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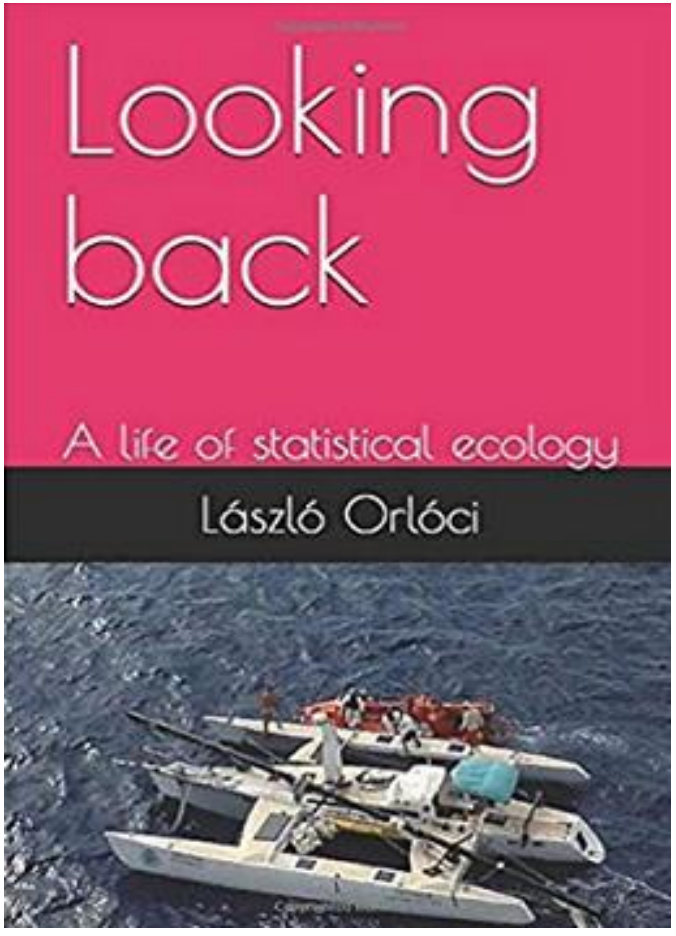

Looking back
A life of statistical ecology
László Orlóci

LOOKING BACK

A life of statistical ecology

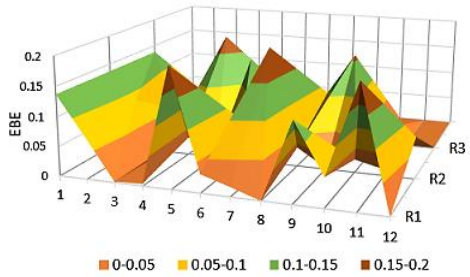
LOOKING BACK tells a personal story that begins in the '30s, a time of peace, order and prosperity for the author's family, and ends in 2012 when the idea of the book was first conceived. The story offers recollections of the pre-World War II period, the war years and their aftermath under Soviet occupation and ruthless dictatorial rule, the author's involvement in the 1956 Hungarian revolution that failed, and finally freedom and academic carrier in Canada. A substantial part of the narrative portrays the author's professional life framed by two questions. How did statistical ecology affect him? And, how did he, a forest engineer, affect statistical ecology? Seventeen sections take up selected topics interconnected but not in strict chronological order.

I KNOW I am narrating a different journey, but not one I would consider exceptional in the annals of my Alumni, the Sopron Forest Engineers. True, the responsibilities were demanding, the workdays long, and deadlines loomed like Damocles' sword. Yet, the sailing was reasonably smooth. How else could had it be? I had the unfailing support of Márta Mihály behind me every step of the way, and daughter Martha's ever-loving cheerful presence on my side. I sincerely thank them and offer to share with them all recognition that came my way.

Márta Mihály BSF, DFE and Martha B. Orlóci HBA, Honolulu 1986.

ON COVER FRONT … Rescue on high seas in the North Pacific, September 2014. Its single mast broken by hurricane force winds, sails gone and small engine idle without fuel. Still on even keel, the catamaran drifted for many days with men on board. When Royal Caribbean's Rhapsody of the Seas has come to dead stop a short distance from them, we were 1200 nautical miles from nearest land, about half way between San Diego, California and Honolulu, Hawaii. Men standing on the left hull received a large ovation from passengers lining the ship's decks several rows deep high above them. My photograph captures the moment of Rhapsody's tender pulling up alongside the disabled craft. Strong trade wind and agreeable sea currents compensated for time lost on the 200 extra nautical miles in detour. We docked at Hilo harbour of the Big Island in Hawaii on schedule.

COVER BACK … Granite surface from the Canadian Shield. Striations scratched into the rock show direction of glacial ice flow 16+ k years ago.



Virtual map of the emergent energy-based entropy cloud in a Coquihalla forest complex at Hope on British Columbia Mainland. Vertical scale in nats. Belt transect of 36 stands is portrayed. Energy-based entropy is a proxy scalar for the potential energy level in the complex. See page 166, https://www.amazon.com/dp/153716788X

LOOKING BACK

A life of statistical ecology

László Orlóci DFE, FRSC, MHAS

Western University, London, Canada

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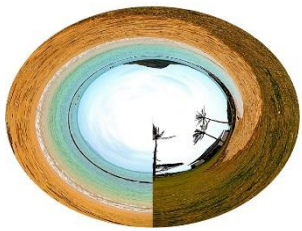

Shoreline and Mokoli'i (Chinaman's Hat, top) at low tide
on Oahu as seen in the picture folded in the direction of the centroid
into two dimensions.

