31–3
  on Landau 4, 45, 78–9, 81–2, 83, 89–90
  his work 32–4, 124–38
Kazan 35, 178
Keesom, W. H. 108–9, 114, 116
  and Keesom, A. P. 33, 121–2
Kelvin, Lord 105
Kepler, J. 98–9
Khalatnikov, I. M. 39, 64
Kharkov 26–30, 74
  Ukrainian Physicotechnical Institute 26, 201
Kikoin, I. K. 10

Kitaigorodsky, A. I. 77, 157
Kompaneets, A. S. 45
  on Landau 45, 48, 53, 78, 82
Korets, M. A.
  on Landau 193
Kurchatov, I. V. 35

Lambda-point 117
Landau, D. L. 16
Landau, L. D.
  awards 35–6, 79
  breadth of understanding of physics 7–10, 44, 47, 57–9, 72, 207
  in Cambridge 23–4
  as classifier vii, 17, 21, 194
  in Copenhagen vii, 20–1, 47
  and the development of physics 3, 65–6
  early life 16–17
  and experimentalists 27–8, 82–3, 174–5, 180
  in Kharkov 26–30, 74
  last years 36
  in Leningrad 17–20, 25
  and mathematics 61–3, 209–11
  in Moscow 31–7
  nickname Dau 20, 88
  outside science 45, 62, 95–7, 188–98
  photographs: frontispiece, 27, 34, 40, 54, 75, 81, 140, 155, 192
  his school 3, 38, 77–85
  and science 1, 2, 18–19, 52, 84, 86, 139, 200, 207
  style of expression 4–5, 52, 74–6, 90, 199–214
  as teacher 30, 38–41, 43, 53, 68–9, 77–97
  his theoretical minimum *see* Minimum
  his theoretical seminar *see* Seminar
  his work 9–10, 22, 44–7, 138–87
  his working methods 73, 153–7
  in Zürich vii–viii, 23–4
Landau, L. V. 16
Landau, S. D. 16–17
Leiden 28, 102
Leningrad 17–20
  Physicotechnical Institute 18
  University 17, 18
Lifshitz, E. M. 59, 64, 72–3, 76–7, 79, 83, 178–9
  on Landau 42, 58, 60, 74, 82, 90–1, 139, 190
Lifshitz, I. M. 39, 79, 189
Lobachevsky, N. I. 2
Loshkareva, N. D. 200

Mermin, N. D.
  on Landau 8
Migdal, A. B. 46, 79, 88
Minimum, Landau's theoretical 45, 56–69, 73, 208–9
Moscow 31–7
  Institute of Physical Problems 31–7, 100, 126, 178, 200
  Institute of Theoretical Physics 37, 39
  Physicotechnical Institute 3, 76

Nobel Prize 11, 112, 175, 187

Obreimov, I. V. 26
Onnes, H. K. 102–3, 108–9, 112–15

Pauli, W. 23, 52–3
Peierls, Sir Rudolf vii–x, 19, 24, 47
Pekar, S. I. 55
Peshkov, V. P. 179–81, 184
Petushkov, A. V. 128
Phase transitions 116–20
Physicotechnical Institute *see* Kharkov; Leningrad; Moscow
Pitaevsky, L. P. 73
Planck, M. 99–100, 146–7
Pomeranchuk, I. Ya. 64, 79, 87–8, 105
  on Landau 38, 83, 87

Quantum 145
Quantum liquid 111–12, 120, 144
Quantum mechanics 109–11, 143

Quasi-particles 145, 149, 150

Rotons 153
Rumer, Yu. B.
  on Landau 9, 18, 23, 76
Rutherford, Lord 23, 31–2, 44, 176

Sagdeev, R. Z. 79
School of Landau 3, 38, 77–85
Schrödinger, E. 18, 21, 23, 139
Second sound 177–81
Seminar, Landau's theoretical 41–56, 94
Shalnikov, A. I. 31, 102, 178–9
  on Landau 4, 59
Shubnikov, L. V. 28, 29, 102
Smorodinsky, Ya. A. 49–50
  on Landau 43, 56
Sokolov, S. Ya. 178
Sokolov, Yu. L. 26
Superconductivity 112
Superfluidity of liquid helium 11, 14, 131–8, 159–66, 170–5

Tamm, I. E. 153
Temperature 103–8
Ten Commandments 10, 12–13, 119
Thomson, J. J. 176
Trapeznikova, O. N. 29, 102
  on Landau 25, 30

Weisskopf, V. F. 47

Zero, absolute 104–8
Zürich vii–viii, 23–4