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Radium & Radon Metrology at NBS / NIST: an Aide-mémoire

These are my personal recollections based on my experiences over the past 40 years, as well as some independent searches of existing laboratory records, and of course my version of the stories told to me by the really old-timers like Lucy Cavallo, Bob Medlock, and Patricia Mullens. Bert Coursey, in referring to me recently, wrote that I knew "more about radium and radon metrology than any living scientist." I am certainly honored by this kind, if not charitable, flattery. These recollections are put together at his request and with his encouragement.

Coursey in his own personal account mentioned that the first Honigschmid radium standard (under the aegis of the International Radium Standards Commission) was received at NBS in Januray 1914. Actually, it arrived at the lab in December, 1913, and the first calibration using it was completed by April, 1914. It is significant to realize that this standard was fully employed to perform calibrations of submitted radium preparations within 5 months of its arrival. This might be contrasted with today's institutional milieu where many more months if not years would be required to develop all of the necessary protocols, standard operating procedures, hazard reviews, safety evaluations, training completions, and approvals by health physicists and safety offices before any real work could be initiated.

It is important to appreciate some of the peculiar history of what was going on during what I have termed the "Radium Craze Era" from 1900 through 1930. A cultural phenomenon came about. Radium (^{226}Ra) became the most valuable commodity on earth. It was bought and sold, and traded, and stored in "radium banks." Its use for imaging and for some legitimate medical treatments was quickly promoted. It soon, however, took on mythical properties. Many nostrum cures were soon advertised and sold by entrepreneurs, often as though they had the authority of scientific or medical proof. Radon (^{222}Rn), the decay product of radium and an inert noble gas, soon entered the mix. In time, all sorts radium and radon preparations as creams, salves., applicators, and inhalers became available to the general public, all without any regulation. With the price of radium reaching thousands of dollars per milligram it didn't take long for the scams to start. Inhalers with no radium to produce radon and radium applicators with no radium in them were being marketed. The Bureau's biggest customer for radium testing at the height of the craze was the U.S. Postal Service for prosecution of mail fraud cases. Newspapers loved the excitement of radium, for both its positive and negative features since both sold newspapers. There were reports of new and novel uses along with the scandals and controversies. Two of the bigger ones were those involving the "Radium Girls**" (f**emale factory workers who contracted radiation poisoning from painting watch dials with self-luminous paint at the United States Radium factory in NJ) and Radithor (a patent medicine said to be a "well know example of radioactive quackery" that consisted of triple-distilled water containing at a minimum 1 μCi (37 kBq) of both ^{226}Ra and ^{228}Ra. A prominent baseball athlete at the time swore by its powers and drank a bottle a day up until his death in 1932 by radiation poisoning. This scandal provided the impetus for governmental regulation by FDA and initiated the demise of most radiation-based patent medicines.

Sometime in the 1980s, I searched all of the annual Director Reports to the Secretary of Commerce to get a sense of the magnitude of the radon calibrations that were performed in those early years. From 1914 to 1942 (though I could not find records for some years in the mid-1920s) over 33,000 certified radium calibrations were performed whose total radium content was in excess of 420 grams. These old Director's reports are refreshingly honest in that they avoid all of today's bureaucratic jargon and usual weasel words and instead make simple declarative statements of exactly what was done, and even includes pointed complaints about the lack of adequate funding by Congress for specific equipment and facility requests. The quantity of radium calibrations in the reports was often also equated in terms of the market value of the radium, which peaked at about $ 3.5 million in 1922 and was typically about $ 1 million per year throughout the early 1930s. By 1922, calibrations were also performed on submitted mesothorium (^{228}Ra + ^{228}Ac) samples, and work had begun on determining radium content by the radon emanation method.

By 1940, radon testing with pulse ionization chambers, following gas purification and transfers, was routine. Measurements were made on water samples, on ores and rocks, as well as on whole-air breath samples of radium workers and uranium miners. Data (that I again collected in the 1980s) show that approximately 9360 were measured for radon in the decade from 1942 to 1952, of which 74 % were of breath samples.

The first radium standards (forerunners of the SRMs of today) were developed circa 1940. These consisted of a series of 15 standard radium solutions and 12 rock/ore samples, certified for radium content. The radium values for the solution standards were given as whole numbers of micrograms of radium, and were absolute in that they were reported without any assigned uncertainty.