Technical assistance: Kathryn Orlóci-Goodison, BSCEM

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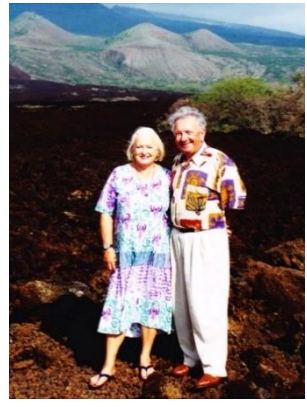
Looking back …

CONTENTS

Introduction



With Márta on Makena Road near La Perouse Bay on Maui in 1986. GPS: 20.604834, -156.424977

Statistical ecology? What is it? Simply stated, a set of powerful conceptual tools designed to verify the expected and to reveal the unexpected in natural complexity.

I am presenting an extended version of my talk at the telesymposium broadcast from the University of Guelph's studio on June 27, 2012.[1] The symposium was organised by Professors Madhur Anand and Kate S. He to honour me on my 80th birthday. I offer to Madhur and Kate my sincerest thanks.

The magnitude of the effort to put the symposium on the air surprised me. The symposium included participants scattered over several continents. I salute the organisers and panel members for a job well done. Panel members included Professors Madhur Anand, Enrico Feoli, Norm Kenkel, Kate He, Valério De Patta Pillar, János Podani, and Otto Wildi. I applaud each for the exceptional career I witnessed in progress after completion of studies with us at the University of Western Ontario.

I thought to make the symposium my last project in Science and considered retirement. But it did not happen that way. Márta's birthday present "*The dreams that stuff is made of*" and a student's question from Brazil changed my mind.

"The dreams …" is a book edited by Stephen Hawking. It contains, as Hawking himself puts it, "the most astounding papers on quantum physics"- on 1071 tightly set pages.

When I opened *"The dreams …"* I wondered what a forest engineer turned statistical ecologist can draw from top papers on quantum physics. I found it out after reading the very first paper, following Hawking's masterful introduction. The article was penned by Max Planck and published in 1901 under the title *"On the law of the distribution of entropy in the normal spectrum".* In this, *entropy* is energy-based, expressible in simplest by $-k \ln P$. The term *normal spectrum* implies the Normal frequency distribution of energy units, the

quanta, over the resonators of a complex. Max Planck puts energy-based entropy and the Normal distribution together and comes up with the idea that *energy-based entropy* is proxy for *potential energy*.

Planck's article reminded me of a question I posed to myself in 1969:

"What can I do with a scalar in form as simple as -ln P in studies of ecological diversity?"

When I posed the question, I had familiarity with Planck's name and I knew about the convertibility of energy and entropy. But I did not associate my problem with energy-based entropy. I was interested to solve an information theoretical problem in ecological diversity on the level of disorder-based entropy. I discussed the problem with David Goodall at G.P. Patil's symposium in the same year at Yale. David used the same form -ln P in his work with Ferenc E. Binet on a probabilistic similarity index. We did not see relevance for -ln P. Pressed by time, I let the idea pass.

The student's question came from my good friend Valério Pillar's lab at the Universidad Federal do Rio Grande do Sul in Porto Alegre, Brazil. I spent much productive time in Valério's lab doing research and teaching. The student whose name I lost, mentioned plans to study community energetic as I understood within the calorific flow frame offered by ecological practice. I thought of it and I knew what I should be doing with scalar -k ln P. [2] I made it the centre piece of my decade-long project on S*tatistical Quantum Ecology* (SQE).[3]

The time I spent on developing SQE became the main reason for years of delay in completing this account of the Guelph speech. I knew time is pressing and Márta too wanted me not to take more time out on ad hock projects, like SQE. But the temptation was too great. For my excuses I mention the success of the articles I completed on SQE, particularly the one I have done in 2015 under the title *"The energy-based entropy structure of natural plant communities. Causes, measurement, statistics".* [4] It have attracted more immediate reads on ResearchGate than any other of my uploaded work. In total, the count of reads adds up to a round 1100 on the first day of publication.

I chose the title *"Looking back. How I become a statistical ecologist"*. I planned to make the presentation personal, expecting that someday my story will interest the youth in the family. For this I had Márta's example to follow. She gives her reason in the book "Reflections"[5]:

"… What inspires me to open my soul and mind so wide for all to see the "I" in me? Age was an incentive. But more directly, my granddaughters Kathryn and Ruth Orlóci-Goodison expressed interest in my family roots and especially in me at my younger years. I felt elated that they want to know me better. The only possible way to fulfil their wish is to share my story."

The occasion to put into practice Márta's example arose sooner than I expected. My granddaughters Kathryn and Ruth Orlóci-Goodison were seating in the studio audience in 2012 with their mother, Martha Orlóci, in the company of Madur's and Kate's family members, faculty and students.[6] The present account has two main parts. The first is telling my story from childhood to 1969. Why 1969 and not some other year for my watershed year? It happened in that year at Kings College in Cambridge that the first time I was introduced as statistical ecologist. I did not care much for the designation. I did not consider myself a statistical ecologist. I shall explain why. The

second part tells the story how statistical ecology affected me, a forest engineer, and how I affected statistical ecology.

Growing up

I am from a military family. We lived a migratory life. Whenever my father's unit moved to a new location we moved too. This happened with regularity at least once every two years.

The prospect of pulling up stakes in one town and moving to another always excited me. Every new place offered a new universe for me to discover. But the part which fascinated me most of that universe, and still does, is outside the urban environment.

I remember well the frequent visits I took with friends as a 6-year old to the sand dunes just behind the house we rented in Nyírbátor. We moved there from Karcag, the first stop after Esztergomtábor where I was born in family housing on the Base in 1932.

