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EXPLORING THE WORLD WITH MATHEMATICS

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Abstract. This is an account of my scientific and personal friendship with Prof. Andrzej (Andy) Aleksander Lasota from 1977 until his death 28 December, 2006. It is a tale that fascinates me because of the intertwined links between many people both East and West of several generations, and it illustrates what I feel is the strength and beauty of the personal side of the scientific endeavor.

This contribution is almost identical to the paper "Adventures in Poland: Having fun and doing research with Andrzej Lasota", Matematyka Stosowana 8 (2007), 5–32. It is in no way to be considered a new contribution, but is rather a record of the second Annual Lecture Commemorating Professor Andrzej Lasota given in Katowice at Uniwersytet Śląski on 16 January, 2009.

1. Prologue

On 28 December, 2006 Andrzej (Andy) Aleksander Lasota died of a heart attack. I lost one of my closest friends and a valued colleague who had a profound influence on my life and my career. What follows is a chronicle of my travels through science with Andy from the time I first met him in 1977. Based on my reckoning, we spent 471 days together over those 29 years, and they were full of fun and frustration for both of us.

In what follows I try to give feeling of the personal side of how our friendship and research collaboration developed and flourished and evolved over the years, as well as indicating some of the many scientific endeavors that we immersed ourselves in. I believe that my account is accurate[1].

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[1] It has been compiled from an extensive survey of my own daily diaries and my research notebooks.

2. Meeting Andy: Fall, 1977

My acquaintance with Andy Lasota started through a phone conversation with Jim Yorke sometime in the summer of 1977. Jim was aware of my interest in mathematical models for periodic hematological diseases [22], and told me he had a friend who was a Polish mathematician working with a hematologist in Kraków, Dr. Maria Ważewska-Czyżewska. He suggested I contact her, which I did.

Andrzej Lasota: January 11, 1932 to December 28, 2006
Picture taken in Michael Mackey's office, February, 1985

Following her invitation to visit, on the way back from a meeting in Varna, Bulgaria during the week of 3–7 October, 1977, I made my way to Kraków where I was met by Dr. Ważewska. Recently widowed (her husband, a physicist who had worked at CERN, had died of causes that I think were somehow related to his work) with five children ranging from teenagers down to preteens, I was welcomed into her household and there I stayed for several days. I soon realized that her background was definitely linked to mathematics since the room in which I was bunking out (I had displaced some of the boys who had to go sleep at their grandmothers' a few hundred meters away) was filled with advanced mathematics books all of which had the name "Tadeusz Ważewski" in the inside front cover. Indeed I was being hosted by the daughter of one of Poland's famous mathematicians of the twentieth century.

During the few days I was in Kraków, I was treated royally by the Ważewska family with much sightseeing organized by her two oldest teenaged children Olga and Nick. There was also a lot of interesting discussion about science, and the most astonishing for me was a paper [40] that she had written with her mathematician colleague, Andrzej Lasota, that had been published the year before. In that paper, starting from a time-age model for red blood cell development they had derived the differential delay equation

$$\frac{dx}{dt} = -\gamma x + \beta e^{-\alpha x_\tau} \qquad x_\tau \equiv x(t-\tau), \tag{1}$$

and studied aspects of it both analytically and numerically. This equation has also been dealt with in [2], and it is well known that the solutions $x(t)$ can either be a globally stable steady state or a globally stable limit cycle that arises through a super-critical Hopf bifurcation. My astonishment derived from the fact that Leon Glass and I had developed a model [22] for white blood cell production in 1976 that was also framed in terms of a differential delay equation given by

$$\frac{dx}{dt} = -\gamma x + \beta \frac{x_\tau}{1+x_\tau^n} \qquad x_\tau \equiv x(t-\tau). \tag{2}$$

In studying Eq. (2), now known as the Mackey-Glass equation[2], the goal had been to understand a periodic form of chronic myelogenous leukemia, a project that has engaged me for almost thirty years and which is just starting to be completed [3].

The solution behavior of Eq. (2) is much richer than that of (1), since one can either have a globally stable steady state, or a Hopf bifurcation to a simple limit cycle which can then show further bifurcations to more complicated limit cycles satisfying the Sharkowski sequence and displaying Feigenbaum scaling. Ultimately 'chaotic' solutions can ensue. We now know that this variety of solution behaviors and existence of multiple bifurcations is due to the non-monotone nature of the nonlinearity in (2).

I told Maria Ważewska that I would like to meet her collaborator, and she said that she had already invited him to dinner for the night of 10 October, 1977.

Andrzej Lasota–what a fateful meeting that was. Andy had come to Kraków from Katowice for dinner, and we spent a lot of time talking about a variety of things in spite of the fact that he was obviously anxious throughout the meal because his wife Elizabeth was pregnant (Natalia was born October

[2] The claim in the recent Polish translation [34] of Jim Murray's beautiful book *Mathematical Biology* that we were led to study equation (2) because of [40] is incorrect. Our paper was submitted to *Science* in December, 1976, and we were totally unaware of the work of Andy and Maria.

30, 1977). He also told me about a recent paper [5] in which he had considered an equation qualitatively identical to (2), but without knowing of our work published in [22]. It was of the form

$$\frac{dx}{dt} = -\gamma x + \beta x_\tau^n e^{-x_\tau}, \tag{3}$$

so the nonlinearity had the same non-monotone character as in Eq. (2). It was at this point that I realized that Andy was someone who I really had to get to know better–our interests were so close that it just made sense. When I went back to Montréal Andy and I corresponded several times (which was unusual, as I discovered later he was a lousy correspondent) and I finally applied to the Canadian government for a travel grant for him to come to Canada. The grant was duly approved and Andy came to Montréal for almost three months from 11 August to 24 October, 1978.

During the time he was in Montréal we started looking at survival data for patients with chronic myelogenous leukemia, and it was very curious indeed. Rather than showing an exponentially declining probability $p(t) \sim e^{-\alpha t}$ of surviving a time t after diagnosis (as would be expected for patients dying randomly) the actual survival data was very well fit by $p(t) \sim e^{-\alpha t^{3/2}}$. In the course of looking at this data and trying to understand what was going on, Andy started teaching me about how you could look at dynamical systems that had really irregular temporal behaviour from a statistical perspective using tools from ergodic theory. Because of my interests in questions about determinism and stochastic effects I found this absolutely fascinating, but very hard going.

After a lot of thought, toward the end of his stay in Canada we finally came up with an idea for a model that actually *predicted* the form of the survival data in the leukemia patients. We arranged that I would soon go to Poland to work on it further.

3. Early research: Spring, 1979

We wrote back and forth a number of times about our project during the period after Andy left in October, 1978, and eventually I planned to go and spend two weeks in Katowice, in May 1979 since I was on my first sabbatical. I arrived in Kraków on Saturday, 12 May and was surprised to be met at the airport not only by Andy but also by his wife, Elizabeth, who I had never met. I was shocked when I discovered that we were on our way to a funeral–we were on our way to the funeral of Maria Ważewska who had died of malignant melanoma a few days before. The funeral was in Kraków in the Church of St. Anna, the same church in which Andy's funeral was held on 6 January, 2007, just a week before his 75th birthday. A sad beginning to my second visit in Poland and little did I know how the circle would close so many years later.

The Lasotas, May, 1979

After the funeral, we went back to Katowice. I was staying in a student hotel on the university campus just across the road from the Mathematics Institute (I think it is now a student health service). Andy and I worked hard trying to put the finishing touches on our paper related to survival statistics in leukemia patients and made substantial progress in the first week that I was there.