All of the modern NIST ^{226}Ra standards have their origins from three batches of radium that were obtained from the Radium Chemical Company in 1940, 1947, and 1957. In fact all of the radium standards disseminated by NBS / NIST up to today are derived from one of these three batches. The current ^{226}Ra SRM solution standards were prepared from 1947 stock material and have about a 68-year ingrowth of the ^{210}Pb subseries, such that the ^{210}Pb to ^{226}Ra activity ratio is now approximately 0.9). All of the ^{226}Ra standards from 1940 to today are linked through direct comparisons (some details below) and all ^{222}Rn measurements performed by the laboratory are linked to these ^{226}Ra standards.

My personal involvement with the laboratory started in 1975, following post-doc stints at Brookhaven National Laboratory and at the University of Maryland. Most of my serious experience up to then was research on nuclear reaction mechanisms that involved fast radiochemical separations on irradiated targets.

I'll provide a little of my earlier background (in case anyone is even remotely interested) in how I got involved in metrology. Bert Coursey, my friend, colleague, and at times boss, once described me as the "most diversified member" of his Ionizing Radiation Division.

As of 2014, I consider myself as having done science for 50 years, marking the start with winning my high school's Science Research Club's Physics Prize (a pretty medal) in 1964. I haven't had an honest job since the summer of that year. Every job, position, or activity that I have been involved with since then was in a laboratory, starting by working throughout my undergraduate years in labs. In those early years, I built

an electron spin resonance detector for classroom demonstrations, cleaned glassware in a stinky, physical organic chemistry lab, served as an undergraduate teaching assistant in an analytical chem lab, supervised student use of UV, IR, and NMR facilities, received certification as a chemical technician, and began to engage in pure research all before receiving my B.S. in Chemistry from Georgia Tech in 1969.

As an undergraduate chemistry major at Georgia Tech, I was enrolled as a co-op student for two years, and worked for the City of Milwaukee testing laboratories as a certified (AIC) chemical technician. I learned what it was like to do the tedium of routine physical and chemical measurements! And at this relatively early age, I also developed an appreciation for the quality control aspects of laboratory work and the importance of being able to defend measurement results. The product of my work performing water and wastewater analyses was used as the basis of court cases of industrial polluters as well as to protect the environment and human health.

I started in this radioactivity business in 1968 at Georgia Tech. For my senior thesis I synthesized ^{55}Fe-labelled ferrocene for precise measurements of the M/L electron capture ratios by multi-wire proportional counting. This was pretty impressive work for an undergraduate and was said to be "better than most graduate students do." It won "Best Student Paper" at the ACS Southeastern Sectional Conference that year. I did this work at the knee of the legendary Professor R.W. Fink, who was perhaps the most demanding and exacting metrologist I ever worked with in my life. He was a "nuclear chemist" and the first to explain to me how a good chemist could learn to do nuclear physics and a few calculations, but a physicist wasn't likely to ever do chemistry – and both are needed in our work. I was the only chemist able to do source preparations in Fink's empire. All of his grad students and post docs were in physics. As a result, he tried to keep me around and when that wasn't to be, he pretty much wanted to take control over what I was to do in the next phase of my life. He wanted me to go to his Alma Mater – Berkeley. Fink got me accepted into two graduate schools with immediate research assistantships before I ever filled out a piece of paper. Coincidentally, the day before my graduation ceremony, Fink also introduced me to our commencement speaker – Glenn T. Seaborg. My first meeting with a Nobel Laureate! Fink was a student of Seaborg's at Berkley.

As a grad student at Rensselaer (RPI), I trained under the Manhattan-Project-era radiochemist Herbert Clark. He was my mentor and was an inspiration to me as a great and wise teacher. He became my friend and taught me much more than science. He exhibited gentlemanly old-world manners, alongside a wry sense of humor, and he possessed a social conscience that is often rarely found in scientists. By the way, Dr. Clark was the one who "blew the whistle" to the *New York Times* in 1952 that the globe was being circulated with fallout clouds from atmospheric weapons tests, based on background measurements made by his radiochemistry classes. The AEC was not happy!