I was 8 when we arrived at the next stop, Szilágysomlyó, in the mountains on the river Kraszna on our migratory path. My father rented a corner house at 2 Szent László street. For me, the house was an ideal choice, large backyard, close to the Beech forests on the Magura at one end of our street and to the riverside of the Kraszna at the other end. I remember local people, pointing high up to the patches of cloud touching the Magura, and saying questioningly something like this: "Uite, Magura fumează o eava". And when they sensed we have problem with the language, some would repeat the same thing in Magyar: "Nézzétek, pipál a Magura". In loose translation "Look, the Magura is smoking the pipe".

The winds of war were already blowing. I remember clearly the events of June 29, 1941. My mother was preparing to serve Sunday dinner, a midday feast with plenty of my favourite things. We three kids were seating with father at the dinner table listening to the 12 o' clock news. I still see my father's face turning from jovial to expressionless then to a sardonic small. It took a few moments before he said something like this: "This mad man. What a colossal mistake." The leading story that day shattered any hopes that Hungary will not be drawn into Germany's war. The regent of Hungary, Miklós Horthy, I later learned already in German custody, read Hungary's declaration of war on the Allies.

We left Szilágysomlyó early summer in 1942, moved to Kiskúnhalas close to the Serbian border on the Great Plane between the rivers Danube and Tisza. For me, the move meant changing scenery from mountains and riverside, to flatland with reed-swamps, sand dunes and alkali flats. Father rented a house on a large property at 21 Bükkönyös street, close to school and market. We were short walking distance to reed-swamps, sand dunes, alkali flats, and planted forest patches, across the rail tracks at the south end of our street. Father unit's barracks were outside town on the same side. Every morning a driver picked him up, and at the end of the day brought him home.

Kiskúnhalas was a summer-winter wonderland for active kids like me. We did it all, kayaking, fishing, swimming, and skating. Swimming was a challenge at first, but I learnt from others to keep to the clear water. There I could see the leaches and submerged alga beds to avoid them. We took to the skating trails

in season which crisscrossed the dense reed swamp.

I returned to the old house in Kiskúnhalas 56 years after we left town in 1944. The visit was not planned, just happened on a field excursion with colleagues from MTA (HAS) on the Kiskunság. We took rooms one night in the city at Motel Penny. In conversation with the proprietor the next morning I mentioned to her that my family once lived in the city on Bükkönyös street. She responded "Umm". I mentioned the house number, 21. Her face changed to a broad smile. "I own that house", she said. "It is just around the corner from here". I did not realise we were so close. We walked over. I recognized the house but felt disoriented. I turned to face the house closed my eyes and pointed to my right. "We had the butcher shop and grocery store that way and beyond the football field", I continued describing the inner arrangements of the three-segmented backyards. "I remember the large orchard fenced off back from the middle yard. My favourites were the plums, peaches and later in the season the delicious Kiffer pears. We had so much pear on the trees, we could not use them all. My Mother sold the surplus to the wholesale buyer just diagonally opposite on the street. We had to take the pears to his tent setup on the nearby football field."

The good lady smiled while I was reminiscing. "The property is still more or less that way. The grocery store and the butcher shop are gone. The football field is built up residential area" she said and continued. "There is one Kiffer pear tree left, probably the last in the city, if not in the region". We walked back to the motel talking. She wanted to know everything about life in Canada.

Soon, my colleagues were ready to start the day and we left town to that day's destination further south near Tompa. István (Pista) Beliczay, a fellow forest engineer and my classmate from Sopron, waited for us. He offered to be our guide on visit to two remarkable sites. The first is Pista's "Memorial Forest", his retirement project on 40 hectares of his own land. István established it to serve as lasting reminder for all who visits the place that there were two revolutions, 1948 and 1956, which defined the Hungarian character more than anything else in modern times. In 1848 the Hungarians rose against Austria's dominance. The 1848 revolution was victorious, but the 2-year long war of independence that followed failed. The 1956 revolution, in which Pista was very much involved, was fought against a suppressive communist regime and the Soviet occupation forces in Hungary. It is so ironic that in both fights Hungarian victory was denied by an overwhelming Russian force that entered the conflict on the tyrants' side.

The second site is called Őrdögárok (Devil's trench). It is a fossil meander of the mighty Danube from the 5th Century AD, long buried by sediments. Folk legend and local historians identify Őrdögárok as the site of a 5th Century Hun city where Attila the king of Huns ruled in his final years. In broad outlines the story spanned is not unlike Géza Gárdonyi's account in his 1901 historic novel "A láthatatlan ember" [7]. In English, "The invisible man [within us]". The locals fervently believe in the existence of a Hun city at Őrdögárok where Attila planed his last two epic campaigns against the Roman Empire. The first culminated in the 451 in the battle on the Catalaunian plains near the city of Orleans in 451. Attila's Huns and allies faced the roman le-

gions and their allies lead by Flavius Aetius, Attila's political and military mentor in his youth. The battle ended suddenly with the forces of Aetius leaving the field under the dark of the night. The second campaign a year later saw the Huns cross the Julian Alps. Aetius' Roman legions could not stop them from taking a series of Roman cities, starting with the strongest, Aquileia.

The local historians believe that Attila and his Huns returned to Őrdögárok from the 452 Roman campaign. Attila died there in 453 and had river burial nearby in a meander of the Danube. I asked Pista what he thinks, where did the Huns go after Attila's death? "Most of them stayed. Those are the Magyars".

