Leuven 1968-69*

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Abstract

This personal recollection is submitted in connection with the 50th anniversary of the Institute for Theoretical Physics in Leuven and its founding by Professor Frans Cerulus.

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1 Introduction

My family and I speak frequently about the time, from September 1968 to July 1969, that we spent in Leuven, but I had never have given much thought to writing up anything about our experiences until a few years ago. Then Désiré Bollé commented to me that he was due for mandatory retirement in the fall of 2011. Désiré was a graduate student when we visited. Since I am a bit older than him, I realized that if I ever was going to put something together about that year, I had better do it soon. After starting, I contacted Frans Cerulus to check on a few facts who informed me about a proposed celebration of the 50th anniversary of the Institute for Theoretical Physics in Leuven in the fall of 2012. This occasion provided the focus for this document.

In looking back over the more than forty years since our visit, I found many scientific coincidences and linkages that had developed since then. It is remarkable how the small cluster of people in the theory group, students, postdocs, faculty, and visitors, evolved in their scientific careers over a variety of pathways. The specifics of these careers are well known, so I will comment briefly on them only when they are relevant to my outsider’s observations during a highly transitional time for all of us, our universities, and our countries.

2 Prequel to Leuven

In 1966 David Speiser visited Case Institute of Technology primarily to work with Leslie Foldy. Near the end of his stay he was joined by Jacques Weyers, who spent a very productive [1,2] part of the summer at Case on his way to a postdoc position at SLAC. Although that summer was the first, and as far as I know, the last time I interacted with Jacques, at his talks and socially with his wife and my family, this brief contact was crucial to future developments.

The first opportunity for me to visit Leuven was for the 1967–1968 academic year. The invitation letter referred to the Institut de Physique Théorique, Université de Louvain and the Centre de Physique Nucléaire in Héverlé. I had to decline for various reasons, but I was delighted when the opportunity presented itself again for the 1968–1969 year at the Instituut voor Theoretische Fysica, Katholieke Universiteit of Leuven, Heverlee. Much about the sensitive topic of the change of names and the linguistic split of the University was
confusing to me at the time, in my considerable naïveté, and led to a number of errors in my addresses and affiliations on various documents, conference participation, and other listings that have persisted to this day. Ironically, about the same time CWRU was in the early stages of its sometimes painful evolution from the Case Institute of Technology and Western Reserve University and this led to a number of early mutations, such as "Case-Western University," that appeared as my affiliations on some of my papers of that epoch. A recent controversial attempt at truncation to "Case" ended with the resignation of the CWRU President who championed it. Nonetheless, as with most direct as compared to inverse operations, marriage is relatively easy to do, but divorce can be quite difficult. At CWRU we experienced both: The combination from 1967 to 1969 into a fairly large Physics Department and then the inevitable downsizing.

We left an America in turmoil in the summer of 1968 just after the riots at the Democratic Convention in Chicago which we viewed from our hotel in New York. We decompressed aboard the SS France for five days before arriving in Southampton. After a couple of weeks in the UK, we took the boat train from London to Ostend and then trains to Leuven via Brussels. We were escorted for the final leg of the train journey by David's students Anne Vandebroucke and Marie-Gabrielle Smoes. In what seemed like magical timing, they identified themselves on the platform at the Brussels by holding a copy of Physical Review Letters, whose green cover was instantly recognizable to all physicists at that time, but possibly not now. Once in Leuven, Anne took us by car for dinner at Inno, which at that time had a supermarket and a cafeteria. She then took us to the housing that the university had provided at Vaartstraat 77.

3 The Institute

In my first visit to the former institute building Frans Cerulus took me around and introduced me to people. Except for our correspondence prior to my arrival, I knew Frans only by reputation [3]. Much of this is a blur now, but I vividly remember being presented to the graduate students who were in a sunny combination office. The friendliness of the graduate students was inviting, but this was before they were subjected to my lectures. Frans showed me my very nice office that had a wonderful view of the park. There are a number of vignettes that took place over the year in that office that I remember quite
well. Many conversations with Frans, of course, usually friendly, brief, and important. Unfortunately, I do not remember the name of the first student who showed up after one of my lectures early in the fall, but I do remember how he looked and the vigor of our discussion concerning whether strong or weak convergence was appropriate for nonrelativistic potential scattering as contrasted with LSZ-type considerations. This was great, and I anticipated more productive arguments generated by the lectures, but, alas, most of the other students were much more polite and deferential, but were thought-provoking nonetheless and usually gave me until the next lecture to respond. I was not asked to lecture in anything but English, which I guess was part of the business plan for my appointment, and I never did, unless Midwestern-accented American can be regarded as distinct from anything else.

