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The Landau school and the American Institute of Physics translation program [1]

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Abstract. In the late 1950s and early 1960s several American scientists recognized the importance of results appearing in Russian language journals. Their efforts, aided by 'Cold War' considerations, culminated in the launch of the AIP program of translations into English of the then Soviet Union's leading physics journals. The present brief review gives a personal perspective on the history of that development.

It is a great honor and privilege to be asked to speak at this commemoration of the 100th anniversary of L D Landau's birth. Others know more and have spoken about his unique contributions, impact, and legacy. Being from a later generation, I have come to know him as a person only at second hand. But I am aware that he rated physicists on a scale from 0 to 5, with Einstein at 0.5. Landau promoted himself from 2.5 to 2. It would be enough for me not to be included among the 'pathologists' at 5. As you can see, your kind invitation induces feelings of inadequacy in me, but I have learnt from some of my best friends that one way of dealing with insecurity is to talk about yourself.

So, this talk is about the influence of his and his students' work on me, particularly the role played in this process by renditions of that work into English. I will tell you what I have learnt about the Soviet Physics translation program of the American Institute of Physics (AIP) — so essential for me and those of my friends with no Russian.

The name Landau often came up during my graduate studies: the Landau levels of an electron in a magnetic field; the de Haas van Alphen effect; the theory of second order phase transitions — particularly the Ginzburg – Landau phenomenological theory of superconductivity; the theory of the Fermi liquid. Strangely, the lectures I attended included nothing on interacting Bose liquids and their quantum hydrodynamics. The famous Landau – Lifshitz series of textbooks, which were translated into English in the 50s and 60s, did not particularly influence me. I find that I bought *Statistical Physics* in 1960 but my recollection is that I found the opening general remarks quite forbidding and closed the book. (Any of you who have seen my little book *Reasoning about Luck* (Cambridge University Press, 1996) will, however, find that the L – L views on entropy did finally become part of my way of thinking.)

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It was only as a postdoc in Copenhagen, at the University Institute (now called the Niels Bohr Institute) on Blegdamsvej, starting in September 1960, that I became fully aware of the scope and power of the Landau school. I wanted to learn about the microscopic theory of superconductivity. A year earlier, at the suggestion of my advisor Walter Kohn, I had started to read about this subject, while finishing a thesis on many-body effects in semi-conductors. I had plowed my way through Bardeen's 1956 *Handbuch der Physik* article [1], the Cooper letter [2], and the BCS paper [3]. The methodology of the last is wonderfully down-to-earth, but it is difficult to generalize. I had also attended a seminar by Valatin using what is sometimes called the Bogoliubov – Valatin method, but this, while a short-cut, actually made the calculations less transparent for me. From somewhere I had learned that the BCS theory had been redone by someone named Gor'kov, and I set about reading his paper, available in translation in the Copenhagen library. Luckily, the techniques were not unfamiliar to me, because John Ward, of the Ward identity and other inventions in field theory, had taught a course during a sabbatical visit to Carnegie Tech, where I was, and because I had read the paper by Luttinger and Ward on a microscopic derivation of some aspects of the theory of the Fermi liquid. This was very fortunate: the Russian style of scientific communication is terse, if not cryptic.

At my desk in Copenhagen in the autumn and winter of 1960, via *Soviet Physics JETP*, names previously unknown to me emerged one by one through the mist of the Cold War: Gor'kov, Abrikosov, Migdal, Eliashberg, and others, often thanking Academician Landau for his comments. This was quite wonderful intellectual communication the old-fashioned way — through print. I know that without the translations I would not have had a prayer of understanding these papers on superconductivity [4], superconducting alloys [5], the microscopic derivation of the Ginzburg – Landau theory [6], and electron-phonon effects [7].