At about the time when we arrived in Kiskúnhalas, the consequences of the family's migratory existence started to show in my school records. Every time we arrived in a new town, the kids from military families had to face the familiar problem of adjusting to a new city, and especially to a new school environment. It was easier for me to keep together with old friends, than to mix with 'locals'. I do not remember having had a local friend in town for the two years we spent in Kiskunhalas. 'Locals' were outside in my world populated with people from military families. It is quite revealing that I could not have enough of reading stories about field marshal Rommel and his Africa campaign or stories of the daring Otto Skorzeny of Gran Sasso fame. My father and fellow officers became my role model, and I started to see my life in the military. It was so easy not to put myself to learning Latin, History, Mathematic and other academic subjects.

I was entering seventh grade in September 1944, the 3rd year in our 8-year Gimnázium system. The Soviet forces have reached Szeged on the east side of the Tisza a sort distance east to Kiskúnhalas. My father's unit was fighting the Russians somewhere. The core moved to a new location further west in Transdanubia, and a month or so later to Amstetten in Austria.

Achtung!
Achtung!
Die luftlagemeldung![8] --
sounded the message on the radio the morning after our hasty debarkation the night before from the military train at the Amstetten Banhof[9]. Our apartment was on the 2nd floor, a single room of the Volksschule [10] on Preinsbacher Strasse near Don Bocce Kirche[11].

The Banhof itself was in bad shape when we arrived. Bomb craters and torn up rails, were in different stages of repair. The American bombers were visiting the city for some time but limited bombing to the Banhof area. The Germans rebuilt the main lines after each raid.

The warning on the radio continued "… Graz, Winer Neustadt, Vienna, St. Pölten, Amstetten …" as the air armada progressed on its bombing run. We were told when we hear Amstetten the sirens sound and we should go to the shelter. Our assigned shelter was a vast tunnel system under the adjacent highland with entrances, one adjacent to the backyard of the school at the end of Schulstrasse on the south east side. By 10 o'clock the American bombers appeared, dropped their deadly load, and left. We got used to the routine so much so that my brother, István, and I did not go to the tunnel on the sound of the sirens. We climbed up on the steep slope to the plateau used only by farmers and grazing herds. We had full view of the Banhof and surrounding city from there. I remember clearly one day we were ob-

serving the bombing. I trained my eyesight on an individual bomb and followed it all the way down to its target. It hit the base of a tall building, lifted it up and the building disappeared in a cloud of dust. When the dust cleared I saw only mound of rubble where the building used to stand. I imagined what that bomb could do to our school. It almost happened in the last raid by Russian planes in early May 1945.

One day when the siren started to sound we had to go with the others to the tunnel. My father had good reason for not letting us do otherwise. He heard the day before of plans for retreating forces taking up position on the plateau. That day the planes bombs were falling on the plateau too on the Banhof side.

The front was moving closer to Amstetten. We could hear the heavy artillery fire and see the flashes overnight from the direction of St. Pölten. I remember my father was suggesting to my mother that we should be ready to move on with others of his unit to Linz. He thought it will be better if we were in the western zone when the fighting ends. My mother became hysterical. She could not bear the thought that we may not be able to return to Hungary. We stayed in Amstetten.

The end of the war came for us on a bright day of May 1945. The city changed hands from German to British, and then to Russian. The German forces held the Russians back, but they did not put up much resistance to the British. Their light armour entered the city without a shot fired. As soon as we heard of the British in town, my brother and I went down to see them. By the time we arrived it was impossible to get close to the British neatly lined up along one side of the main square. They were throwing candies to the kids and just enjoying the jubilation of the citizenry. Not getting closer may have saved our lives. I still remember well the festive atmosphere. Then the unexpected happened. Russian aircraft flying low from the east started to strafe the British with heavy cannon fire. My brother and I ran to a narrow side street for protection. On the way back to our place we were wondering what on earth was really happening.

The Russian aircrafts returned that evening. Father, mother, two sisters, my brother and I were sitting at the dinner table in our second-floor unit when we heard the engine noise of low-flying aircraft. Just at that moment a tremendous blast shook the building and shattered the windows, throwing us on the floor. It was a Russian plane dropping its ordinance on the Roman Catholic nunnery diagonally opposite to us. Father was certain, our building was the intended target. It was a military depot.

Next day my brother and I went out to see what is happening in town. The British were gone, Russian heavy tanks and infantry were everywhere in the city. There were no welcoming crowds on the streets. The people stayed indoors and waited for instructions what they should do. It was then that I had my first encounter, one-to-one, with a Russian soldier. It cost me my boy scout packet knife, a compass, and my bicycle. He took them all. When I tried to resist, he kicked me in the pants, and quickly rode off, perhaps with more looting in mind. It was ridiculous to see the grown man with a machinegun across his chest riding my ¾ size bicycle. We were locked into the Russian zone of Austria.

The family returned to Hungary late May 1945 and settled down in Nagyréde, my mother's paternal village. It is a delightful little place of 3000 inhabitants, on the foothills of the Mátra Mountain. It

happened there that one day I went down to the main highway, 3 kilometres south of my grandparents' place. I wanted to see the endless column of Soviet soldiers on horse-drawn wagons of all kinds taken from the civilian population in Austria and Hungary, loaded with loot of all sorts, moving East. I thought back of the Russian soldier I met in Amstetten. Perhaps he was one of the drivers, taking my bicycle home with other loot. I hated him for what he has done. Then I thought he may have kids, perhaps my age he had not seen for years and wanted to surprise them with some present.

I went back to school after missing one academic year. I did poorly. Life did not start to turn around for me academically until 1948. It happened that year through the good graces of my godfather, Géza Dolentz, a forester himself and my father's comrade in arms in World War I, that I was selected to take the entrance examination for admission to the Forestry Gimnázium, a technical secondary boarding school in the historic city of Esztergom. I did well on the test and was admitted to the school in September 1948. In 1950 the school moved to Sopron, on the piedmont of the Alps, the home town of the Faculty of Forest Engineering where I started university in 1952.