Once when Roland Omnès came to give a talk (in French) on astrophysics, Frans brought him to my office. Initially, the discussion was purely one-sided and I had the distinct feeling that he preferred not to converse in English. Then, after what seemed to be an eternity, he opened up, clarified the problem with three-particle unitarity that I had described and then he left in evident relief. I have always been grateful for Frans for this interaction with an iconic figure of the dispersion-theory era, which given the differences in language, personality, and status, would not have occurred in any other way. My whole feeling about this event was softened by another visitor to the Institute, Jean-Louis Basdevant, who, at the time was actively involved in computations of pion-pion scattering with the late Ben Lee. Jean-Louis, whose English is excellent, told me that Omnès composed elaborate stories, an episode per night, as bedtime tales for his children. I never saw Omnès again, but to my surprise Jean-Louis emerged from the exactly same office I occupied in the High Energy Physics Division at Argonne National Laboratory during my 1986-87 sabbatical when I visited the laboratory briefly again in 1989 to work with a collaborator. Jean-Louis recalled, amusingly, a lunch at the GB cafeteria with Francis Halzen, Peter Minkowski, David Speiser, him, and me. David remarked that Francis and Peter looked like the uniformed underlings of a super villain in a James Bond film in their identical, navy blue Izod-Lacoste polo shirts with their crocodile emblem, to which I brightly offered was manufactured by an American company. (Actually, René Lacoste was originally called the “Crocodile” in the American press so the USA factored into the brand and maybe into my subconscious.)

The coffee room provided me introductions to a lot of other people. I first met Raymond Gastmans there who often dropped in while he was spending
the year fulfilling his military service. Indicative of the poor health habits of the time, I remember him offering me what were without question the strongest cigarettes that I had ever smoked. If I recall, Raymond was very interested in computational methods with the latest technology, but I had yet to move into modern computing techniques after my earlier experiences with desk calculators and slide rules. Nevertheless, as proof of the assertion that old connections to Leuven never die, some of Raymond’s later work [4, 5] on new methods for calculating gauge theory scattering amplitudes played into the reference list on several papers that I wrote with Robert Brown and Stanley Brodsky [6] on radiation amplitude zeros as did one of Francis Halzen’s papers [7] which is one of the standard RAZ references.

To the best of my recollection, Raymond and I never crossed paths after July 1969, even when he spent a year in the United States. That was not the case with Francis who, in driving me to the Brussels airport in his Mini Cooper on the morning of my departure home, was the last Belgian to whom I spoke before leaving. Again of the time, the airport was small and security free and I proceeded directly to the Pan Am desk where I was greeted by the first American accent that I had heard since I put my family on a plane to Cleveland in London three weeks earlier. Francis came to the States a year or so later on his way to the University of Wisconsin and stopped in Cleveland and gave a talk at CWRU. He has been back here quite a number of times since the early 1990’s to give talks and attend conferences after our Department developed a strong particle astrophysics group. My professional overlap with Francis occurred intermittently since Leuven after serving on his Ph.D. thesis committee near the end of my stay. As kind of an inverse of that, Francis was on the Visiting Committee, along with Stan Brodsky (!), for the High Energy Physics Division at Argonne 18 years later when I was finishing my sabbatical there. Once I became involved in experimental particle physics in 1992, Francis and I had a number of interactions at Fermilab and at an SSC-hopeful conference in 1993 that spread from Madison to Argonne in its staging. Although we both followed trajectories that led from theoretical to experimental physics, perhaps by accident in both cases, Francis has made a very successful second career out of it. To my knowledge, the only paper on which Francis and I are listed as co-authors is a review [8] of the proposed FELIX detector for forward physics at the LHC which evolved, in small part, from some work reported by J.D. Bjorken, Cyrus Taylor and myself at the Madison-Argonne workshop [9].

