My life with Leggett

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Thirty years ago, I wrote "My life with Landau". I had never met the man. My title meant that Landau's work had had an enormous influence on my life as a physicist. Thirteen years ago I celebrated the centenary of 1905 by writing "My life with Einstein".

On the other hand in "My life with Peierls" (1997) I wrote about an important colleague who was also a personal friend. So also in "My life with Fisher" (2001), "My life with Kohn" (2003), and "My life with Wilson" (2014). I thought that today's contribution to the series would complete my own private pantheon, but last month Joel Leibowitz signed me up to write "My life with Hohenberg."

This is what happens to scientists over 80. Tony, be warned!

I.

I first heard the name A. J. Leggett 51 years ago. He was 29. There is a "Note added in proof" at the end of a paper I wrote in 1967. I had found a simple proof that a normal Fermi liquid *had* to support what Landau called "zero sound". My proof used a plausible field-theoretic conjecture, that I couldn't prove. I sent preprints to Gordon Baym and Leo Kadanoff, who were both junior faculty at Urbana.

One of them must have shown it to Tony, who was there as a postdoc. A letter arrived from somebody I didn't know, explaining that the gap in my argument was filled by some analysis he had published two years earlier. So at the beginning of my life with Leggett I viewed him as a brilliant young formalist who was better than me at manipulating Green's functions.

Several years later my paper with Tony's refinement was cited in Pitaevskii's second edition of the Landau-Lifshitz *Statistical Physics.* That's how half a century ago Tony played a central role in one of my earliest professional accomplishments.

II.

I can't remember our specific interactions over the next half dozen years. We must have met several times, because when Tony spent a month at Cornell in 1973, I remember it as an unexpected visit from an old friend. This was a year after the discovery by Osheroff, Richardson, and Lee of strange magnetic phenomena in liquid helium-3. Tony's visit was arranged by Bob Richardson. I couldn't have known Tony all that well, because

I remember wondering why Bob was interested in bringing to Ithaca a brilliant young formalist.

Tony sat for a month in the office next to mine. I had the pleasure of watching him, day by day, make increasingly good sense of the crazy magnetic resonance signals coming from the helium-3.

I was pleased to rediscover from an acknowledgment in Tony's paper on that work that I had told him about a strange way of thinking about spin-1 angular momentum states that I'd learned as a graduate student from Julian Schwinger. I can no longer remember what I told him, but it had something to do with the mysterious "d-vector".

III.

My next specific memory is of a sweltering week with Tony during one of the hottest English summers of the 20th century. This was at the Sussex Symposium on superfluid helium-3 in 1976. The Sussex downs — normally about as green as grass can get — looked like brown Southern California hills. That visit led to my spending an entire year with him at the University of Sussex two years later. I can pin that one down because there were two historic disasters toward the end of my visit: the Iranian revolution and ensuing crisis of American hostages, and the start of Margaret Thatcher's prime-ministership and her efforts to dismantle the British welfare state.

During my stay we had one of the coldest English winters of the 20th century. I watched people slipping and sliding while trying to drive up the icy hill I lived on before deciding that it was up to me, as a knowledgeable American, to take buckets of ash from our fireplace and sprinkle it on the road. After that everybody was OK. Nobody complained about my sullying the roadway.

Tony attended my lectures on the topological theory of defects. I attended his on the foundations of quantum mechanics. I clearly did not pay enough attention, since I was out of town lecturing when Sussex was visited by the wonderful John S. Bell, whom I did not meet for another ten years, only a year before his premature death.

IV.

The following year, 1980, Tony visited Cornell to be one of our first few Bethe Lecturers. What I remember most about that visit is that he gave a lecture on quantum foundations that finally did catch my attention, persuading me that there were issues here worth thinking about. I must, however, have promptly forgot about it again, since it was

at least two years after that, that I finally started thinking about such questions.

V.

In 1981 Tony played a dramatic role in the paper that made me famous. This was my essay in *Physics Today*, which described my campaign to make the word "boojum" an internationally accepted scientific term.

When *Physical Review Letters* agreed to let me use the term "boojum", they questioned my use of the plural "booja". I consulted Tony, who insisted that it would have been evident to any ancient Roman that boojum was a word of foreign origin, and words of foreign origin are indeclinable. Therefore if I wanted a latinate plural it couldn't be "booja". It had to be "boojum". One boojum, two boojum. So PRL used "boojums", and people are using "boojums" to this day.

But a few years later, "boojum" appeared in the Russian literature. The Russian boojum offered spectacular counterexamples to Tony's theory that highly inflected languages would treat the word as indeclinable. In one page I found the nominative plural (budzhumi), the genitive plural (budzhumov) and he instrumental singular (budzhumom). A few years after that I actually found a published paper using the instrumental plural: budzhumami!

I sent Tony a copy of a page with specimens of the many different inflected boojums. Here is his reply:

"I bow, however reluctantly, to the wisdom of the majority. I anticipate that we shall now presumably be getting, from Accra, reports of *m'boojum* and from Singapore of *boojum-boojum*, while those investigated by Olli Lounasmaa will presumably behave *boojuksesti.*"

VI.

I have an undatable memory of being with Tony in Sicily. We were at one of those meetings in Erice. I can't remember whether the subject was helium-3 or quantum foundations.

There was an excursion to the Greek temple and theater at Segesta. I remember standing on the hillside near the almost perfectly preserved semicircle of seats carved out of the hill, looking down towards the stage. Suddenly somebody stepped out down below me onto the center of the circle, and delivered a speech in what seemed to be classical Greek.

It was Tony, trying out the acoustics. They were perfect. I have never before or since heard him speak so loudly, slowly, and clearly. It was an enchanting moment.