My favorite character at RPI was Professor Paul Harteck, who many contend would have been successful in developing Germany's atomic bomb in the 1940s had he, rather than Heisenberg, been in charge. Among his many accomplishments, he developed the xenon lamp, one of the most widely used tools in photochemistry, was co-discoverer of tritium, originated the concept of centrifugal isotope separation, and correctly predicted the atmosphere of Venus in a simulation chamber before any spacecraft spectroscopy could verify it. His course on molecular spectroscopy (Quantum Mechanics prerequisite)

was the most difficult and challenging class I ever had. He was a colorful man and would delight students with his wonderful stories about Heisenberg, Schrodinger (and Schrodinger's wife who resented that his Nobel Prize was shared with Dirac), his thesis committee (Fritz Haber, Paul Ehrenfest, Max Planck), and Rutherford (who Harteck thought was a great experimentalist, but didn't know much physics!).

My research at RPI was a bit of an unusual experience. Firstly, before classes even started for me, my research began. I worked on three distinctly different projects, anyone of which could have served as my thesis. My degree was in Nuclear and Radiochemistry, but I never actually did much with my official Nuclear Chemistry thesis advisor. I instead worked more with physicists and did extraneous studies on the chemistry of technetium with Herb Clark. My thesis eventually turned out to involve atomic physics and analytical chemistry. Nevertheless, all of the several projects that I was involved with involved atomic physics and nuclear reaction studies in one way or another using a variety of low energy and medium energy accelerators, until I converged on my thesis topic, which was inner K- and L-shell impact ionization with high energy electrons. It was "high" energy for atomic ionization physics, but "low energy for nuclear reaction physics. My thesis is almost impossible to characterize because it had two parts: pure atomic physics of inner shell electron impact ionization (experimental & theory); and application of it for trace elemental analysis (analytical chemistry) that required almost no source preparation (because of high electron energies). The cross section measurements were the first systematic study across a wide range of atomic number Z = 23 to 83 to test atomic theory ionization by relativistic 2 MeV electrons. The use of high energy electrons for elemental analysis by energy dispersive x-ray spectrometry was novel since the method minimized the need for the usual stringent x-ray sample preparation. My thesis committee reflected the diversity in the topic having as members a nuclear chemist, a radiochemist, a classical analytical chemist, a theoretical quantum chemist, and a nuclear physicist. My thesis advisor was Ivor Preiss (a former student of Fink), though all of my research was done in much closer collaboration with Angela Li-Scholz, Editor Emerita of *Atomic Data and Nuclear Data Tables*, and her husband Wilfried Scholz at SUNY-Albany. Angela (whose godmother was Madame C.S. Wu from Columbia and of "Fall of Parity" fame), more than anyone, took me under her caring wings as a young man and gave me scientific maturity. She's the one who taught me to do the experiment completely in my head before going into the lab to "tinker."

Because I did a lot of things beside my official thesis, I finished grad school with five publications ranging from analytical chemistry to atomic physics to nuclear reaction physics and nuclear structure physics, working with accelerators at RPI (4-MeV Van de Graff & 100 MeV electron LINAC), SUNY-Albany (4-MeV Dynamitron), and at the Yale tandem Van de Graff (allowing me to tell personal anecdotes about D. Allan Bromley). I always said all the other stuff beyond my thesis work was just for fun. My very first publication (about the design and construction of an isopropanol / solid CO_2 target cooling system for really high-power electron beams for bremsstrahlung radiators) is almost an embarrassment. It was the first of many engineering failures, overshadowed later by subsequent radon emanation devices.

Perhaps the biggest claim to grad school fame however was that from initial enrollment to completion (thesis defense) I finished in 33 months (< 3 years), when the average length for chemistry or physics PhD in those days was about 6 years. This is the fastest chemistry or physics PhD in RPI history, and is still

the record. I revel in the fact that my record is from the oldest technological university in the English-speaking world and the first school in USA to offer **a** European-style Ph.D. in physical sciences.

My days at Brookhaven National Laboratory and then at the University of Maryland cyclotron lab are actually some of my fondest science memories.