Of course, this was the Khrushchev era with the first faint stirrings of future winds of change. There were two Soviet physicists at the Institute, S I Drozdov and V G Soloviev. Both specialized in Nuclear Physics. The former once recounted his experience giving a seminar in front of Landau. After presenting his theory, he started into a section he had very carefully prepared, describing experiments, when Landau said something like "Such theories always agree with experiment," and called for the next speaker. The latter was

[1] The paper was presented at the L D Landau centennial memorial session held on 19 – 20 June 2008 at the Central Scientists' House of the Russian Academy of Sciences, Moscow. This publication coincides with the 50th anniversary of the start, in September – December 1958, of the journal *Soviet Physics Uspekhi* (currently *Physics – Uspekhi*), whose first volume corresponded to that year's volume 66 of the original Russian language *Uspekhi Fizicheskikh Nauk* journal (*Editor's Note*).

connected with Dubna and the Bogoliubov school. I have no recollection of serious scientific exchanges with either of them, quite probably because of a difference in the phenomena that interested us: theirs, nuclei; mine, condensed matter. On anything approaching politics, they were both models of correctness. My most memorable personal encounter with a Russian in Copenhagen was while pretending to waltz — she did not follow my lead — with Ekaterina Maximova at a post-Giselle reception given by the institute, the zany brain-child of Norton Hintz, on sabbatical leave from the University of Minnesota. He and Gerry Brown (with the approval of the institute director Aage Bohr) had sent the Bolshoi Ballet, at the time in Brussels prior to their Copenhagen engagement, a telegram which began "In the interests of promoting the relations between culture and science, we...". It proved irresistible: a cultural attaché at the USSR embassy in Copenhagen soon telephoned to accept.... But I digress.

From anecdotal evidence, I think it likely that I would have been terrified of Landau, but never had the opportunity to put that feeling to the test. In 1962, the same year that Rudolph Nureyev dramatically defected, Landau had his tragic accident.

My presence here today may be because my early contributions to the physics of superconductivity [8 – 11] were noticed in the Soviet Union. These papers built on the foundation provided by the reading just described of work influenced by Landau. Indeed, the calculation [9], with my graduate student Alexis Baratoff, of the temperature dependence of the Josephson current, starts by translating Josephson's steps into Gor'kov's language, thereby making it possible to visualize the effect transparently in ordinary, as opposed to momentum, space. Another paper [10] generalizes the seminal Eliashberg theory of 'strong coupling' superconductivity to transport coefficients. Ludwig Tewordt and I constructed a theory of the thermal conductivity of such materials. Our paper contains the first explicit writing, to my knowledge, of the finite temperature Eliashberg equations. When these were numerically solved by Scalapino, Swihart, and Wada [12], my student James Woo and I [11] were able to explain semi-quantitatively the long-standing puzzle of the rapid decrease with temperature of the thermal conductivity of superconducting lead. None of this could have happened without the translations.

So, why, when, where, and how did the translation program come to be? Recently, Google led me to an extremely interesting and informative paper by David Kaiser, Associate Professor in the Program in Science, Technology, and Society at MIT. Some of this section of my talk makes substantial use of his paper — available on the web [13] — and source material from the AIP Niels Bohr Library which he provided me, with the permission of the AIP.

As background, it may be useful to recall that in the immediate post-WWII years, with the Soviet Union dominating Eastern Europe and the demonstrated Soviet nuclear bomb capabilities, the US and the USSR were in competition for world-wide influence. In some quarters, anti-communist and thus anti-Russian feelings were widespread, including fears that communist conspiracies were somehow threatening the "American way of life." The House Committee on Un-American Activities — descended from one set up in 1934 with the aim of investigating Nazi and other extreme right-wing organizations — was by now looking in the other direction, seeking and claiming to see evidence of leftist sympathizers in the film industry and elsewhere. In the Senate, McCarthy was contributing his name to a new 'ism', which also saw communist malignancies in the executive branch of the US government and in the universities.

That these fears extended to Russian science in general is well captured at the end of a hand-written letter [14] from William H Clohessy to his thesis advisor Hans Bethe. This letter is referred to in passing by Kaiser, but I have a copy from the Bethe papers in the Manuscript Collection at my home institution, Cornell University. Here it is in its entirety:

"The University of Wyoming
Laramie Wyoming
Prof H A Bethe Nov. 24, 1948

Sir,

I enclose some stamps which you asked for plus a few others you may or may not have. As I am not acquainted with methods used I left the postmarks along with the other.