When I arrived in Esztergom, I was returning to my birth place. It is an ancient city upriver from Visegrád on the Danube. Esztergom was the seat of the first Christian king of Hungary, Vajk, the son of Géza, who took the Christian name István at his coronation in 1001. The Magyars did not take kindly to Christianity. They resisted the fervent brutality by which their original culture was being decimated. But István forced the issue knowing well that it is the price the Magyars had to pay to survive as a nation state in the hearth of Europe.

Sopron was already a city under Roman rule but gained ultimate fame among Hungarians in 1921. Sopron was given the title "Hungary's Most Loyal City" when the citizens voted overwhelmingly to stay with Hungary, given the choice to become part of Austria.

The Forestry Gimnázium offered a 4-year course in forestry science, physical sciences, and humanities, in a semi-military boarding school setting. I found the environment just made for me. I liked the school's student-oriented atmosphere, the structured life, rich academic program, compulsory summer jobs, and the prospect of guaranteed intermediate middle management-level employment in the forestry profession. I applied myself to my studies and started to get good marks. Four fantastic years later I graduated with top marks in 1952. What a time it was for my father to see my record book. My brother, who was always top in his class, and already at university studying hydraulic engineering, my two sisters and mother, all agreed with father - I may get somewhere with my life.

At university

My outstanding graduation record from the Forestry Gimnázium guaranteed my acceptance to the Faculty of Forest Engineering at the University of Sopron. The university had three faculties at that time. Forest Engineering is still in Sopron. Mining and Oil Engineering moved to other location in Hungary.

I began the 5-year academic program in September 1952. I brought with me basic familiarity with finite mathematics, linear algebra, and of all things, parallel projective (or descriptive) geometry. These gave me a leg up in university

mathematics and the engineering courses. I also had good background in general botany, floristics, plant systematics and, of course, in environmental and forestry science. I felt equipped to do well as I immersed myself into the 5-year program.

1952 turned out to be a defining year in my life for yet another reason. It happened on the first day at the University that I caught site of a spectacularly beautiful, self-confident class mate, a forester's daughter, whose name was Márta Mihály, just before mine in alphabetical order. I found myself assigned to the same study group for labs, field exercises, tutorials, and other group activities. The years went by. We progressed in our studies and our relationship grew.

We were entering the first semesters of the 5th year in September 1956 when the unexpected happened. The Hungarian people's voice against the suppressive Soviet-installed regime, simmering for months, reached crescendo. The communist regime reacted with firing into the demonstrating crowds in Budapest. This triggered the peoples' nation-wide revolution on October 23, 1956. The regime collapsed, but freedom was short leaved. The Soviets retreated first, regrouped and on November 4th re-invaded Hungary with a fresh force of more than 60,000 Soviet troops equipped with thousands of heavy tanks. They crushed the revolution with brutality to teach a lesson. Ironically, and so typical of the suppressor, his solders were not told whom and why they were fighting. Many thought they were finishing the last act of the 2nd world war against the remnants of the Nazi forces. It is revealing that the troops were decorated with the same medal as the troops received after taking Berlin in 1945.

Márta and I were actively involved and expected reprisals. We decided to leave for Austria. On the way, I proposed to Márta and she accepted. Months later, with the Sopron group already in Canada, we got married in Abbotsford on March 17, 1957. The group became an Academic Division of the Faculty of Forestry at the University of British Columbia in Vancouver. Márta has written about this at length in an article *"Invited immigrants: the Sopron saga"*.[12] Her paper, a historic document for both the Canadian and Hungarian Governments, narrates the details as it traces the steps. The saga began with the Canadian Government's invitation delivered to the Sopron group by the good man, the Right Honourable J. Pickersgill, Minister of Canadian Immigration. We were at that time in Ferienhort on the Wolfgangsee in Salzkammergut in Austria. Most of the group accepted the invitation, we were among them. Soon we boarded one of CP's Princess class luxury liners in Liverpool for winter crossing the North Atlantic. We left on Liverpool New Year's Eve and arrived in St. John, New Brunswick, on January 8, 1957.

The invitation of a large academic group, including students, professors, some support personal and families, and allowing the group to continue functioning as an academic unit at UBC, was an unparalleled event in the annals of Canadian universities. The story attracted attention from news organizations across the World. TIME and other major news magazines published lengthy articles about the group. 'Sopron' became a well-recognized name any where we went. Gyurka Leskó my classmate and I found this out in Aleza Lake, a small hamlet eastbound from Prince George on the Upper Fraser Road in central British Columbia. We were on our summer job at Aleza lake on the first job and first

summer in Canada at the Experimental Station of the Provincial Forest Service at Aleza Lake. One day we went to town for supplies a few kilometres from the Experimental Station. The proprietor of the store, standing at the counter with a recent issue of TIME in hands, looked up at us, then looked back to his magazine page and turned the magazine page in Gyurka's direction and asked pointedly: "Is this you fellow on the picture?" Gyurka answered 'yes'. We shook hands, and with that became instant local celebrities.

The Sopron story lingered on at Aleza Lake for some time, just as it did in the headlines across the World, then interest faded. But luckily, our welcome lasted indefinitely in Canada. We were on the way to become young engineers determined to succeed. The historic record shows the Sopron group did it with flying colours.

In September 1957 we picked up our studies at the University of British Columbia where we left it off a year earlier at the University of Sopron. Márta and I both graduated with the Bachelor of Science degree in Forestry (BSF) from U.B.C. We received the document for our originally intended Forest Engineer Diploma (DFE) from our Sopron Alma Mater decades later in Hungary.

