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Lille Conference talk

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I am especially grateful for the honor of the Russell Varian Award in recognition of the spin echo paper I wrote more than 50 years ago. It signals the recognition of many others as well. There exist today many implications, variations, and applications of the echo pulse method I did not anticipate at the time of my discovery. How else could a paper written so long ago be remembered if it has not been kept alive by compatriot NMR researchers?

I consider this recognition unique. As a post doc in the research group of Felix Bloch at Stanford (1950–1952) I had the opportunity to interact with Russell Varian, and now it is as though his ghost has come back to connect with me. After discussing my paper at length with him at that time, not long after it was published in 1950, he proposed that I write it up as a patent. The Varian company administered it as a pulse spin echo chemical analysis patent application, giving Varian a free license.

I remember Russell Varian would often visit and consult with Bloch in his office (in the old Stanford ''physics corner'') Campus Quad building. More than once Bloch commented how impressed he was with Varian's physical intuition—a perceptiveness like that of Michael Faraday—not formal at all but very skillful at presenting a physical picture. In fact Russell Varian even had facial features somewhat like those of Michael Faraday.

During my two years at Stanford I witnessed the early expansion of Varian Associates as it moved its facilities out of a few remaining original cinder block buildings in Redwood City to its present location south of the Stanford Campus. The company was beginning to expand from its main klystron oscillator product into a larger enterprise of commercial manufacturing of various kinds of technical instruments. Among them was the original Varian development of continuous-wave high-resolution NMR for chemical analysis, stimulated by the NMR discoveries of Bloch, Packard, and Hansen at Stanford. The technical staff of the Varian company became populated for periods of time with NMR notables, among them Richard Ernst, Ray Freeman, and former students of Felix Bloch.

Though it did not make much headway in the early 1950s and went mostly unnoticed, Russell Varian's spin echo patent adoption at that time backed the first venture in which the pulse NMR echo method was sold on the market. If I remember correctly, Varian sold only two spin echo pulse apparatuses to an oil company for the purpose of measuring relaxation times. The market demand was too small to continue production of the maverick pulse approach. In those days the introduction of the pulse technique was alien to the tradition of spectroscopy in the steady state. The CW method of chart recorded NMR spectroscopy prevailed. Pulsed NMR was too far ahead of its time, particularly for many chemists who were intimidated at first by any use of pulses. General use of the pulse method for spectroscopy did not come into being until after 1970 when fast electronic Fourier transform technology was developed and Ernst and Anderson pioneered Fourier transform measurements of FID transients following pulses. This method of spectroscopy showed the way for a greatly improved measurement efficiency, replacing the slow method of CW spectrum chart recorder analysis.

Accident of discovery

Certainly if I had not discovered the spin echo (Fig. 1), displaying the first free induction decay signal after a pulse as well, it would have been discovered by someone else quite soon. I was lucky to be familiar with the use of pulses because my duty in the Naval Reserve during WWII was to teach shipboard pulse radar and sonar electronics to Navy personnel. With that knowledge, as a beginning graduate student right after the war I decided to use gated radiofrequency power to saturate proton resonances at Illinois to measure spin-lattice relaxation times in liquids. At first, transient NMR signals that were not very short could be

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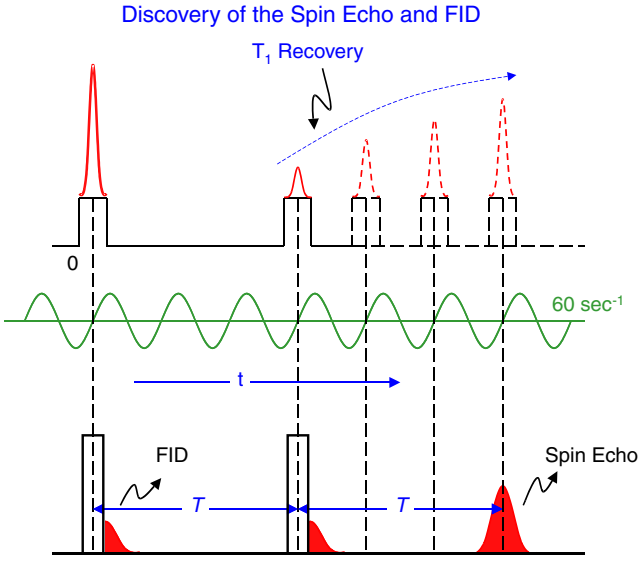

Fig. 1. Discovery of the spin echo and FID.

detected by using an ordinary narrow band commercial radio receiver. Broadband amplifiers conveniently available from war surplus radar equipment enabled the detection of faster signals following the application of short high-power pulses.

Although my doctoral thesis was not about the discovery of the spin echo, my thesis apparatus provided experimental conditions for its accidental discovery later. The apparatus was designed to display adiabatic fast passage, spin lattice relaxation, and transient signals during the application of radiofrequency fields. It should be made clear, when speaking of the spin echo pulse method, that reference is made to transient signals after the radiofrequency field is turned off, not while it is gated. Just when I finished my thesis in April 1949, unknown to me, Henry Torrey at Rutgers scooped me by publishing his work on transient nuclear spin nutations in gyroscopic terminology, commonly referred to as Rabi flop oscillations. I did the same experiment independently as part of my thesis. I measured these oscillations that start after the radiofrequency field is gated on and remains on at nuclear resonance. I was inspired to pursue this measurement after reading in Bloembergen's thesis about calculation of the effect by Schwinger for a two-level spin system. Based on this effect with a short pulse, the free induction decay that follows was first observed later with my discovery of the spin echo.

After I received my Ph.D. in April 1949, I remained as a post doc at Illinois for one year. During that time I upgraded my thesis apparatus by the application of shorter and more powerful gated pulses. By happenstance, in July 1949 the apparatus I developed for measuring spin-lattice relaxation times in liquids accidentally satisfied conditions for creating the echo and free induction signals because I began to apply short pulses in pairs.

In the first edition of Charlie Slichter's book *Principles of Magnetic Resonance*, it is stated that I discovered spin echoes as a graduate student, but actually I was a post doc. Because of Charlie's first book I got the minor reputation of being a sort of "Wunderkind" example as a graduate student. I don't fault Charlie for that error because it so happened when Charlie came to Illinois nobody happened to say a word to him that I was a post doc, so he assumed I was a graduate student. This was a natural deduction for Charlie to make because all the evidence he had to go by was that I was just as ignorant as a fresh post doc just after my Ph.D. as I was before I got my Ph.D. Later when I was invited to give a talk at some physics department, occasionally whoever introduced me as the speaker would patronizingly browbeat graduate students in the audience with the challenging advice, "See ... look what the speaker discovered as a graduate student ... you also can discover things as a graduate student ... blah, blah ...." I could tell by this challenge that the students would shrink ever so slightly down into their seats, being expected to admire the thought while actually resenting the obligation thrust on them by virtue of my presence. So to sooth them I had to explain that my thesis was not about the spin echo and that it was discovered after I received my Ph.D. I assume there was some consolation in telling them this, because I had to remove myself as an example that might oblige them to discover something like the echo for their thesis.

Certainly there are fewer and fewer nuggets like the echo that remain to be uncovered. In this case, anyone who first began to apply pulses extensively was bound to stumble on it. So I happened to be the first one in line. It was a combination of accident and serendipity. The war was just over, and happening to use double pulses for other reasons allowed the echo to appear. Circumstances for discovery cannot be dictated by fiat. They have to be lived through as they arise.