After the first week my wife Nancy arrived with our three oldest boys, Fraser, David, and Alastair so the second week was spent partially working and partially doing sightseeing types of things. One of the first things we did when they arrived was to go to the Chorzów Amusement Park in Katowice with Andy and his wife and their daughter, Natalia, who was at that point age two. I also discovered that Andy had a playful side as shown in the picture where he is fooling around with Natalia's backpack carrier. At that point the Lasota family was living in a rather tiny apartment on one of the upper floors of an apartment block on Mieszka I Street, and Elizabeth hosted all of us for dinner more than once which I know was difficult for her. We also travelled to Kraków for a few days, and to my amazement Olga and Nick again acted as guides for the five of us which really was above and beyond the call of duty considering that their mother had just died. They also put us up at their apartment for the time we were in Kraków.

Fooling around

By the time we left Katowice by train for Prague on 26 May, Andy and I had a pretty good idea of how to explain the leukemia survival data and had even partially written a paper. For some unaccountable reason it took us many months until we actually finished the paper and submitted it in February, 1980. It was eventually published later that year [9]. Some years later John Milton and I were able to use the same approach to examine survival statistics in a number of other diseases [24].

4. Planning and writing our book: 1980 to 1985

4.1. College Park & Katowice

Later in 1980, Andy visited the University of Maryland in College Park to work with Jim Yorke. I flew down and spent the week of 20–27 April, 1980, and we covered a lot of ground both scientifically and personally. One of the things that we started talking about intensively was the success that Maria Ważewska had had in treating patients who had developed aplastic anaemic due to chemotherapy, radiotherapy, or exposure to certain organic compounds. We discovered we were able to come up with a reasonably interesting and straightforward physiologically realistic model for the process. In terms of dimensionless variables the model was formulated as a reaction-convection equation for the normalized red cell precursor density $u(t,x)$ at time t and maturation level x:

$$\frac{\partial u}{\partial t} + c(t,x)\frac{\partial u}{\partial x} = \left[p(t,x,u) - \frac{\partial c}{\partial x}\right] u(t,x),$$

where

$$c(t,x) = \begin{cases} x, & 0 \le x \le 1, \\ 1, & 1 \le x, \end{cases}$$

is the normalized cell maturation velocity and

$$p(t,x,u) = \begin{cases} \lambda(1-u), & 0 \le x < 1, \\ 0, & 1 \le x < L+1, \\ -\infty, & L+1 \le x, \end{cases}$$

is the normalized relative proliferation rate. L is related to the range of maturation levels. Using this model we were able to precisely explain the success that Maria had in treating her patients through a decrease in the cellular maturation rate which led to a minimization of the low levels of red blood cells during recovery periods.

Another topic of conversation that occupied us during that week was how to understand the behaviour of chaotic dynamical systems. I became more and more convinced that the ideas and the insight that Andy had into how to deal with deterministic systems that had quite irregular behaviour (chaotic in the current vernacular) was extremely important and was something that really needed to be communicated to the scientific establishment in a way that was more comprehensible than what mathematicians were accustomed to. We talked about this a great deal during walks around College Park and on the campus of the University of Maryland, and finally decided to try to write a book in which we would explain for mathematically sophisticated scientists (but not necessarily mathematicians) exactly how ergodic theory and the concepts from it could be married with dynamical systems theory to look at the statistical properties of chaotic systems. We thought that we would be able to write this book in two years, and with a great deal of temerity drew up a plan and both signed it with a pledge to finish on a certain date. In reality it took us more than five years of intense hard work to do it [11]. So much for plans.

I went back to Katowice in late September of 1980 for a week and we finished writing up the first draft of the work on Maria Ważewska's therapy that was finally submitted in May, 1981 and published later that year [18] with three authors, two living and one dead. During that week we also started to intensively work on the book that had been conceived of in College Part the previous Spring. We wrote an extensive outline of the first six chapters that, while offering us some guidance, was significantly different from the final product. It was also arranged that I would go back to Katowice in the Spring of 1981 and that Andy would come to Montréal immediately afterwards for a month.

It was on this trip that one of the most remarkable of many remarkable coincidences with Andy surfaced. One evening I was browsing through the myriad of books in Andy's study, and spied a book entitled *Differential Inequalities* [38] written by a fellow named Szarski. I was struck by this because when I had been an undergraduate at the University of Kansas I had been taught my first differential equations course by a Polish mathematician named Szarski who was on sabbatical for a year from Poland. I told Andy about this, and he looked at me in astonishment and said that Szarski, who had also taught him differential equations, had spent a sabbatical somewhere in the midwest at exactly the same time that I had been an undergraduate. He checked further with the people at Jagellonian University and indeed my prof in Kansas was the same Szarski. Andy made me a present of his copy of *Differential Inequalities* with a very nice inscription on the flyleaf and I treasure that book to this day.

During the early months of 1981 we sent drafts of portions of Chapters 1, 2, and 3 back and forth between Montréal and Katowice. One must remember that at that point in time we were relying on the quaint snail mail system–none of this emailing of Latex documents back and forth. Almost everything was handwritten, and numerous drafts were produced. I regret that in a fit of enthusiasm I threw out all of those a few years ago.

In any event, when I went back to Katowice at the end of May, 1981, we were well launched and both of us were really in the harness. By this time I was sufficiently friendly with both Andy and Elizabeth that they occasionally invited me to stay with them in their newer and larger apartment on Bocianów Street (where they lived from 1980 to 1988) across from the Kościuszki park, where I slept on a hide-a-bed in the living room. This apartment was like a dream compared to the previous place, and Andy had a commodious work place in the corner of the living room. We worked long hours in the week that I was there, with the work periods punctuated by lots of fun talk[3] about a variety of things between us as well as with Elizabeth. By the time I left on 3 June, 1981, I was exhausted but really felt that we had made some substantial progress. Andy came to Montréal a few days later for a month, again supported by a travel grant from the Canadian government, and we continued work ("like small f...ing devils" was one of his favorite expressions) interrupted only by a visit from Shui-Nee Chow who we invited up to Montréal to give a seminar in the Mathematics Department at McGill. Andy went back to Poland at the end of June, 1981 and I was supposed to go back for another intensive writing session for a month in January, 1982 (my teaching was always in the Fall semester, and over by the end of December) but other events intervened.

Shui-Nee and Andrzej
in my dining room, June, 1981

4.2. Martial Law: 13 December, 1981 to 22 July, 1983

On 13 December, 1981 the Polish people found themselves under the rule of martial law, which lasted until 22 July, 1983. Much has been written and debated about the wisdom and correctness of this action on the part of the government of Wojciech Witold Jaruzelski, but it is not my intention to get

[3] Andy loved jokes, and one of his favorites dates from that era. Two guys, Andrzej and Henryk were talking one day, trying to figure out who had invented Communism. Henryk said he thought it had been invented by scientists, but Andrzej said it wasn't possible since scientists would have tried it on dogs first.

into this debate–first I don't know anything about it and secondly this is about mathematics, and science and friendship. Regardless of the correctness of the act, it certainly put a crimp into the plans that Andy and I had made for me to go back to Katowice in January, 1982 for a month. Although I had applied for and won an exchange fellowship between the Polish Academy of Sciences and the National Academy of Sciences (USA) for a trip to Poland, my family was understandably not crazy about me going. However, after a few months I decided that it probably would be perfectly safe to do so, and thus I spent 12 April to 14 May, 1982 in Katowice living in a student dormitory (which is now the Hotel Asystencki, ul. Paderewskiego 32–32a) and working with Andy on writing.

It was an interesting month, and one in which I lost several kilos (which I could well afford to do) since I was living primarily on boiled cabbage, onions, potatoes, and carrots that I cooked on a hot plate in my room (which was quite nice). The dormitory was co-educational and men and women shared the showers, at least on the ground floor where I had been put (presumably to keep an eye on me). One morning as I was showering a girl pulled the curtain open and we stared at each other in astonishment for a few seconds before I had the presence of mind to shut the curtain.