The demise of the communist dictatorship was complete, and the Soviet occupation of Hungary ended in stages between 1989 and 1991. Hungary and its peoples are free and thriving as rarely before.

I entered graduate school at UBC, completed MSc in 1961 and PhD in 1964, both under the guidance of Professor Vladimir J. Krajina. I am grateful to his memory. He taught his students more than Forest Ecology. The Ziva interview I gave a decade ago in Porto Alegre in 2002 sums up my feelings and expresses my deep respect for him. I paraphrase what I have said:

"I have learnt much from Professor Krajina in class, but he commanded my highest respect for his sense of freedom and courage, outspoken nature, fairness to others, unusual liberality of mind, and of course, a special kind of humour. All who knew him well should want me to mention his deep love for home land and his Czech nation, and important to his character, his exemplary citizenship in his adopted country for which he was made a Companion of the Order of Canada.[13]

Post-doctoral training

Two unexpected events steered me closer to becoming statistical ecologists. Still at UBC, sometime in the Autumn of 1963, the department head of Botany, Professor T.M.C. Taylor, asked me to drop by his office, as he had a sample copy of a book that I may like to read. It was a copy of "Numerical Taxonomy" by Robert R. Sokal and Peter H.A. Sneath published in 1961 by W.H. Freeman. Professor Taylor knew my background quite well. He also knew that I was doing some multivariate statistical analysis in my graduate research. He told me he would gladly help me to get the necessary funds to support me on postdoctoral training with Bob Sokal, if I were interested. Of course, I jumped on his offer, but I was not a trained taxonomist. I preferred post-doctoral training with Professor Peter Greig-Smith in quantitative ecology at the University College at Bangor in the United Kingdom.

I knew about Professor Greig-Smith's research interests from his 1957 book on "Quantitative Ecology". His brand of quantitative ecology appealed to me more than Numerical Taxonomy. Application done, NATO science fellowship

received, Márta and I rolled into Bangor in our Volkswagen on a bright Mayday in 1964. Professor Peter Greig-Smith welcomed us in his densely populated lab.

I wanted to experience all aspects of expertise in the lab. I decided to do a brief vegetation survey on the sand dunes of Newborough Warren on the Isle of Anglesey, a short drive from our rented home in Llanfair P.G. The survey kept me and sweet Márta in close contact with field botany and ecology, but often in weather we would rather not have. Frequent gale-force winds and the usual low temperature and drizzling rain forced us to wear pullovers and other protective gear. After about a month work, we got the data I wanted.

On the odd day, the skies would open over the sand dunes and we could see clearly the shores across Menai Strait in the direction of Carnarvon Castle. Not far to the East, on the highland, is the site of the 1st Century Roman auxiliary fort of Segontium. From there we could see the sand dunes of New Borough Warren visible on clear days by unaided eye. The Segontium story intrigued Márta very much. We were surprised what she managed to dig up in her readings. She wondered if Plinius the 2nd - who visited Segontium and mentioned Mona, the Isle of Anglesey, in his Naturalis Historia - had seen the sand dune system. As she found it out later, he could not. Romans abandoned Segontium more than a thousand years before the sea-currents carried the sand in quantity to the Anglesey shores.

By the time of our arrival in Bangor, the foundations of community level Statistical Ecology, often called Quantitative Ecology or Mathematical Ecology, have been laid. I mention a few examples that I find typical of the era: A.S. Watt's 1947 work on plant community pattern and process, P. Greig-Smith's seminal work on the scales of vegetation pattern and environmental determinants in 1952, D.W. Goodall's 1959 paper on "ordination", and the W.T Williams and J.M. Lambert papers on association analysis and other ecological topics in 1959. Simulation modelling as an operational tool was about to be introduced into plant ecological practice by David Goodall.

I certainly felt I could be original in my work, and I knew how. Professor Greig-Smith supported my choice of research topic. I did the research and completed the manuscript in 1965 for 1966 publication in the Journal of Ecology.[14] Chosen as a ISI Citation Classic, it was listed by Eugene Garfield's editor J.T. Barrett for portrayal in their 1986 volume on "Contemporary Classics in Science".

What made that paper a citation classic? Its seminal nature certainly helped. It provided an overview of ordination methods and suggested interesting innovations, among them the RQ duality principle. This widened the utility of eigenanalysis, the mathematical core of parsimonious ordinations, such as Principle Components Analysis, on the then available digital computers, such as our Elliott 803 available for us in Greig-Smith's lab.

The use of the duality principle came reasonably easy once I understood the problem to be solved. Simply stated, phytosociological data sets usually include very large numbers of species recorded in a smaller or much smaller number of quadrats. The objective is to get component scores for quadrats with less calculations. I realised this can be done in the most parsimonious way if the product matrix is computed for the lesser number of quadrats. But to get what I wanted required computational tricks that needed some familiarity with

the linear matrix algebraic concept of characteristic equations. I worked it out in 1964 and solved the problem of performing eigenanalysis on my huge Newborough Warren data set. I published it 1966. The solution's concise description is in a 1967 issue of Systematic Zoology.[15] This is what I did:

I use letters R and Q to designate dual symmetric product matrices. Using the ecologist's terminology, R is a species-centred product moment matrix of p species and Q is a species-centred product moment matrix of n relevés. As defined, $R=AA'$ and $Q=A'A$. In this A is the p x n data matrix centred by rows (species). The characteristic equation of R is $AA'B=\lambda B$. Based on this we compute component scores for the n relevés, $Y=A'B$. We can write $A'AA'B=\lambda A'B$. This is the same as writing $A'AY=\lambda Y$, which happened to be the characteristic equation of the product matrix Q. The component scores Y based on the Q matrix are thus the same as the component scores based on the **R** matrix. From there after, we could select the **R** matrix when p << n or the Q matrix when p >> n without affecting the outcome. I first used the RQ-duality principle in 1964 in the code that I wrote for input in PCA to analyse our Newborough data set. The data set contained much more species than relevés.