Brookhaven was like the fulfillment of boyish dreams. It was the most collegial environment I ever worked at. It was not unusual for a young guy to be invited to sit down at lunch with a department chair and some visiting distinguished guest. My direct boss was Gerhart Friedlander, and my office was adjacent to that of W. Rubinson (who generalized the Bateman differential equations to include transformations from nuclear reaction production in addition to that from decay chains at all initial states) and down the hall from Ray Davis (of solar neutrino experiment fame and who loaned me his precious gas counters and taught me how to use a mercury Toepler pump. Nearly every day I'd see these textbook figures like Victor Weiskoff, Hans Bethe, or Aage Bohr visiting, and walking around, and you'd get to meet them. Exciting time in physics. I met Emilio Segrè when he visited, just after he retired from Berkeley, and on talking to him he challenged me as to why I didn't go to Berkeley, which according to him was the only place in the world to study radioactivity. I also met and shared a lunch table with Edward Teller, even though he was somewhat persona non grata with the Brookhaven liberals.

At Brookhaven, I mainly did nuclear reactions studies at medium energies (measure excitation functions to test nuclear reaction models) and at high energies to search for new isotopes far from stability made by exotic mechanisms. In addition to doing fast radiochemistry in wet labs next to the beam dumps (e.g., a 2-minute Fe separation and a 90-second Os flash sublimation on targets that might have produced hundreds of radionuclides from spallation reactions), and having access to the then world's highest resolution Ge(Li) and Si(LI) photon spectrometers and the world's largest super computer (two side-by-side CDC 6600 computers with a shared extended core), my tools were an ancient Van de Graff, the tandem Van de Graaff (at the time the world's largest electrostatic accelerator), the 60-inch cyclotron, and the 200-Mev proton LINAC injector for the 33-GeV AGS accelerator (used in three of the lab's Nobel Prizes). I got another half-dozen publications from my two years there.

In my "Laureate's Lecture" for the JARI Medal (2011) I mentioned that I personally knew five of the past nine awardees. One of them was Alfred Wolf, a father of the nuclear chemistry face of nuclear medicine. I met Al while working at Brookhaven shortly after receiving my doctorate. It was the early 1970s and an exciting time to be doing nuclear reaction studies, which called for rapid radiochemical separations from accelerator targets and subsequent spectrometry. At that time, Al led a large and active research group in the relatively new and burgeoning field of nuclear medicine. The group was well known as the "Wolf Pack" with a large number of postdoctoral fellows and students, and Al tried to recruit me into it. I suppose he thought that my radiochemistry experience might be useful to him for the synthesis and labeling of radiopharmaceuticals and that my knowledge of nuclear reaction mechanisms, based on my own research, might be useful for the production of those exotic nuclides like ^{123}I, ^{178}Ta, and even easy things like ^{18}F that were beginning to be used in medicine. I declined. Maybe I thought that nuclear medicine didn't have much of a future!

At Maryland, I was classified as part of the research "faculty", but what it really meant is that I was just a glorified post doc with a few other academic responsibilities. I was in charge of a large spectroscopy lab used by nearly 20 grad students in the Nuclear & Atmospheric Science program (meant making sure all the counting systems worked & helped any students needing help) and I got to teach a course I designed on tinkering (for foreign students & women who were afraid of tools). My research pretty much continued what I did at Brookhaven, except now I started a systematic comparison of reaction mechanisms. Main idea was to compare and look at difference between (p, 3p) reaction and (n, n'2p) reaction and bremsstrahlung-induced (γ, 2p) all of which from same target A would yield the same B. If I could predict cross sections for production of unknown nuclides from the systematics, then I could set upper limits on half-lifes for nuclides not detected. It was a revolutionary approach. There was only one other lab in the world in competition doing the same kind of stuff – Los Alamos. Accolades from this period were: Sigma Xi chapter (Honorary Science research society) gave me "Young Scientist's Award"; and at age 29 I was an invited speaker at the Gordon Research Conference (summer school) for Nuclear Reaction Physics. As a "chemist" no less, and lecturers at these summer school conferences were typically old guys who had years and years of wisdom.

Oh yes, the bremsstrahlung irradiations were done with the NBS LINAC, thanks to the generosity of Jim Leiss and Jim O'Connell. The times were different. I could drive onto the campus in the middle of the night (only time I could get beam time) with minimal security at guard posts to stop me, retrieve my targets from the end of the beam lines via rabbit shuttle, and load up them up in a lead pig for transport back to the Maryland campus in my car. There was no paperwork involved in this entire process. I'm also reminded of how many times I flew from the New Haven airport to Albany with a pig sitting between my feet on commuter flights when bringing sources back from Yale. As well as flying with similar pigs from Islip airport near Brookhaven to BWI a few years later.