During martial law there was a curfew at 22.00 and nobody was supposed to be on the street after then. One evening I had taken the tram back from Andy and Elizabeth's flat on Bocianów Street, and was walking from Kościuszki street along Powstańców over to my dormitory room, but it was after the curfew. I was sure that I would be OK, but as luck would have it I got stopped by a pair of young soldiers in front of the church Katedra Chrystusa Króla w Katowicach. I figured out they were asking for identification and so showed them my American passport–which was cause for great excitement. After cooling my heels for a few hours in some nearby headquarters I was finally seen by a very nice fellow about my age who spoke excellent English. After satisfying himself (I think) that I was not a spy he and a driver took me back to my dorm with the caution to be more careful in the future–as if he had to tell me! I never told Andy about this, but he did find out indirectly.

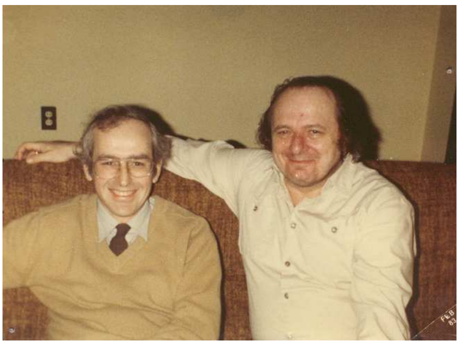
By this point in time we were well into the book and working on parts of what eventually became Chapters 4 (Studying Chaos with Densities) and 5 (Asymptotic Properties of Densities).

I finally went back to Montréal 14 May, 1982, just in time for my son David's birthday, and arrived home with large quantities of the first six chapters in handwritten form. Chapter 6 (The Behaviour of Transformations on Intervals and Manifolds) was the chapter in which we illustrated the material of the preceding chapters with specific examples, mostly for one dimensional transformations on intervals or the line[4]. Access to copying machines was

[4] Let $(X, \mathcal{A}, \mu)$ be a normalized measure space and $\mathcal{S} : X \to X$ a non-singular transformation that preserves the measure μ which has density f_*. As is the case when examining

strictly controlled during that period, so it was always the case that I had the only copy and I am sure that Andy had more than one nightmare about what would happen if my plane crashed. The usual procedure was that when I got back to Montréal I would make copies of everything and send them to him. Often the copy would never arrive, so we figured that someone in the police was trying to educate themselves about ergodic theory applied to dynamical systems!

Andy had been invited to Michigan State University in Lansing, Michigan, for the 1982/83 academic year, hosted by Shui-Nee Chow and T.Y. Li. This was excellent news for me, of course, because it is a lot closer to go to Lansing than it is to go to Katowice–and I hoped desperately that the year in Lansing would allow us to really break the back of the book and near completion. Such was not to be.



Andrzej (right) and me, April, 1983

Andy, Elizabeth and Natalia arrived in Lansing at the start of the MSU academic year, and took up residence in university housing on Cherry Lane, complete with the biggest and clunkiest old Oldsmobile that I have ever seen. Built like a tank, it drank gas like an alcoholic. I made several trips to Lansing that year (24 January to 3 February, 5–15 April, and 22 May to 4 June, 1983) to hopefully finish a good portion of the remaining parts of the book. However, we were stuck–dreadfully stuck–in what became the infamous Chapter 7 (Continuous Time Systems). We toiled over that chapter until I thought we were both going to either drop dead or

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the temporal evolution of single trajectories emanating from a given initial condition in a dynamical system, there can be a variety of dynamical behaviors of densities when evolving from an initial density. The weakest type of convergence is contained in the property of ergodicity. $\mathcal{S}$ is ergodic if every invariant set $A \in \mathcal{A}$ is such that either $\mu(A) = 0$ or $\mu(X \setminus A) = 0$. Next in the hierarchy is the stronger property of mixing. $\mathcal{S}$ is mixing if $\lim_{n\to\infty} \mu(A \cap \mathcal{S}^{-n}(B)) = \mu(A)\mu(B) \quad \text{for all } A, B \in \mathcal{A}$. Finally, we have the strongest property of asymptotic stability. Let $\mathcal{S}$ be such that $\mathcal{S}(A) \in \mathcal{A}$ for each $A \in \mathcal{A}$. $\mathcal{S}$ is asymptotically stable if

$$\lim_{n\to\infty} \mu(\mathcal{S}^n(A)) = 1 \quad \text{for all } A, B \in \mathcal{A}.$$

If P is the Frobenius-Perron operator corresponding to $\mathcal{S}$, then asymptotic stability is equivalent [11] to $\lim_{n\to\infty} \|P^n f - f_*\| = 0$, i.e., $\{P^n f\}$ is strongly convergent to f_*, for all initial densities f. If P is simply a Markov operator satisfying this condition then it is said to be asymptotically stable. The three dynamic behaviors of densities are related in that asymptotic stability implies mixing which implies ergodicity. The converse is not true. Ergodicity and mixing are properties that may be present in both dynamical and semi-dynamical systems. Asymptotic stability, however, is only possible in semi-dynamical systems.

kill each other. At one point Andy and I had a huge row (over what I can't remember) and I left the office and went to dinner. When I got back to my room at the Kellog Center I found Andy sitting on the corridor floor outside the door to my room with a bottle of vodka–so we made up over a few drinks, and the next day were back at it.

Throughout all of these visits Elizabeth was incredibly patient, putting up with us talking and writing at all hours of the night, cooking dinners, and generally making the way as smooth as possible for us. From my side, my own family was quite understanding about my long absences that year. In spite of all of the support that we both got at home, we did not make as much progress that year as we had hoped. Indeed, when Andy and his family went back to Katowice in the Summer of 1983 I think we were both slightly depressed and wondering whether or not we would ever manage to finish what had initially seemed like a straightforward project.

In our frustration with the book writing we turned back to doing some original bio-mathematics during that year. I had described to Andy the interesting cell kinetics statistics used to describe various properties of proliferating cellular populations. One of these is the fraction $\alpha(t)$ of cells that have not divided a time t after their birth. If $\psi(t)$ is the distribution of generation times, then $\alpha(t)$ is given by

$$\alpha(t) = 1 - \int_0^t \psi(x)\,dx. \tag{4}$$

Another statistic that was in vogue at the time was the fraction of sibling cell pairs whose inter-mitotic times differ by at least a time t. This is denoted by $\beta(t)$. We developed a simple model for the cell cycle based on three hypotheses.

(i) There is a substance (mitogen) produced by cells that is necessary for mitosis, and the dynamics of mitogen are given by

$$\frac{dm}{dt} = g(m), \quad m(0) = r;$$

(ii) The probability of mitosis $\phi(m)$ is a function of mitogen levels; and

(iii) At mitosis each daughter cell receives exactly one-half of the mitogen present in the mother cell.

With these three assumptions we were able to show that the evolution of the density of the initial amount of mitogen, $f(r)$ from generation to generation was governed by the integral operator

$$f_n(x) = Pf_{n-1}(x) \equiv \int_0^l k(x,r) f_{n-1}(r)\,dr, \tag{5}$$

where $k(x, r)$ is a stochastic kernel [11]. We were able to analytically compute $\alpha(t)$ and $\beta(t)$, and show that under very general conditions there was a unique fixed point $f_*(x)$ for Eq. (5) and the Markov operator P defined by Eq. (5) is asymptotically stable or exact [11]. Other consequences of the model that were in accord with experimental data were that the convergence often occurred within one cell cycle, that the correlation ρ_{ss} in cell cycle times between sister cell pairs is positive ($\rho_{ss} > 0$), and that the correlation ρ_{md} between mother and daughter cell cycle times is negative ($\rho_{md} < 0$). We submitted this paper during the Summer of 1983, shortly before Andy's stay in Lansing came to a close. It was later published [10], and a few years later Martin Santavy, Pavla Selepova and I were able to use this to understand a great deal of *in vitro* cell cycle data [27].