In 1969 I was invited to a group discussion in Kings College at Cambridge. Nicholas Jardine and Robin Sibson were presenting, with great gusto, their results on k-clustering in mathematical taxonomy. John Gower was there from Rothamsted Experimental Station in the U.K. I found out then his interest in solving the same problem I solved very simply and called it the RQ principle. He published a long paper on his solution problem in Biometrika in 1966, the same year as I did the first paper on the subject, "Geometric models …".

I owe much to Peter Greig-Smith for the opportunity he has given me to study quantitative ecology's fundamentals in his lab, and to meet the many talented people in this field. I count among them scientists' names that are now legendary. I had considerable overlap with Robert (Mac) McIntosh who was on sabbatical leave from Notre Dame University in the U.S. I had considerable overlap with Pál (Pali) Juhász-Nagy, a British Council Fellow from Hungary, and Mike Austin, a research fellow from the U.K. It was during my Bangor year that I met the first time Bob Sokal, Peter Sneath, Bill Williams, David Goodall, and Mike Dale. These people were in fact creating initial context in Quantitative Ecology and Numerical Taxonomy. With most, my professional life intertwined for many years to come.

On tenure-track in academe

I took on tenure track position in 1965 at the University of Western Ontario (now Western University) as assistant professor. My responsibilities included teaching, research, and some administration. I received research grants from NSERC and some travel grants from the University. I started to appear regularly at conferences, symposia, and workshops. I liked Márta to come with me whenever she could, and this was not changed after the birth of our daughter Martha Barbara in 1966. Martha too came with us.

As I progressed through the ranks, I could spend more time on the extramural research of my choice. One of the remarkable events of my career came early at the numerical taxonomy conference in 1966 at the Universidad National in Mexico City. Robert R. Sokal and F. James Rohlf were organizers. This was my first one-on-one contact with Sokal, who did not remember me from Great Britain, but knew about my Systematic Zoology paper already in process. Others I met were in the fore wave of developments in the new field, or at my stage, but ambitious and rising.

Bob Sokal invited me to a conference in 1968 he organized with James Rohlf in Lawrence at the University of Kansas. The conference was called to establish the North American Chapter of the Classification Society. The Classification Society already existed in Europe. The society's president, Peter H.A. Sneath, was attending from the University of Leicester in the U.K. I knew Sneath as a name from their text book, and I met him at the 1964 Botanical Congress in Edinburgh, Scotland, at which he gave, jointly with Sokal, a lecture on numerical taxonomy. I do not think he remembered me, but he knew my paper, and he may have been the one who suggested my name for the discussion session at King's College in 1969.

Sokal asked to present my thoughts on metrics, RQ duality, and whatever other topics I wanted to discuss, ad hock, at the Lawrence meeting. I did that and made some points. An interesting discussion followed. At the meeting, we accepted the motion from Sokal and formally launched the North American Branch of the Classification Society. I became the society's first treasurer.

Just before the Lawrence conference, Mike Dale, an acquaintance from my post-doctoral year in the U.K. was visiting me at Western. He had a stop-over with us on his way from the University of Sheffield, where I met him the first time, to a research position with Bill (W.T.) Williams' group at CSIRO in Australia. I took Mike with me to Sokal's conference. Mike was not invited, but already known, and well-received. The participants represented a good cross section of active researchers from numerical taxonomy and quantitative ecology. David W. Goodall was among them. I had much to discuss with him. I got to know his thoughts on ordinations, modelling, and his combinatorial work with F.E. Binet's help at CSIRO. I should mention, that David always knew it better and never would leave anything he did not like uncorrected.

David Goodall came to the conference from Riverside in California where he served for a sort time with Robert H. Whittaker in the same department. I am saying this, because I met Whittaker in 1970 at Professor Reinhold Tüxen's symposium on vegetation science in Rinteln, Germany. We were seated together and had lengthy conversations. I found that Bob, a person who left nothing up to chance, was always maximally prepared. He delivered perfect talks on relevant topics, using a deep, baritone voice. He was very much aware of his rank in science, yet very proud of his roots as a farm boy from Kansas at the Symposium's banquet. He was flattered when I complemented him, the farm boy who made it in big time science. He returned the compliment for my work and background. It was so ordained that Dave Goodall's name came up as a mutual acquaintance from quantitative ecology for which Bob expressed unexpected interest. I openly wondered how could, two personalities so different as David Goodall and Bob Whittaker, get along in a shared hyper-competitive environment, like Riverside. I was told later that their relationship was in fact rather vitriolic.

I received invitation to a landmark event in 1969. It came from Professor Dr. Ganapta P. Patil of Penn State, to speak at Yale University at the conference he was organising for statisticians and ecologists. I should express my tribute to G.P. for his life-long effort to help young scientists to showcase their talent and establish co-operation. A superb theoretical statistician, with a feel for applications and foresighted facilitation, G.P.

organised the Yale conference to bring together ecologists interested in statistics and statisticians interested in ecology. Through similar conferences which G.P. organised, statisticians and ecologists gradually learnt how to communicate. He spoke of my discipline as statistical ecology, and of his, as ecological statistics. He invited well-known quantitative ecologists to his conferences, among them Chris Pielou and David Goodall, and well-known statisticians such as M.S. Bartlett from the U.K. and C.R. Rao from India.