My colleagues of today would be surprised to learn that at this point of my career I was considered to be somewhat of a wiz with electronics, interfaces, and the computers of the day. Jim Cumming, who oversaw some of my work at Brookhaven, had written the first complete non-linear regression code that could simultaneously fit multiple decay curves. He got me started on writing simple programs to analyze spectral and decay data. I brought these to Maryland with me and they pretty much got incorporated into grad student use there.

On my arrival at NBS, Wilfred Mann was the head of the Radioactivity Group. He was the founding editor of JARI and was one of the prime movers in organizing ICRM, which holds a biennial meeting of the world's radionuclidic metrologists. Dr. Mann largely built the Radioactivity Group at NBS (before we were known as NIST) into the world class laboratory that it became. Yes, he had the wisdom to hire me. Coursey mentioned all of the young turks (Bert Coursey, Mike Unterweger, Jim Noyce, Bob Ayres, Larry Lucas, Ken Inn) who came into the group all within a few years just before I did. I was not hired as a government employee, but instead was guest researcher employed by a consortium of all of the major North American radiopharmaceutical manufacturers, organized under the aegis of the Atomic Industrial Forum (AIF) -- which morphed into the U.S. Council for Energy Awareness (USCEA) and then into the Nuclear Energy Institute(NEI). I had heard about a possible opening in Radioactivity from people I had met at the NBS

LINAC. Although I was largely selected by Mann and Lucy Cavallo, I was interviewed by a committee at the AIF headquarters in NYC. They put me up at the Waldorf- Astoria on Park Avenue, and it was the last time that I received any largesse from them. The job I was offered was to work on the new Radiopharmaceutical Standards Program, which was pretty much under the direction of Lucy Cavallo. I hadn't changed my mind about nuclear medicine, but thought I'd like doing metrology.

It was different lab back then. It never handled such high levels of activity (still low for the reaction work I did before) nor radionuclides with half-lifes ranging from 6 hours (^{99m}Tc) to just a few weeks. The lab was quite staid and set in its old-fashioned ways and methods. I tried to shake up a lot of things, not very successfully. I knew chemistry and how to handle substantial quantities of radioactivity, but I did need to learn to do metrology. For this new program, many new facilities, techniques, and procedures need to be developed, most of which are still in use today. I couldn't learn to flame seal ampoules so I created the semi-automatic ampoule sealer (and it took forever to convince Wilfred that that there wasn't something wrong with the seals or that there was actually less evaporation of the solution). Use of precision automatic dispensers (pneumatically driven at first) over hand pipetting was another hard sell. For the first time, individualized lead shields for each ampoule had to be used during filling, sealing, and transport. A "hot cell" for the high level standards had to be designed and built. I often had to argue the case that it is better to do reduce personal exposure by doing some tasks quickly rather than relying on cumbersome shielding and remote handling that not only takes much longer and increases accident risks.

The production and calibration schedule was demanding since it called for an issue every month. There was rarely time to relax. Every month a new batch of stock radioactive material had to be ordered and received, the lab had to set up in total well in advance, pre-arrangement for the shipment of the sources to the various manufactures had to be done, master solutions needed to be prepared, the two levels of standards needed to be dispensed with gravimetric determinations of masses, every source had to be measured in ion chambers (or by some other method), a quick data reconciliation had to be made before the source were packaged for shipping, and of course there was nothing but transportation headaches in the beginning years. And all of this had to be done fast because of the short half-lifes. Lucy and I had to work through the nights when doing ^{99m}Tc. And then we might have to personally take the packaged standards directly for Dulles airport to get them on early morning flights. The work-up of final certified values and preparation of the certificates (painfully edited by Mann) was almost an afterthought. And then another month would roll around.

Once the program was up and running and after I went through a full year's cycle of monthly standards, I was ready to move on. This sort of repetition, demanding work output, and lack of any real research challenges was not my forte. I announced that I was looking for another job. Lucy convinced Wilfred to hire me, with the expectation that I would find, hire, and train a new AIF guy. It wasn't clear what I was to do after that!