Shortly after Andy and Elizabeth went back to Poland at the end of the Summer, 1983, I went to Katowice for two weeks from 26 October to 8 November, 1983. I was again staying at the same dorm as in 1982. It was during this visit that Andy told me he had been interviewed by the police before departing for Lansing, and they had asked specifically about me. They had mentioned that I had been picked up past the curfew, and were curious to know why this Canadian guy with an American passport was spending so much time in Poland. I can't remember what he told me his response was, but it must have been satisfactory since they kept giving me visas to go back. It was on this same trip that Andy told me he had been elected to the Polish Academy of Sciences–something that he was immensely proud of and which clearly meant a great deal to him.

The cemetery adjacent to the hospital where Andrzej was confined with kidney stones in the Spring, 1984. The bench where most of the final version of the infamous Chapter 7 was written is just to the left of the stairs of the chapel

We were quite determined to get past the block that we had in Lansing, and thought that the best way to do it might be to work on some other things in addition to Chapter 7, which seemed to be dealing us such fits, and so we did quite a bit of work on Chapter 9 (Entropy). As it happens, this was one of the most significant aspects of the entire book writing project for me. The entropy chapter and the discussions we had while writing it touched deeply on interests I had had since I was an undergraduate and had to grapple with paradoxes about entropy changes in irreversible thermodynamics and the properties of the dynamical equations typically written down in physics. The writing of this chapter, and especially the proof of the theorems connecting the behaviour of the Gibbs' entropy with the property of asymptotic stability or exactness [11], had a profound impact on my research for over twenty years and continues to do so. It was the basis for a book that I wanted to write with Andy, but he declined saying that the area was too shrouded in controversy on the part of physicists. I eventually wrote it [21] and it was published in 1992.

We made good progress on Chapter 7 during that two week period, and were both sufficiently heartened that it was decided I should come back to Katowice in the new year since I had again applied for, and received, an exchange fellowship between the National and the Polish Academies of Science. The terms of that award, which was for a month, allowed me to split the visit into two portions–which I did–and the first visit was 15–28 February, 1984. We had been trading versions of the manuscript back and forth by mail, and there were parts of Chapters 7 and 8 (Discrete Time Processes Embedded in Continuous Time Systems) in existence. We worked mostly on Chapter 8 with some minimal attention to Chapter 7 during that two weeks.

The second portion of the visit was a few months later between 16 April and 12 May, 1984. When I got to Katowice I found a mess. Andy was in the hospital Szpital Kliniczny im. A. Mięleckiego (ul. Francuska 20, Katowice) being treated for kidney stones and hardly in any condition to do or write mathematics. Amazingly, however, we both managed to do both. Between his bouts of intense pain, we would discuss how to proceed and eventually came up with a way of dealing with Chapter 7 that we were both happy with. I was staying, again, in my favorite dorm on Paderewskiego and the modus operandi that we developed was for me to go talk with Andy during periods of lucidity, and then I would go out into the Cmentarz Ewangelicko-Augsburski which was next to the hospital and write among the grave stones while sitting on a bench. Then go back to the hospital to discuss with Andy, etc. We went through many iterations of this procedure, punctuated by trips with Elizabeth off to various places to buy medicine for Andy and some Żywiec beer that was supposed to be especially good for kidney stones. (I never actually believed this, but it helped him feel better.)

Andrzej working in his study on Bocianów Street, May, 1984 between bouts of kidney stones

In any event by the time I left Katowice in mid-May, Andy was out of the hospital and on the mend and we had really broken the back of Chapter 7. Thank goodness! We were so relieved. Before I went back home we had outlined much of Chapters 10 (Stochastic Perturbation of Discrete Time

Systems) and 11 (Stochastic Perturbation of Continuous Time Systems) and it seemed like the writing of the material in those was much more straightforward than what we had been dealing with earlier. That four week period in April and May, 1984, was one of the most intense I have ever had work wise. On this trip, as well as some of the previous ones, we had amused ourselves by trying to figure out a good title. We finally settled on *Probabilistic Properties of Deterministic Systems.*

Again, after my return to Montréal, we started sending material to each other and by the time I returned to Katowice for two weeks 5–17 June, 1985, the whole book was pretty much complete. We also had had a firm commitment of interest from a publisher (Cambridge University Press), had signed a contract, and we spent the two weeks I was there really working like maniacs to put the entire manuscript into a form we were comfortable with and which could be sent to them. When I got back to Montréal, the manuscript went down to their New York City editorial office for editing which took most of the summer.

Toward the end of the summer we finally received the page proofs, and a copy was sent by registered mail to Andy and he actually received them. We read them completely, and many of our students did too, with Piotr Bugiel winning the prize for finding the most number of mistakes. Finally in the late Fall, 1985, we received the first copies of our book [11] and I don't know who was happier or more relieved–Andy or myself. Andy was in Montréal 5–19 February, 1986, courtesy of my NSERC grant and we had a grand celebration with all of my family in attendance. I had had two copies of the book bound in leather by a friend, and I gave one to Andy at that dinner. It is still in his study.

Celebrating the book publication,
8 February, 1986

During that February trip, an event took place that was quintessential Andy. One day after lunch at the McGill Faculty Club we were walking onto the campus next to Redpath Hall and a student in a racoon coat tried to give Andy a copy of the newspaper of the Socialist Workers Party. Andy went ballistic, yelling at the kid that he was so privileged (wearing a fur coat) and that he (Andy) lived in Poland and that it was definitely no socialist workers paradise. The whole incident ended with Andy chasing after this kid who dropped his newspapers in terror. Many who watched this performance were highly amused.

While Andy was in Montreal that February, we started looking at an interesting problem–namely how adding noise to a relatively uninteresting (from the evolution of density point of view) dynamical system could induce the property of asymptotic (statistical) periodicity. Let me explain more.

Asymptotic periodicity is highlighted by a theorem [4] first proved in a more restricted situation by [7]. It is related to constrictive Markov operators[5].

THEOREM 1 (Spectral Decomposition Theorem [4]). *Let P be a constrictive Markov operator. Then there is an integer $r > 0$, a sequence of nonnegative densities g_i, a sequence of bounded linear functionals λ_i, $i = 1, \dots, r$, and an operator $Q : L^1 \to L^1$ such that for all densities f, Pf has the form*

$$Pf(x) = \sum_{i=1}^{r} \lambda_i(f) g_i(x) + Qf(x). \tag{6}$$

The densities g_i and the transient operator Q have the following properties:

- *The g_i have disjoint support so $g_i(x)g_j(x) = 0$ for all $i \neq j$.*
- *For each integer i there is a unique integer $\alpha(i)$ such that $Pg_i = g_{\alpha(i)}$. Furthermore, $\alpha(i) \neq \alpha(j)$ for $i \neq j$. Thus the operator P permutes the densities g_i.*
- *$\|P^n Qf\| \to 0$ as $n \to \infty$, $n \in \mathbb{N}$.*

Notice from (6) that $P^{n+1}f$ may be written in the form

$$P^{n+1}f(x) = \sum_{i=1}^{r} \lambda_i(f) g_{\alpha^n(i)}(x) + Q_n f(x), \quad n \in \mathbb{N}, \tag{7}$$

where $Q_n = P^n Q$, $\|Q_n f\| \to 0$ as $n \to \infty$, and $\alpha^n(i) = \alpha(\alpha^{n-1}(i)) = \cdots$. The density terms in the summation of (7) are just permuted by each application of P. Since r is finite, the series