The work here is not strenuous and is quite interesting. Since there is little research here I have determined to start some going. Everybody seems to be measuring magnetic moments and quadrupole moments of nuclei by resonance methods and I thought I could do something along those lines. I think when I have finished the papers in the P.R. on this subject I will need a new pair of glasses. Quite a lot is being done in this field. Particularly interesting, I believe, is the theoretical work on quadrupole moments of linear and symmetric top molecules, by Bardeen etc. High accuracy in verification of the known effects here will be of great importance.

I wanted to ask if there had been done the electron scattering experiments to which you called attention in Russian journal. If these have not been repeated here I may get something started along those lines. I am again pursuing the calculation and will let you know when I have the result desired.

I find the climate here quite splendid and invigorating. There are mountains south of here and intermediate size hills both east and west. Medicin [sic] Bow national park practically surrounds Laramie, As for the school the library is terrible — not a single forien [sic] language journal. When I suggested the Russian journal I was told I was treading on dangerous grounds and that such 'redness' was little tolerated.

Sincerely Yours,
William H Clohessy"

This letter refers to both of my personal heros in the world of physics, Hans Bethe and John Bardeen, whom I will mention later.

The view of Russian science from Laramie in 1948 was parochial to be sure, but not uncharacteristic of a general suspicion of open communication. To quote Kaiser: "Even at major centers like the Synchrotron Laboratory at Caltech, physicists had to seek permission from Atomic Energy Commission authorities before sending reprints of published articles to colleagues behind the Iron Curtain: the Commission likewise requested detailed lists of all such reprints received from Soviet sources."

The level of distrust diminished in the Eisenhower years, and by the mid-50s it also became clear that the quantity and quality of physics being openly published in Russian was such that it could not be ignored. There were two responses. First, several graduate schools allowed and encouraged the option of Russian to partly satisfy the then common two-foreign-languages requirement. Second, the issue of translating

Russian journals was taken up within the American Institute of Physics, and vigorously pursued by Elmer Hutchisson, at the time the Dean of the Graduate School at the Case Institute of Technology in Cleveland. Under his leadership, and with the sponsorship of the US National Science Foundation (NSF), a "Study of the feasibility of a comprehensive Russian-to-English translating service in the field of physics" was undertaken. The Final Report [15], dated October 6, 1954, makes for interesting reading. Its main opening points are that "for many years to come the great majority of physicists will need to depend on translations if they are to follow in any detail the progress of Soviet physics," that "Science progresses through the free flow of knowledge...," and that "There are probably more scientists and engineers being trained in the Soviet Union than in the United States. Can we afford not to keep abreast of the scientific output of this rapidly growing mass of scientific talent? Certainly, the easiest way of losing out in any race is to underestimate your opponent." The last thought will soon come up again in the history of this development. The report also offers evidence of support from the physics community for the idea, provides an estimate of the cost of translating a typical journal, and proposes to begin with the translation of the *Zhurnal Eksperimental'noi i Teoreticheskoi Fiziki*.

By the autumn of 1955, the first volume of *Soviet Physics JETP* was being produced, under the editorship of Robert Beyer, with an NSF grant of $40,000 to cover the first year. This was a small-scale operation — the proposal was for a single secretarial assistant. Translators were needed: physicists with Russian skills were pressed into part-time service. Two of them, reached by me, have responded with recollections. Freeman Dyson, known to all of you, has asked me to give you his greetings. He describes his work as follows:

"My memories of the translating I used to do give you only a worms-eye view. It was an important source of income for me although they only paid four dollars a page, later raised to six. My wife and I used to work late at night, the only time the babies were quiet, my wife typing while I dictated the translation. The worst of all was a paper by Vladimir Fock which was printed in very small type on large pages, so that each page seemed to go on for ever. I had to wake up my wife as she fell asleep at the typewriter. We needed the money as my salary was small and she was a stay-at-home mother with three children. There was another Russian whose name[2] I forget who wrote interminable papers with the title, "Action as a Space Coordinate", which was a version of five-dimensional relativity. I was sorry for him as he was a friend of Landau who was sent to the Gulag and totally isolated for ten years. After his release he could only get a job in Tomsk and so he remained isolated. At that time Russian physics was quite boring as most of the good stuff was classified.[3] Then some years later came the big surprise when JETP appeared full of papers about the suddenly declassified Dubna accelerator... After that it became more interesting, but I gave up translating as soon as I could afford to live without it. The only other thing I remember was that the part-time translators mostly dropped out soon after I did. As the volume of stuff to be translated grew larger, only the full-time translators could handle it efficiently."

[2] Yurii Borisovich Rumer. See *Usp. Fiz. Nauk* **171** 1131 (2001) [*Phys. Usp.* **44** (10) 1075 (2001)] for the story of his tragic life (*Editor's note*).

[3] The early papers on solid state physics of interest to me could not be described in this way; perhaps they were not considered of strategic significance.

John Armstrong retired in 1993 as Vice President and Director of Research of the IBM Corporation. He writes:

"I entered graduate school at Harvard in the fall of 1957, and with permission of my graduate advisor I enrolled in an intensive Russian course that amounted to half of my course work. Then in the summer of 1958 I spent 40 days in the Soviet Union as part of the first student-exchange program since the 1930's. During 1959 I signed on as a part-time translator with AIP. At that point the program was run by George Adashko. I operated just as Dyson describes; I stood over my wife, who was seated at the typewriter, and dictated my translation. Unlike Dyson, however, by the early 60's we were getting $14 per Russian page, and since *JETP* had quite small pages, I thought the money very good indeed. I kept at it for about three or four years. In retrospect, I realize that dictating (rather than writing out and editing) was not a particularly sound way to get a good translation... but this was before the days of word processors."

The "volume of stuff", as Dyson puts it, did indeed increase dramatically at the end of the 50s. Kaiser [13] convincingly makes the case that a key event was the launching of Sputnik on October 4, 1957. I was in graduate school in Pittsburgh at the time, and I remember the consternation and alarm set off by those shrill beeps from space. A steady drumbeat of warnings was sounded by many, some with their private wish-lists. Senator Jackson — who represented the State of Washington, but was sometimes called the Senator from Boeing — proclaimed [13] "that it was a 'devastating blow' to the country, adding that Eisenhower should declare 'a week of shame and danger.'" The 'man-power gap', based on the flawed [13] assertion that the Soviet Union was training 2 to 3 times as many scientists as the US, became a rallying cry in support of the National Defense Education Act — signed into law by President Eisenhower in September 1958 — which, among other things, substantially increased the money available for science education and research.

An increase in the number of journals translated under AIP auspices soon followed. Elmer Hutchisson re-emerges as an important force: he had become Director of the AIP on July 1, 1957. The pressing need for more study of science by the nation's youth was always on his mind. On October 7, a few days after the Sputnik launch, he addressed a press luncheon at the then AIP headquarters in New York. According to a *NY Times* article of the next day [16], headlined "Nation is Warned to Stress Science," he said that unless future generations appreciate the role of science in modern society and understand the conditions under which science thrives, "our way of life is, I am certain, doomed to rapid extinction." One month later the AIP "launched a vigorous campaign to increase the circulation of their translation journals.... To aid in this quest, the institute officers solicited written testimonials from leading physicists about the journals' importance — along with permission to use the statements in the institute's subscription campaign." [13] Among the responses [17] are:

"To sum up, I think the amount of good work to be found in the Russian literature is, in the solid-state area at least, a very sizable part of the world total. It would be inexcusable for American scientists not to follow it as closely as possible. Incidentally, I have made a few inquiries of other physicists here, and the general answer seems to be that many workers find them as useful as the sum total of British literature, or even more so."

Dr. Conyers Herring
Bell Telephone Laboratories, Inc.