This quality assurance program that I helped to establish with its proficiency testing component was a first and became a model for many others around the world and in other disciplines. It is still going strong after 40 years – though Dan Golas who replaced me is now retiring. (I'm not so sure about the new guy who

seems to have been hired without any relevant experience.) Dan had taken the radiochemistry course at RPI and was recommended to me by Herb Clark. I tried to convince Wilfred and Lucy that the program did not require a PhD level scientist (who might again get bored with it in a short order) and that a technician with an associate or bachelor's degree in science would be better. Dan with a Master's in Environmental Science was the compromise.

I joined the Radioactivity Section as a federal employee at the end of 1976. I had a few different jobs and re-incarnations at the laboratory over the years, but I've more or less been doing standards development since then.

It didn't take long for me to realize that the Mann and Collé egos could not comfortably fit within the same room – i.e., within the Radioactivity Group. I considered myself sufficiently senior that I didn't deal well with Mann's micromanagement style. Despite how much I admired and respected Mann, I must admit that I never bought into the Mann mystic that somehow allowed very competent, accomplished and senior scientists to bend their will to Mann's whims. I again began to look around for greener pastures at other labs and universities. Instead, another opportunity opened up.

Elmer Eisenhower was charged with starting a new program office called the "Office of Radiation Measurements" (ORM), which was sort of a staff organization off the Center for Radiation Research and outside any of the Center's Divisions or Groups. Elmer offered me a position in the Office, I accepted, and I was reassigned. My departure so quickly from the Radioactivity Group created some hard feelings, particularly by Lucy and Wilfred, in part because they made an effort to bring me on board. In time, within a few years, Wilfred and I got along famously because of some similar interests in history and art forensics.

The ORM was largely an outreach program, dealing mainly with State radiation control offices and with industry. The Office's principal responsibility was analysis and development of program plans. It mainly served as a liaison between outside users and the line organization with the NIST Groups. A large part of the office's work was also to drum up monies for the Groups. The ORM consisted of Elmer, myself and Tom Heaton. The entire radiation world was divided into two spheres of influence: dosimetry and radioactivity. Tom was in charge of the former, and I the latter. Tom quickly got involved in setting up regional state calibration laboratories, and I quickly got involved in helping state labs with the some environmental radioactivity issues, particularly radon. Elmer Eisenhower was the best immediate boss I ever had. He respected me, never challenged or second guessed any decision I made, and he left me alone to do my work. Throughout my tenure with ORM I worked closely with the Radioactivity Group, particularly with Robin Hutchinson and Dale Hoppes. I continued to do a lot of experimental work with them, brought substantial funding to them, and with the advantage of never having to be answerable to them.

A big part of the job was to interact with and serve as a liaison between the Bureau and many outside groups, establishing contacts throughout the industrial, state and federal government, and academic research

communities One of the first big tasks I was charged with was working on a committee to Upgrade Environmental Radiation Measurement Data. There were a half dozen or so subgroups. The committee had representatives from federal agencies, national labs, state radiation control offices, and industry. The state of affairs with environmental data collecting and reporting was a disaster with great inconsistencies between different labs, and with a fair degree of incorrect and illogical practices. I chaired the group that addressed the issue of these inconsistencies in reporting requirements and practices for environmental radiation data, and I was primarily responsible for developing a consensus on technically reasonable practices for environmental radiation data, and on a sound approach to reporting. The topics covered included unit dimensioning, significant figures, treatment of uncertainty statements, and detection limits. The data reporting guidelines were adopted quickly (by the environmental measurement community as well as by the states, and federal regulatory authorities like NRC, EPA, and DoE) and are largely still in use after 35 years. For my efforts, I received a Bronze medal , which disappointed Elmer since he had nominated me for a Silver medal and believed that the work was of great significance to deserve even Gold.

A major and extremely difficult aspect for this work involved treatment of uncertainties. I began to work closely with the Statistical Engineering Division, particularly with Harry Ku and Churchill Eisenhart. Churchill and I became very close friends and he took me under his wing, teaching me and thrilling me with stories of his life. As a result of my work on uncertainties, Dr. Ambler (then NBS director) selected me to represent NIST on the BIPM working group, which ultimately resulted in the present international guidance, commonly referred to as the GUM. I was the originator of the terms "type A" and "type B" assessments in uncertainty analysis, and served as first chair of the ISO uncertainty committee, which eventually led to the GUM. Amblers as well as Giacomo (BIPM Director) were insistent that the development of uncertainty guidance should be done by metrologist and scientists and NOT by statisticians. Even Ku and Eisenhower at the time concurred. Over the past 20 years, the original philosophy inherent in the GUM seems to be getting more and more lost, and the statisticians have indeed taken it over creating nearly unreadable documents that will eventually become useless and irrelevant.