[5] A Markov operator P is said to be constrictive if there exists a set A of finite measure, and two positive constants $k < 1$ and $\delta > 0$ such that for every set E with $\mu_L(E) < \delta$ and every density f there is some integer $n_0(f, E)$ for which

$$\int_{E \cup (X \setminus A)} P^n f(x)\, dx \leq k \quad \text{for } n \geq n_0(f, E).$$

This definition implies that any initial density, even if concentrated on a small region of the phase space X, will eventually be smoothed out by P^n and not end up looking looking like a delta function. Notice that if X is a finite phase space we can take $X = A$ so the constrictive condition looks simpler:

$$\int_E P^n f(x)\, dx \leq k \quad \text{for } n \geq n_0(f, E).$$

$$\sum_{i=1}^{r} \lambda_i(f) g_{\alpha^n(i)}(x) \tag{8}$$

must be periodic with a period $\tau \le r!$. Further, as $\{\alpha^n(1), \ldots, \alpha^n(r)\}$ is just a permutation of $1, \cdots, r$ the summation (8) may be written in the alternative form $\sum_{i=1}^{r} \lambda_{\alpha^{-n}(i)}(f) g_i(x)$, where $\alpha^{-n}(i)$ is the inverse permutation of $\alpha^n(i)$. This rewriting of the summation portion of (7) makes the effect of successive applications of P completely transparent. Each application of P simply permutes the set of scaling coefficients associated with the densities $g_i(x)$. Since τ is finite and the summation (8) is periodic (with a period bounded above by $r!$), and $\|Q_n f\| \to 0$ as $n \to \infty$, we say that for any smoothing Markov operator the sequence $\{P^n f\}$ is asymptotically periodic or, more briefly, that P is asymptotically periodic.

One interpretation of (7) is that *any asymptotically periodic system is quantized from a statistical point of view*. Thus if n is large enough, which simply means that we have observed the system longer than its relaxation time so $\|Q_n f\|$ is approximately zero, then

$$P^{n+1} f(x) \simeq \sum_{i=1}^{r} \lambda_i(f) g_{\alpha^n(i)}(x).$$

Asymptotically, $P^n f$ is either equal to one of the basis densities g_i of the i^{th} pure state, or to a mixture of the densities of these states, each weighted by $\lambda_i(f)$. The limiting sequence $\{P^n f\}$ is, in general, dependent on the choice of the initial density f.

We investigated the properties of the system

$$x_{n+1} = S(x_n) + \xi_n, \quad n = 0, 1, \ldots, \tag{9}$$

where $S : \mathbb{R}^d \to \mathbb{R}^d$ is such that

$$|S(x)| \le \alpha |x| + \beta, \tag{10}$$

where $\alpha < 1$ and β are non-negative constants, and the $\xi_0, \xi_1, \ldots$ are independent d-dimensional random vectors that are all distributed with density g, i.e.

$$\text{prob}(\xi_n \in B) = \int_B g(x)\, dx, \quad B \subset \mathbb{R}^d \quad \text{Borel},$$

and g has a finite first moment

$$m = \int_{\mathbb{R}} |x| g(x)\, dx < \infty. \tag{11}$$

As we had shown in [11], if one examines the evolution of densities under the action of 9, $f_{n+1} = Pf_n$, the corresponding Markov operator is given by

$$Pf(x) = \int_{\mathbb{R}^d} f(y)g(x - S(y))\,dy. \tag{12}$$

Our first result was

Theorem 2. *If the transformation* $S\colon \mathbb{R}^d \to \mathbb{R}^d$ *and the density of the distribution of the stochastic perturbation respectively satisfy* (9) *and* (11) *then the Markov operator defined by* (12) *is constrictive.*

Consequently the addition of any stochastic perturbation with a continuous distribution to a deterministic transformation on $\mathbb{R}^d$ will make that transformation asymptotically periodic from a statistical point of view. We illustrated this behavior with a stochastically perturbed Keener map, and then went on to generalize the results in the rest of the paper. An exposition of this can be found in [21, Chapter 10]. This work was published that year [12], and later Nick Provatas and I were able to extend the study [36] as well as study an inherently asymptotically periodic system in some detail [35].

5. Oxford: 1986/87

In the 1986/87 academic year I was on my second sabbatical, and the first six months were spent at the Mathematical Institute in Oxford at Jim Murray's Centre for Mathematical Biology on St. Giles' Street. Andy had never before been to England or Oxford, and said that he would like to visit me while there. We had rented a large house, and so we had room to spare for Andy who stayed with us during his visit 8 October to 8 November, 1986–a visit that was supposed to last for two months. I had arranged payment of his airfare and living expenses out of my research grant, but we had to wait some time for the bureaucracy at McGill to grind out the check and send it to Oxford.

Andrzej Lasota in my office, Centre for Mathematical Biology, University of Oxford, November, 1986

The working conditions at Murray's Centre were ideal, and we shared my large office working to clean up some objections of a referee to ourpaper [12].

We also started some new work which was, in a sense, an extension of what we had done in [12] and which would eventually lead to a divergence in our scientific interests. Namely we considered a stochastically perturbed discrete time dynamical system of the form

$$x_{n+1} = S(x_n, \xi_n), \quad n = 0, 1, \ldots, \tag{13}$$

where S is a deterministic transformation defined on a subset $A \times V$ of $\mathbb{R}^d \times \mathbb{R}$ with values in A and the ξ_n are independent 1-dimensional random vectors with values in V. Given some technical assumptions on S and the ξ we were able to examine the convergence of measures under the action of (13) and show when the convergence was unique. We also showed how this system could be interpreted as an iterated function system.

During the month Andy was in Oxford we worked incessantly, both at the Centre and at home, often long into the night in the kitchen, and my family was remarkably gracious about it. I think that this is one of the things that made working with Andy so unique–the fact that we both felt we had license to talk to each other no matter when, and that our time together was too precious to waste. About half way through the visit, the check from McGill finally showed up and I helped Andy cash it so it was in pounds sterling. To my astonishment, and the astonishment of my family, the next morning he announced that he was terribly homesick and was going back to Katowice immediately. I found this highly irritating.

Andrzej, Oberwölfach, March, 1987, during the Mathematical Biology meeting

When I took Andy into Heathrow Airport to fly back to Poland we had very sharp words about his precipitous departure, and it was clear that it really affected him. On his return, I wrote him asking if he would travel to the Mathematical Biology meeting in Oberwölfach the following February (1987) if I could wrangle an invitation from Karl Hadeler who always organized the events. He actually agreed and we duly met up there 16–20 March, 1987, with me travelling from Bremen (I spent the last 6 months of my sabbatical there in the Institute für Theoretische Physik at the Universität Bremen) with my friends Uwe an der Heiden and Helmut Schwegler.

Andy and I hardly slept that week we talked about so much and worked so hard. In addition to trying to put the finishing touches on the work started in Oxford we discussed a variety of other issues. One of the things we talked a great deal about was what we called "two function dynamics". We were trying to understand how to set up a new type of dynamical description for a situation like cell division in which at some point in phase space the mother cell divides and ceases to exist but two daughter cells take her place. We didn't get very far with it, but we did do quite a few numerical experiments trying to understand what the statistical properties of such a system might be. I think that later Andy did some work with Jim Yorke on this, but I don't know if anything was ever published from it.