"I have been following the Russian work on nucleon-nucleon scattering and on meson-nucleon scattering. Some of the very best results in these fields this year have appeared in the Russian literature, and it is largely accessible to me through the translations of the American Institute of Physics... ".

Dr. Robert R Wilson
Cornell University

With the help of a score of such testimonials, subscriptions to the translated journals were increased dramatically, and the resulting inflow of cash allowed the AIP to launch several more, such as *Astronomy, Crystallography, Solid State, and Uspekhi*, a journal of review articles akin to *Reviews of Modern Physics*. These efforts were aided by additional subsidies from the Atomic Energy Commission and other sources, all influenced by the notion that a 'race' for scientific supremacy was under way.

The rest, of course, is history: the expanded AIP program continued into the 1990s. (The reasons for its demise are not well known to me and, in any case, not of relevance here.) Kaiser's take on the beginnings is: "By parlaying a modest exploratory grant proposal into a significant governmental priority, officers at the American Institute of Physics managed to expand the raft of research journals with which all those fresh graduate students would occupy their time." One such (unknowing) beneficiary of the Cold War, who would have benefited even without the 'sputnik surge', stands before you.

But, the theme of this symposium is L D Landau. It goes without saying that in ordinary mortals like me the breadth of his understanding and of his achievements inspires nothing less than awe. The comprehensiveness of his knowledge of physics and his ability to move from the general to the particular are legendary. It would be quite wrong to overemphasize the difference between a top-down and a down-up view of physics, or completely attribute the former perspective to him, but it is useful to compare his style and vision with those of more 'one-step-at-a-time' workers — for example, the two other great theoretical physicists I mentioned and learned to revere: Bethe and Bardeen. I don't know how Landau ranked Bethe, but I know he had a low opinion of Bardeen.[4] Bardeen and Bethe reveled in sifting through phenomena, and building the theory up from them. By contrast, here is what Landau and Lifshitz say about nuclear forces in their *Quantum Mechanics*: "There is as yet no complete theory of nuclear forces.... In consequence, to describe nuclear forces it is still necessary to rely on experiment to a much greater extent than would be needed if a consistent theory were available." The lack of a consistent theory in this field is probably as true now as it was in the 50s when the sentence was written. However, even in areas such as condensed matter, where no one doubts that the basic theory is known, recent history confirms that it remains truly essential to pay close attention to and to learn from carefully designed experiments. Indeed, it is the lack of observations that has made so much of contemporary high-energy theoretical physics disconcertingly speculative. Nature may not be malicious but she is often more than subtle.[5] Sometimes she is mysterious if not downright devious. To understand her, we need every talent of every sort. Lev Davidovich was without question among the very greatest. I join you in jointly and posthumously promoting him from 2 to 1!

[4] I. Dzyaloshinskii, private communication. Someone, possibly in this audience, once told me that when an issue of the Physical Review arrived in Moscow in the 50s Landau would check off articles on all manner of topics and ask one of his students to report on them, and that he deliberately passed over articles by Bardeen. It is true that in some of Bardeen's early short papers on Superconductivity his intuitively constructed theories were wrong, but they were wrong in a deep way, identifying elements that would be needed in the definitive BCS work.

Acknowledgments. David Kaiser responded to my inquiries, after I stumbled on Ref. [13], and provided material from AIP files. Spencer Weart of the AIP gave permission to quote from these sources. Norton Hintz confirmed a Copenhagen memory from 1960 – 1961. Freeman Dyson and John Armstrong shared their interesting recollections. Igor Dzyaloshinskii replied to a request for information about Landau's opinion of Bardeen. My colleagues on the Cornell faculty Joel Silbey, History, and Neil Ashcroft and David Mermin, Physics, commented on a draft of the manuscript. Martin Blume, past Editor-in-Chief of the *Physical Review*, and Marc Brodsky, recent Executive Director of the AIP, read an almost final version. I thank them all very much.

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[5] Compare Einstein's much quoted 'Raffiniert ist der Hergott, aber boshaft ist er nicht.'