I cannot recall whether I first met Peter Minkowski in the coffee room, at
lunch, or somewhere else. Wherever this meeting took place, it was early on in my stay and our subsequent interactions were then quite frequent throughout the year. It helped that our wives got along well and that I had a car, but despite this we never really had overlapping research interests. My lectures in the fall of 1968 were supposed to be on contemporary particle theory, but really were more concerned with various topics in scattering theory than anything else. Fortunately, dispersion-theory related things were very popular then as were various aspects of the few-body collision theory as applied to hadron physics. Peter was too polite to be critical, but, deep thinker that he is, he really was not interested in these things at this level as his subsequent very productive career has illustrated. Peter stopped in Cleveland for a few days on his way to spend a year at Cal Tech and that is the last time that I saw him.

Lunches connected with visitors, the late Robert Brout’s weekly lectures, or for no special occasion at all except, perhaps that someone gathered up a group who were hungry at the same time, were important to the scientific atmosphere of the Institute and contact with people that I could not meet with naturally in any other way. Luckily English was the default language. Brout was a fascinating person and his lectures were masterpieces of erudition and intellectual highlights of the year. Brout was the thesis advisor of my best friend then, the statistical mechanician and former CWRU colleague Michael Coopersmith, so I had some prior expectations of his personality and accomplishments. At the time, the Regge-pole theory of strong interactions was enjoying renewed success in connection with the duality phenomenology of Dolen, Horn, and Schmid; Brout spent most of his lectures describing theoretical techniques connected with this approach. I do not recall spontaneously-broken symmetries in gauge theories being mentioned at all in any context or in any venue by Brout, but my memory might have been blocked since these were not popular concerns in particle theory at the time and I would not have been primed to remember comments about them. Also, Brout and Francois Englert, who I met once at the what I recall as the winter All-Belgium theory conference, seemed to be working on things totally different from their seminal work in field theory in 1964, or on what was to become the substance of standard model, namely, gauge theories. At the theory meeting, for example, I recall Englert speaking about eikonal approximations in field theory. They also both seemed to be getting involved more in gravitational and cosmological formalisms, a trend that persisted for decades afterwards. Shortly prior to that theory conference Frans informed
me that Richard Dalitz had to cancel coming as the guest foreign speaker and he asked me to substitute for him since I possessed the major qualifications of being a foreigner, a putative theorist, and a pulse. I recall speaking not too memorably about means of implementing three-particle unitarity in hadronic collisions with chalk and blackboard which really makes it difficult, without the glories of PowerPoint, or even good old color-marked transparencies, to make anything, except something really good, look interesting by means of presentation entertainment. I survived, but I am not sure about my audience.

The audience for my second round of lectures in the spring was different, and smaller, from the those that attended in the fall. The subject was also quite different, namely the nonrelativistic three-body collision problem. Frans insisted that the lecture notes be written up, typed and duplicated and the Institute would supply the secretarial labor and duplicating. For those who remember, this was a tedious process involving typing on master sheets of some sort which could be then be duplicated on some primitive device. Error correction was difficult. Even as late as 1983 I recall going through this procedure for the proceedings of the Karlsruhe Few-Body conference. Word processing as we now know it now was non-existent. The Institute staff, whose names I have forgotten, were wonderful in helping me produce these notes which formed a starting point for a monograph that I wrote twenty years later with Sadhan Adhikari [10]. Francis and Peter, among others, were not interested in these things, but a number of graduate students seemed to be. In particular, those who were pursuing thesis work in scattering theory. Désiré was one of them and after some conversations we collaborated together on a paper [11]. Contrary to my advice to him to veer his interests more towards particle theory, Désiré continued to work on topics that applied scattering theory to a variety of physical situations and he did quite well in pursuing that agenda. He visited the few-particle group at the University of Maryland for a year in the late 1970’s and during that stay he also visited CWRU to give a talk which is the last time that I saw him, although through the years we remained connected via common collaborators in Tom Osborn and the people in the Nuclear Theory Group at Maryland.

There were a number of very interesting visitors to the Institute throughout the entire year. I have already mentioned Roland Omnès and Jean-Louis Baseviant. Most vivid to me besides those two were

- T.D. Lee who was among the most energetic and compelling speakers
that I have ever seen and heard. Whatever it was that he spoke about, the memory of it is obliterated by the image of a progressive strip tease from suit, tie, and shirt sleeves, as his body temperature rose. We had a great lunch in a very nice place; I had the impression of some sort of faculty club. Lee’s enthusiasm was contagious and everyone seemed inspired by it for longer than the usual half-life after he left.