The other new thing that we were looking at was how to generalize the definition of mixing for transformations S that were not measure preserving. We came up with a couple of ways of looking at it for a σ-finite measure space X and a transformation $S : X \to X$. The first was:

Definition 1. We say that S is *LM1 mixing* if and only if for all $A, B, C \subset X$ such that $\mu(A), \mu(B), \mu(C)$ are non-zero and finite we have

$$\lim_{n\to\infty} \frac{\mu(S^{-n}(C) \cap A)}{\mu(S^{-n}(C) \cap B)} = \frac{\mu(A)}{\mu(B)}. \tag{14}$$

Remark 1. This reduces to the normal definition of mixing if we add the property of measure preserving, i.e. consider $B = X$ so $\mu(X) = 1$. Then $\mu(S^{-n}(C) \cap B) = \mu(S^{-n}(C) \cap X) = \mu(S^{-n}(C)) = \mu(C)$ so we have

$$\lim_{n\to\infty} \mu(S^{-n}(C) \cap A) = \mu(A)\mu(C).$$

Example 1. The modified baker transformation

$$S(x, y) = \begin{cases} (2x, \frac{1}{4}y), & (x, y) \in [0, \frac{1}{2}) \times [0, 1], \\ (2x - 1, \frac{1}{4}y + \frac{1}{2}), & (x, y) \in [\frac{1}{2}, 1) \times [0, 1], \end{cases} \tag{15}$$

is not measure preserving, but it is LM1.

Remark 2. This definition of LM1 is good for situations in which the contraction or expansion of a set by iteration is independent of the set.

We also had a second more general definition given in:

Definition 2. We say that S is *LM2 mixing* if and only if for all $A, B, C \subset X$ such that $\mu(A), \mu(B), \mu(C)$ are non-zero and finite there is a constant λ independent of C such that we have

$$\lim_{n\to\infty} \frac{\mu(S^{-n}(C) \cap A)}{\mu(S^{-n}(C) \cap B)} = \lambda. \tag{16}$$

Remark 3. In general λ will depend on A and B.

Remark 4. If $\mu(X) = 1$ and S is μ measure preserving, then LM2 is equivalent to the usual definition of mixing[6].

We never pursued this problem very far other then doing a lot of numerical work looking at the nature of the attractor for a pseudo-Henon map of the form

$$S(x,y) = (4x(1-x), \alpha x + \beta y), \quad \alpha, \beta \in (0,1].$$

Andy and I also managed to finish our Oxford work during the week in Oberwölfach, and we submitted the paper in April, 1987. This work was eventually published [13], and I believe that this probably was the point in time that Andy became so fascinated with the properties of iterated function systems. I personally found them mathematically interesting, but felt that they bore absolutely no relation to anything in the physical or biological world, and therefore was not terribly interested in pursuing their investigation.

[6] This is easy to show if we take $B = X$ since B is arbitrary. Then we have

$$\mu(S^{-n}(C) \cap X) = \mu(S^{-n}(C)) = \mu(C),$$

because of measure preservation, so (16) becomes

$$\lim_{n\to\infty} \mu(S^{-n}(C) \cap A) = \lambda\mu(C).$$

Then we may write

$$\lim_{n\to\infty} \mu(S^{-n}(X \setminus C) \cap A) = \lambda\mu(X \setminus C).$$

However, the left hand side may be rewritten in the following way:

$$\begin{aligned} \mu(S^{-n}(X \setminus C) \cap A) &= \mu(X \setminus S^{-n}(C) \cap A) \\ &= \mu(A \setminus S^{-n}(C)) \\ &= \mu(A) - \mu(A \cap S^{-n}(C)), \end{aligned}$$

so

$$\lim_{n\to\infty} \mu(S^{-n}(X \setminus C) \cap A) = \mu(A) - \lambda\mu(C),$$

and thus

$$\begin{aligned} \mu(A) - \lambda\mu(C) &= \lambda\mu(X \setminus C) \\ &= \lambda\left[\mu(X) - \mu(C)\right] = \lambda\left[1 - \mu(C)\right], \end{aligned}$$

so $\lambda = \mu(A)$ and we recover the usual definition of mixing:

$$\lim_{n\to\infty} \mu(S^{-n}(C) \cap A) = \mu(A)\mu(C).$$

6. Lublin: 1987/88

At this point in time, Andy was having serious problems with his health–notably cardiac problems as well as problems with his sinus' that necessitated the first of several operations to try to correct the problem. He talked incessantly about how bad the air in Katowice was for his health (which was true–the pollution was staggering by anybody's assessment) and how he had to go live in a healthier climate. He had arranged a new position at the University M. Curie Skłodowska in Lublin, and this lasted for the 1987/88 academic year. I had the definite impression that the Lublin year was not one of the best for the Lasota family, and I know Andy was intensely unhappy that Elizabeth did not want to follow him to Lublin.

Henryk Gacki and Andrzej in Lublin, April, 1988

Janusz Traple, 17 April, 1988

I was back in Poland 5–24 April, 1988, courtesy again of the Polish and National Academies of Science with another exchange fellowship and livingin yet another dorm–but this time I was visiting Andy in Lublin with some time in Katowice (13–16 April with a visit to Kraków to talk with Janusz Traple). During that period we covered a huge range of topics. A partial list is as follows, and I give the list in such detail only to give the reader a sense of the breadth of topics we talked about and were interested in.

- We spent a considerable amount of time discussing the interesting statistics of single ion channel open and closed times. We had the idea of using chaotic maps with some of the same dynamics we had used to explain the non-exponential survival statistics in leukemia in our first paper [9].
- When I was on sabbatical in Bremen in 1986, Helmut Schwegler and I had started to think about how one could look at the dynamics of maps in which

the dynamics were density dependent. We were motivated by the Bohm [1] rewriting of the Schrodinger equation

$$i\hbar\frac{\partial\psi}{\partial t} = -\frac{\hbar^2}{2m}\nabla^2\psi + V\psi$$

by setting $\psi = \sqrt{f}e^{i\mathcal{S}/\hbar}$, with $\psi^* = \sqrt{f}e^{-i\mathcal{S}/\hbar}$ so $\psi\psi^* = f$ and f is a density. It is a simple series of calculations to show that f and $\mathcal{S}$ satisfy the pair of coupled partial differential equations

$$\frac{\partial f}{\partial t} + \nabla\cdot\left(f\frac{\nabla\mathcal{S}}{m}\right) = 0$$

and

$$\frac{\partial\mathcal{S}}{\partial t} + \frac{(\nabla\mathcal{S})^2}{2m} + V(x) - \frac{\hbar^2}{2m}\frac{\nabla^2(\sqrt{f})}{\sqrt{f}} = 0.$$

The reason that density dependent dynamics like this are potentially interesting is because of the following. If you think about the sequence of solution behaviors potentially available through bifurcations in dynamical or semi-dynamical systems it is typically:

stable steady state $\rightarrow$ simple limit cycle $\rightarrow$ complicated limit cycle
$\rightarrow$ 'chaotic' solutions.

If, on the other hand, one thinks about the bifurcation structure in the evolution of sequences of densities under the action of a Markov or Frobenius-Perron operator it is:

stable stationary density $\rightarrow$ simple asymptotic periodicity
$\rightarrow$ complicated asymptotic periodicity,

so the clear question is "How could one construct an evolution operator for densities that would display a 'chaotic' evolution of densities?". Markov and Frobenius-Perron operators are linear, so the suspicion is that in order to have a chaotic density evolution it would be necessary to have a non-linear evolution operator. The type of density dependent dynamics derived by Bohm suggested that it might be worth pursuing. Andy, Henryk Gacki and I spent a lot of time discussing various types of density dependent maps based on this idea, but really never came up with anything.