VII.

In 1983 Tony spent an entire academic year at Cornell, on his way from Sussex to Urbana. By then we were both engaged in quantum foundational questions. I can pin down that visit because I remember that he acted as referee of a long, highly mathematical 1984 paper that Anupam Garg and I had been writing for the past two years. The best thing about our paper was our title "Farkas's Lemma and the Nature of Reality". The referee did not object.

Indeed, our paper pleased the referee enough that he brought Garg back with him to Urbana as a postdoc when he left Ithaca. There they wrote a paper "Is the flux there when nobody looks?" which has received 1000 citations. That's twice as many as the earlier paper of mine which, I'm proud to say, inspired their title.

VIII.

Some of my most interesting exchanges with Tony were in referee's reports. As a referee, he was always anonymous, but his written voice was impossible to disguise. In 1993 I sent a manuscript to *Reviews of Modern Physics* updating John Bell's 1966 critique of von Neumann's 1932 no-hidden-variables theorem. The referee of my paper remarked that the author considered himself to be a writer of fine scientific prose, but, he added, citing a horrible sentence from my text, here was an instance where Homer nodded.

I had no idea what that meant. Google being unavailable in 1993, I called the only physicist-classicist I knew, to ask him about"Homer nods". "Ah," Tony explained, "it means that even the best of us sometimes mess things up." So "nod" meant not to signify agreement but to doze off. I filed this away in my cabinet of metaphors. And I fixed the horrible sentence.

Last year I came upon three papers by philosophers of science claiming that Bell had misread von Neumann, whose argument actually made perfectly good sense. Swept away in the wreckage of Bell, 1966, would be Mermin, 1993, which I had come to feel was one of my nicest papers.

So I enlisted the help of my friend Ruediger Schack to reread von Neumann, Ruediger in German, I in English translation. We concluded that it was the philosophers, not Bell, who had misread von Neumann, who really had committed the surprising blunder

identified by Bell. We're working today on a paper to explain this. Thanks to Tony's 25-year old referee report, I already know our title: *Von Neumann's surprising oversight: Homer nodded.*

IX.

In the summer of 1990 at Amherst college, I spent almost a week with Tony at what was and remains the best conference I have ever been to. The subject was the meaning of quantum mechanics. Other participants were John and Mary Bell, Viki Weisskopf, Kurt Gottfried, Abner Shimony, Danny Greenberger, Mike Horne, and Anton Zeilinger. We spend a week in a fraternity house sitting around a large table chatting. No prepared talks, no agenda, no conference proceedings. Weisskopf and I sight-read a Mozart piano sonata at a dinner party. Everybody played croquet.

X.

One of the great things about the Osheroff-Richardson-Lee Nobel Prize was that they were all collaborators, and each of the three made full use of his authorization to invite up to ten guests to the festivities. So in 1996 there was a week-long reunion in Stockholm of many of the wonderful people who had participated, 25 years before, in the earliest years of the subject, when there was a limited amount of mysterious data to make sense of, and a limited number of papers to keep up with. Always the best time in any subfield of physics.

There were more good things to eat and drink than in any week of my life. It was completely exhausting.

So when I heard, 7 years later, that Tony himself had won the theory version of that Prize, I had several reactions:

(1) I was impressed that the Nobel Foundation had managed correctly to identify the theorist who had played the most important role in unravelling the mystery of what the hell was going on. (2) My heart went out to Tony for having to repeat, at a more advanced age, a week that would be even more exhausting than the one we had enjoyed together seven years earlier. (3) I wondered whether his fellow laureates Abrikosov and Ginzburg would give him a hard time for his current interest in as un-Russian a subject as the quantum measurement problem.

XI.

Tony's views and mine on quantum foundations started diverging almost as soon as he finally succeeded in convincing me that I really ought to have views. Tony has always

been worried by the quantum measurement problem. He has wondered whether quantum mechanics might break down at a certain level of macroscopicity. He has thought about how to design experiments to test for such a breakdown.

I, on the other hand, have suspected almost since Tony started me thinking about it, that the quantum measurement problem was not a problem about the *world*, but a problem about *us*. The perception that there *was* such a problem, was a sign that we were not thinking the right way about the nature of our scientific description of the world.

My view has the advantage over Tony's that it's not necessary to persuade anybody to do any experiments. It *is* necessary to think about how *I* manage to know what *I* think I *do* know.

Part of that thinking involves reexamining the nature of probability. Isn't it *strange* that quantum mechanics, a theory whose meaning people have not agreed on for ninety years, a theory that for the first time builds probability into its very foundations, has not led physicists into a systematic reexamination of the nature of probabilistic assertions?

Physicists still view probability in terms of frequencies and ensembles — a view that many mathematicians and statisticians have rejected in favor of viewing probabilities as numerical measures of the likelihood that personal expectations would be realized. Suppose quantum probabilities are personal judgments about what's going to happen to the physicist who is using quantum mechanics. That raises all kinds of questions, but it sure does get rid of "the quantum measurement problem."

A letter to Physics Today once asked how I could possibly believe that wave-packets were not collapsing in the early universe, long before there were physicists. My reply was to ask whether the letter-writer believed that probabilities were updating in the early universe, long before there were statisticians. This rhetorical debate remains unresolved.

My guess is that by the time today's 29 year-old physicists turn 50 — when Tony and I turn 101 and 104 — everybody will agree that physics is about how *we* relate to the world, and only historians of science will know what was meant by "the quantum measurement problem."

Physicists might well still think there is a problem in figuring out just what it is that *we* are. But, with Rudolf Peierls, I doubt that anybody will call that problem physics.