Earlier, I mentioned what I termed the “Radium Craze Era” prior to 1930. The latter part of the 1970s and early 1980s saw what I have called the “Radon (or neo-Radium) Craze Era”, brought about by real and imagined hazards of indoor radon. The issue was totally politicized [cf., Leonard Cole, *Element of Risk: The Politics of Radon*, AAAS Press, Washington, DC, 1993], championed by Rich Guimond and EPA for personal promotion and bureaucratic power. Once started, and then accelerated by the Watras house affair, promulgation of radon programs became a runaway and unstoppable freight train. The Bureau got drawn into it at the request of state agencies and other federal agencies (EPA, DOE, HUD, etc.)

By 1978, I pretty much became the lab’s “radon expert” and spent a good part of the next 15 years involved in radon standards development, radon measurement comparisons, assistance to other labs, and endless committee assignments. My first task was to assist the State of Florida with radiation protection matters involving phosphate strip mining and reclamation of phosphate lands. Much of this was in the way of advice, but it also involved some hands on activity. I must have made nearly a dozen trips to central Florida over the course of 18 or so months. This includes one stint of several weeks in the center of the phosphate region (near Lakeland, FL) trying to evaluate the possibility of making exposure rate measurements at better than 10 % inaccuracy at levels around 1 to 10 $\mu R\ hr^{-1}$ and the looking at the efficacy of the correlation between exposure rates and radon concentration. EPA had proposed using gamma-ray exposure rates (at 1 meter above the land

surface) as a predictor of future indoor radon daughter concentration. It was stretch at best, which should have been self-evident from intuition alone. The initial relation was based on scant data and irt soon became apparent that it was without merit. Despite that EPA wrote a regulation formalizing it and hung onto it for several more years before putting a moratorium on use of land and eventually requiring a different reclamation method (restoring the overburden in the same order it was removed). A half-dozen years later, EPA decided that radon exhalation measurements (quantity of radon leaving a surface per unit time and surface area) was the way to go, which led to our work on the large surface area "flux density standard" (later).

A few years after Mann's famous 1973 paper on national needs for radioactivity standrads in various fields (co-authored with many others in the Radioactivity Group) , I wrote a few papers in the period 1978 to 1983 on the needs for national radon measurements and support services and the role of NBS. This was in part supported by "A Survey of Radon Measurement Needs and Activities in State Radiation Control Programs", jointly published by NBS and Conference of Radiation Control Program Directors in 1982.

In late 1970s, ORM secured funding to build a large 600 L environmental chamber, primarily to serve a radon exposure chamber for both radon and radon daughter calibrations. It was completed just in time to be used for post-calibrate the survey meters used by NRC and DOE for gaseous beta emitters following the Three Mile Island nuclear power reactor accident and release!

At about this same time in 1979, the ORM sponsored a "radon in buildings" meeting to address indoor radon – this was the first radon in buildings meeting ever held, anywhere. It was termed a Roundtable Discussion that brought together a number of participants with diverse interdisciplinary interests in radiation protection, radiation measurement, and building technology.

By 1980, I, though remaining in ORM, was actively involved in experimental collaborations with Robin Hutchinson and others in the Radioactivity Group. Some of this initial work involved: assembling the nucleus of a new radon lab; refurbishing and re-inaugurating the old 1940s pulse ionization chambers (and it gas purification and transfer system); constructing small radon manifolds (including those for the large radon chamber); evaluating the only commercially available solid, flow through radon and thoron sources (Pylon); calibrating and distributing a special-fabricated set eight of alpha sources (for Rn daughter measurements) to the principal radon labs in the US (EML, EPA, Bureau of Mines, etc.).

Brian ended his part of the presentation by talking about what I probably am most proud about in my career. Radioactivity work has been going at NBS/NIST since 1914 (exactly 100 years), and by their reckoning I was responsible for the training and mentoring of more new people than anyone else in this long period, including the training of Brian himself.