- Another question that we spent a considerable amount of time discussing, partially with Ryszard Rudnicki, was whether or not you could find the property of asymptotic periodicity, that we had studied [12], in continuous time systems. Andy and Ryszard were of the opinion that it was impossible.

Their reasoning was that in discrete time systems which are asymptotically periodic there is a shuffling (permutation of coefficients) between orthogonal densities g_i that have disjoint support. However, they felt that in an continuous time system it would be impossible to have a smooth transition of densities. We now know that a phenomenon precisely analogous to asymptotic periodicity does occur in numerical studies of ensembles of differential delay equations [20].

- When we were in Katowice 13–16 April Andy, Krzysztof Łoskot and I started talking about how to understand the coexistence of cellular populations. We considered the densities $u_i(t,x)$ of two populations of cells, both competing for a resource, whose dynamics were described by a time (t)-maturation (x) model framed as reaction convection equations. We were able to obtain local stability results for the coexistence of the two populations based on the parameters in the problem, and the paper was eventually published [8] a few years later.

7. Back in Katowice

By the Fall, 1988, the Lublin experiment seemed to be over, and from 25 October to 14 November, 1988, I was visiting Andy in Katowice, staying again at the dormitory on Paderewskiego. Also by this time the Lasota family had moved from the apartment on Bocianów Street to a larger and more commodious flat on Marie Skłodowskiej Curie Street that had a separate room for Andy's study and his (by now) considerable library of science and mathematics books. They lived there from 1988 to 1999.

A student of mine, André Longtin, and I had been thinking about the effects of both additive and parametric noise in the radial equations, specifically the effects of noise in the one dimensional systems

$$\frac{dx}{dt} = x(c - x^2),$$

and

$$\frac{dx}{dt} = x(c + 2x^2 - x^4).$$

Andy got interested in this, and after a lot of hard work we finally were able to prove conditions under which the solutions $f(t,x)$ of the corresponding Fokker-Planck equation existed and were globally asymptotically stable by a combination of previous results and using Liapunov functions. This was partially written up on that trip, and then finished and submitted shortly after my return to Montréal, to be published not too long after [23].

This was in the middle of a very difficult period for all of my family. In December, 1989, our oldest son Fraser (21) had been diagnosed with Ewing's sarcoma which he survived for another three years[7]. His treatment was difficult and terrible for him, though he never once complained, and I did a mere fraction of the work and travelling that I had done before.

One of the events that I had been working on long before Fraser's diagnosis was a month long workshop at the IMA (Institute for Mathematics and Applications) in Minneapolis. The focus of the workshop was differential delay equations, specifically in biology, and the roster of the participants (which included Andy) looked like the Who's Who of the field. I was loath to go for the month, but Fraser insisted and so off I went from 19 March to 13 April, 1990.

Late in 1989 Cambridge University Press had told us that they would not reprint our book since the sales had been quite miserable. In fact the sales were miserable (1200 copies in total) simply because their advertising was so miserable. In any event, Andy and I hatched the idea of buying the rights from Cambridge, and rewriting the book with the goal of having a publisher like Springer-Verlag re-issue it. I approached Rudiger Gebauer who, at the time, was the Mathematics Editor in New York, and he was enthusiastic. The month we were at the IMA was, I am afraid, another one of those periods in which we slept very little. Not only were we going to the workshop, talking to other participants, but we were also writing like small devils to clean up parts of the first book that needed work, and also adding new material.

This second edition of the book occupied much of our energy during this workshop as well as during three subsequent trips to Poland, the first of which was 28 October to 10 November, 1990. Most of that two week period was spent on the book, but I also started some other work with one of Andy's students, Ryszard Rudnicki (more about that later). It was also at this point that Andy and I returned to extensions of our original work on cell cycle statistics with questions triggered by work that Andy's student Joanna Tyrcha was pursuing for her doctorate. We had a number of good ideas that came to us, some in conversations with Joanna and some by ourselves. We soon realized that we could develop a general framework for looking at the statistical dynamics of what we called irregular biological events, and made some substantial progress. Joanna Tyrcha came to Montréal in June, 1990, to work on it some more with me, and then I was back in Katowice 28 October to 10 November, 1990, staying in the Dormitory Dom Studenta Nr 7, ul. Studencka 16, Ligota. Andy, Joanna and I finished our paper, which I submitted on my return to Montréal. It was published a couple of years later [17].

[7] Fraser died 7 November, 1992.

We made good progress on the second edition that trip, and it was followed by the two of us meeting at a workshop in Karpacz (16 February to 1 March, 1991) organized by Piotr Garbaczewski. Unfortunately, on the way to that meeting I was mugged and knocked unconscious on the train as it was pulling out of the Warsaw train station. All of my documents, wallet and money were taken so I was faced with a two week sojourn in Poland wondering how I would ever get back into Canada (or even get to Canada). Fortunately a number of the others attending the workshop were incredibly kind and I did survive. However, I think that I must have had a minor concussion since I had a horrible headache for the first week of the workshop. As one can imagine, I do not hold Warsaw dear in my heart, and indeed have never been back–nor do I ever intend to go back if I can help it.

My last trip to Katowice before publication of the second edition was 20–23 April, 1992. It was a very short trip, tagged on to a NATO committee meeting in Seville, because of the considerations at home. According to my research notebook, in addition to our talking about revisions of the book we also went back to a discussion of density dependent maps. Also, the exact title of the book seemed to occupy us a great deal and *Chaos, Fractals and Noise: Stochastic Aspects of Dynamics* finally won out over the clumsier *Chaos, Fractals and Noise: Understanding the Statistics of Nonlinear Dynamics* (thank goodness!). A second thing that we worked on was a contribution to a meeting being organized by Ovide Arino in Pau for September, 1992. I never made it to the meeting because of the situation with Fraser, but Andy did go and presented our work. Due to a variety of snafus most of the papers at that meeting were never published, and we eventually published it [16] in the proceedings of a conference on "Differential Equations with Applications to Biology" held in Halifax in July, 1997. This was Andy's last trip to Canada, and he was in Montréal 15–28 June, 1997 before we went to Halifax.

In the end, by the time the second edition [14] was published in 1994 under a different title (sexier according to Springer, but Andy and I never lost affection for the first title) substantial amounts of new material had been added (about 20% I think) and we had also added exercises at the end of each chapter to make it suitable as a graduate text. The sales with Springer were brisk with over 3,000 copies being sold and sales continue even today.

The one other thing that we packed into that short visit was to start a discussion about an idea that Andy had hatched concerning stability of cellular populations even if the intracellular biochemistry was dynamically unstable. Uncharacteristically, this work proceeded almost exclusively with us working at a distance, and the lions share was done during my 1993/94 sabbatical in Bremen and Oxford. The paper was eventually written and submitted in January of 1996 (so much for the efficiency of long distance working) and published much later [15].

8. Ronnie

No story about my work and friendship with Andy would be complete without mention of Ronnie, the boxer who became part of the Lasota family in 1990 and who was named after Ronald Reagan, the 40th president of the United States, who was much beloved by Andy because of his tough stance (along with Margaret Thatcher of the UK) against the Soviet Union.

Ronnie

By the time Ronnie joined the family I was usually staying with Elizabeth and Andy, sleeping on a hide-a-bed in the living room. Unfortunately, the hide-a-bed was also where Ronnie usually slept, and every night there was a tussle with me going to bed, being joined by Ronnie who I would dutifully push out. This back and forth went on several times every night until we reached a compromise–Ronnie slept in the bed with me.