- Rolf Hagedorn who spoke about networking the computers for the experiments and analysis at CERN. I don’t recall too much of the details, but this might have been some precursor to the internet. He seemed doubtful if it could be done effectively. I cheerfully responded from the audience that he need not worry, since the telephone system in New York seemed to function quite well despite its complexity; he gave this remark the silence it deserved. We have all come a very long way computationally since then.

- Jean Iliopoulos who gave a calm, clear talk, which, in retrospect, seems to have been concerned with the motivations for the what became the GIM mechanism. I can’t be sure about this, but I am confident that he did talk about weak interactions which was a bit unusual in that hadronic dominated era.

There were plenty of others, some whose talks that I may have missed for some reason, but they do not stand out in my recollection. It is a tribute to both David and Frans that under their direction so many first-class people were brought in to provide updates on their work and to interact with people in the Institute.

4 Other People, Other Connections

I was reminded for several years after my stay in Leuven of all the people who were there in 1968-69 and of some ancient features of the town by the charming Christmas holiday greeting cards that were signed by the entire Institute group. I was always touched when I got them. This was such a nice thing to do. Those cards and the connections with CWRU that persisted always reminded me of the relatively care-free days of 1968-69. For example, before the arXiv, when people sent preprints, Jean Cleymans never failed to put a few words of greeting on those that he sent me. I am not sure when or
where I first met Jean at the Institute, and I did not have too much scientific interaction with him then, but much later our interests overlapped a bit on the subjects of multihadron production and disoriented chiral condensates.

Raf Dekeyser and his wife escorted us to a concert in town as well as to the Rector’s reception in the old “Hall” early on in the academic year. Although my memories are vague in this regard, contacts with Raf seemed to occur throughout the year despite the fact that we had no scientific overlap. My wife and I appreciated the Dekeyser’s help in getting adjusted to life in Leuven.

A number of other former particle theorists from CWRU followed my stay in Leuven.

- Jerry Pietenpol, who was a PostDoc of Paul Kantor and to a lesser extent, Les Foldy, spent several years in Belgium.

- Cindy Bilchak, who was a former student of Bob Brown, came to Leuven for a year or two.

- Les Foldy visited during a summer a bit after my stay.

In the other direction:

- Roger Van Royen gave at least one talk at Case in the mid-1960s on the quark model.

- Although I was not in charge of our research grant at CWRU until 1971, I seem to recall that we offered Pierre de Baenst a post-doc position after my return, but he decided not to come after all.

There was someone, apparently a Professor who seemed in his mid-fifties, from the French-speaking part of the Geology/Meteorology Department in the old Institute building who showed up regularly to the coffee room. On no occasion except one did he ever speak to me, presumably because of language incompatibility. On that day, June 06, 1969, in a fairly empty building at the Institute since we were well into summer, I wandered in for some coffee and to my horror there was no in the room one except him and me. To my astonishment he came over and addressed me in nearly perfect English. It came to pass that he was a consulting meteorologist for the D-Day invasion in England on this day 1944 and was among those responsible for providing the weather forecast for deciding whether or not to go ahead with the landings.
It appeared as if he needed an American, with at least a nominal ability to appreciate the technicalities, to share this with for he never acknowledged in the slightest me again. I was profoundly moved by his evident emotion and I think of him, the coffee room, and the Institute, and the unsung heroes of the war every anniversary of D-Day.

5 Epilogue

Among the last times, maybe the last, that I saw Frans was at the Three-Body conference [12] in Birmingham in the summer of 1969, although we have had a number of communications over the years. The last time I spoke to David Spieser was at a Symposium [13] at CWRU in October of 1987 that he attended. He asked me what I thought, in retrospect, about my year in Belgium, and he seemed by his tone to anticipate a negative response. Whatever my exact words, I tried to tell him, in the midst of a noisy party at the Cleveland Museum of Art, that it was one of the best years of my life.

We have only visited Leuven once since leaving in 1969 and that was in 1983 as part of trip to visit the UK and to attend the Few-Body conference that was being held in Karlsruhe that year. The summer of 1983 was one of the hottest all over Europe and we saw not a drop of rain during our whole time in England, Belgium, and Germany, except for the brief moment when we stepped off the train from Brussels at Leuven station in the early evening and we experienced a shower of just right intensity and duration to remind us most poignantly of many days in 1968-69.


3. For example, F. Cerulus and A. Martin, Physics Letters, 8, 80 (1964).

4. R. Gastmans, lectures delivered at the 18th Winter School of Theoretical Physics, at Karpacz, Poland, 1981.