This was not as bad as one might think, since Ronnie was a lovely dog modulo that (like all boxers) he slobbered incessantly. And he loved Andy without question. Andy and I would often take him out for a walk to clear our heads, or just to talk about what we were working on, and a favorite game involved a very noisy little dog (known as a MRT in our family) who lived near to the Lasota apartment. The dog would run back and forth on top of the wall bordering his yard (a couple of meters above the sidewalk level) barking incessantly at Ronnie. Andy and I would do everything we could think of to get the dog excited enough to actually jump off the wall so Ronnie could have a go at him–to no avail. However it did afford us considerable amusement and entertainment. Sadly, Ronnie died in 2002 and is greatly missed.

9. Post 'Chaos, Fractals, and Noise'

I returned to Katowice after the publication of our second edition of the book 20–29 April, 1996, and we spent the time in a variety of pursuits.

One of these was considerations of density dependent maps again. In my first attempts with Helmut Schwegler in 1986, and later in 1988 with Andy, we had concentrated on trying to look at a density dependent hat map

$$x_{n+1} = \begin{cases} a[f_n]x_n, & x_n \in [0, \frac{1}{2}], \\ a[f_n](1 - x_n), & x_n \in (\frac{1}{2}, 1], \end{cases}$$

where the functional $a[f]$ is defined by

$$a[f] = 1 + \int_A^{A+\delta} f(x)dx.$$

The corresponding nonlinear evolution (pseudo-Frobenius-Perron) operator is

$$Pf(x) = \frac{1_{[0,a[f]/2]}(x)}{a[f]} \Big\{ f\Big(\frac{x}{a[f]}\Big) + f\Big(1 - \frac{x}{a[f]}\Big) \Big\}.$$

This time, however, we concentrated on a modification of the r-adic map:

$$x_{n+1} = r[f]x_n \qquad \text{mod } 1$$

with

$$r[f] = 1 + \int_A^{A+\delta} f(x)dx,$$

and corresponding nonlinear evolution operator is given by

$$P_f f(x) = \frac{1}{r[f]} \Big\{ f\Big(\frac{x}{r[f]}\Big) + f\Big(\frac{x+1}{r[f]}\Big) 1_{[0,r[f]-1]}(x) \Big\}.$$

One thing was clear. If P_f^n is to be periodic then $r(f_n)$ has to be periodic, and the same comments apply in the case of chaotic evolution. One way to proceed would be to see if one could prove the existence of a fixed point f_* of the operator so $P_{f_*} f_* = f_*$, and then maybe try to linearize P_f around f_* and see if it is possible to derive conditions such that $\|P_f f - f_*\| > 1$, etc. We never made any progress on this.

I was in Poland from 19–28 June, 1998 and Andy and I traveled together to attend the Fifth International Conference on Mathematical Population Models in Zakopane, Poland 21–26 June, 1998. Curiously enough we did not talk about much science, except for a quite lengthy discussion concerning entropy convergence that was later to crop up in my work with one of his former students (see below). When we were not at the meeting I was staying with him and Elizabeth.

10. Work with students of Andy's

Along the way, in addition to working with Andy there also developed the marvelous opportunity to work with some of his former students. I have already mentioned the work with Łoskot [8] and with Tyrcha [17] as they were very much part of things I did with Andy.

However I have also had the pleasure of working with two other of his former students. In 1990, from 28 October to 10 November, 1990, when I was in Katowice working on the second edition of our book with Andy, I started talking intensively with Ryszard Rudnicki about some intriguing first order partial differential equations (reaction convection equations) that arise from a time-age-maturation cell model that have delays in some of the nonlinearities. Eventually these conversations led to the writing of a couple of papers that were published in 1994 [25, 37] and which I found quite pleasing.

Our second foray was in 1996 when I was visiting 20–29 April, 1996 and was the result of Ryszard's refereeing of a paper for me. In the process of doing this he realized that one of the earlier versions of [25] was a generalization of the paper he was refereeing, so we duly dusted off the old manuscript, improved it considerably and submitted it for publication which eventually happened some years later [26].

In the Spring of 2003 I was in Bremen at the Institut für Theoretische Physik and doing a lot of reading about vacuum fluctuations and pondering if they might act as an effective 'noise' source giving rise to irreversible behavior in apparently reversible systems. After doing some numerical simulations in which I perturbed a dynamical system with the trace [11] of a chaotic map [19], I discovered to my astonishment that the dynamical system appeared to have a Gaussian distribution of values along the trajectory, i.e. it was as if the Central Limit Theorem was operating.

When I was in Będlewo in 2002 for the School on Mathematical Modeling of Population Dynamics I met one of Andy's former students, Marta Tyran-Kamińska. While visiting Katowice 16–30 May, 2003, I showed these results to Marta. She immediately had an idea of how they could be understood and the result was a very nice paper in Physics Reports [28] followed by [31] and [32]. We have also managed to make some significant comments on outstanding questions revolving around the definition of entropy in non-equilibrium states [29, 30] as well as investigating dynamics in piecewise deterministic Markov processes [33].

11. Epilogue

So, dear and gentle reader, after walking with me through these memories you might well ask why Andy and I were so closely tied together for almost 30 years? Why did I spend 250 days of my life in Poland, Andy 105 days of his in Montréal, and the two of us 116 days in locations that were only temporarily home? What led us to do the research for 10 papers, write a major book and then revise it? Sadly I will never fully know Andy's reasons, but I believe they can be partially gleaned from an interview[8] he gave in 2001. There is an English translation[9] by Natalia Lasota.

From my own perspective, working with Andy was like a constant intellectual high. Andy was initially trained in physics before switching to mathematics, and I think that this is what contributed to his being a scientist and not just a mathematician. These are exactly the terms I would (and have) used to describe myself–I am a scientist. We had so many overlapping areas of interest in a variety of fields that it was uncanny. One, which many will find amusing and perhaps incomprehensible, is that Andy, like myself, was a strict determinist[10], and had a philosophical streak that resonated with my own. To have a glimpse of this see [6, 39].

And, too, and most importantly, talking and working with Andy was just plain *fun*. What better way can you spend your life than working on interesting problems, getting paid to do it, and having a good time in the process? How many times do you become so close to a collaborator that you stay in one another's home and have the freedom to talk to each other at any point, day or night, about something you are thinking about? It is at least rare, if not unique. I have had few collaborators in my life like Andy, and I value each one of them because of the intense intellectual and emotional satisfaction that I derive from the relationship.

In conjunction with the School on Population Dynamics (17–21 June, 2002) in Będlewo, Poland, on 22 June, 2002 there was a special day honoring Andy for his 70th birthday, which included talks by a number of his colleagues followed by a lavish dinner. My talk covered some of what I have related here. When it was over, Andy came up to me as we were going out to the coffee break with tears in his eyes, and simply said "Thank you". I think that my words told him just how much our years together had meant to me.

On one of my recent visits to Poland, Andy and Elizabeth invited me to join them and Henryk and Anna Gacki for dinner at the *Wünderbar* restaurant

[8] `http://gu.us.edu.pl/node/207651`

[9] `http://www.cnd.mcgill.ca/bios/mackey/mackey.html`

[10] I never managed to figure out how he reconciled this position with his religion. Nor did I ever manage to really figure out what his religion was. He once told me that I was lucky in that I was so steadfast in my atheist convictions.

to celebrate Andy's 74th birthday–the night of 11 January, 2006. It was a wonderful evening, and Andy was full of beans as only Andy could be.

As I have been putting these memories down in writing I have realized that though there was ample opportunity for Andy and I to initiate new work in the intervening years since the second edition of our book was published we did not really do so in spite of a number of enjoyable meetings. Why? I really do not know. Maybe Andy's intense interest in different things had taken hold, or maybe my changing interests had done the same. Most likely both are the case. I do know that I have felt a sense of loss over the past decade or more which has become irrevocable with his death.

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