I presented a paper on information theoretical techniques in Yale. I was developing classification and pattern analysis on that basis. In my lecture, I mentioned Alfréd Rényi's remarkable paper of 1961, presented at the 4th Berkley Symposium on Mathematical Statistics and Probability. The paper was paradigm setting in diversity analysis, available for ecologists in the symposium proceedings.[16] Rényi develops generalization of disorder-based entropy and information, a parental class of C.E. Shannon's entropy function and S. Kulback's minimum discrimination information statistics. C.R. Rao stopped me in the hallway between sessions and asked for Rényi reference. I quoted it to him from the reprint I had. It was rather lengthy. I offered to write it down for him. "It will not be necessary" he said, "I remember it".

I mention another of G.P.'s conferences, called in 1978 at the Universita di Parma. The topic could not be less sweeping, "Multivariate methods in ecological work". My paper presented thoughts on the nonlinearity problem of ecological data structures in conventional multivariate analysis. I will have more to say on this later. The Parma conference proceedings were published in 1979 by International Co-operative Publishing House in Fairland, Maryland with L. Orlóci, C.R. Rao and W.M. Stiteler as editors. My paper, appreciated by many at the conference, started a second life on ResearchGate with 1083 reads since 2017. This tendency is very much the same with my very early work on ordination and classification. My 1978 book "Multivariate analysis in vegetation research", all but forgotten for three and a half decades, came to life on ResarchGate with 1256 reads in the past few years.

Sabbaticals

I wanted freedom from the beginning, to pursue my science freely in my way. The University of Western Ontario, my principle home institution in continuity since 1965, gave me the opportunity. I could spend time at universities and scientific institutions anywhere of my choosing.

After every sixth year I have taken the 7th on sabbatical leave. I chose visiting professorships, the first in 1972, and the year I was promoted to Full Professor. The Botany Department at the University of Hawaii at Manoa was my first choice. Professor Dr. Dieter Mueller-Dombois, my fellow graduate student at UBC, kindly helped with the arrangements. I taught a graduate course on quantitative ecology, continued with my research project, and in the time left, I worked on my book 'Multivariate analysis in vegetation research'. It saw 1st edition in 1975 and an enlarged 2nd edition in 1978, just in time for my 2nd sabbatical.

With family with me, Márta perfected how to get the family through a year away from home, with the least amount of hassle and on budget. She managed the household, oversaw daughter Martha's schooling, did volunteer work at the Waikiki Aquarium, audited courses

at UH, researched Hawaii's ancient culture and natural history, attended conversational classes in Spanish at the Community College on Dillingham, and enjoyed her daily swimming at Ala Moana beach. The year spent at UH on our 1st sabbatical became a watershed year for us. We developed the modus operandi that helped to plan subsequent sabbaticals.

Martha attended her first year in elementary school at Hokulani, just a short walk from the University Housing on Dole Street where we rented accommodation. She made friends with classmates, most of them from academic families at U.H. from countries of all continents. She had a similar experience during our 2nd sabbatical during the 1979/90 academic year attending 8th grade at Washington Middle School in Honolulu. The experience at Hokulani and Washington taught her how to function in a very complex multicultural environment, with elements of conflict between different cultures. She not only gained experience in managing cultural adversity but practiced it. She could pass onto her daughters the positive attitude toward others, so important part of life in Canada's rich cultural mosaic.

We enjoyed the Island life and I found at U.H. a stimulating academic environment. We returned time and again, and in that process, Hawaii became a second home for us.

Early lessons

Bob Sokal and P.G. Patil were remarkably successful in their conferences and symposia. They could draw the heavy weights into active multidisciplinary discourse and introduce into their circle young scientists, just entering the highly competitive arena on merit.

I published early after my Ph.D. and kept on using such outlets as the Journal of Ecology, Nature, and Systematic Zoology. The publications launched me on an exceptional learning experience as a regular at events and a participant in the development of conceptual tools in my field. I should put into perspective what I have learnt about statistics during that early phase of my career. I soon realised that the core of applied statistics that I found in ecology represents a specific dialect. I called it for brevity 'Fisherian statistics' (FS). I realised that the FS's modus operandi is hard to satisfy outside experiments performed in controlled environment into which the experimental material is placed in a manner dictated by well-defined, regularity conditions. These conditions demand response variables, whose probability distribution is normal, measured on units taken in random sampling. What a level-headed rigidity? – I asked myself. I started to portray FS as a project-independent data analytical machine, validated only if the regularity conditions can be validated. This appeared far too generalised, on the verge of uselessness.

With engineering background, my very first reaction was to questions FS' operational validity in a complex system's environment, such as the natural vegetation. An example:

The normal distribution of population performance (X), individually or collectively, is a necessary condition in FS. The assumption of normality early that normality may fit well plant population performance if it is linear. It can be that way in a short ecosere. In any case normality should not be routinely assumed. I develop thoughts on this in the paper I prepared for G.P. Patil's 1978 Parma conference.[17] Considerable interest in this paper is continuing to present, shown by

more than 1290 reads in last few years on ResearchGate.

I now explain why I think the Normal spectrum is not a satisfactory model for p joint species responses on a long ecosere. For such a case the Normal distribution's density function is

$$f(X_j)=\frac{1}{B}e^{-0.5(X_j-\mu)'\Sigma^{-1}(X_j-\mu)}$$

Inspect the joint response graphs for two species X_1 and X_2 drawn in Figure 1. At any point j on the horizontal axis E (a map of the ecosere), level of forcing of the natural environment is shown. The response vector corresponding to E_j is

$$X_j=(X_{1j}\ X_{2j}\ \ldots\ X_{pj})$$

This is the phytosociologist's record set, the relevé of the vegetation stand from sample plot j. μ is the centroid, the 'average' performance in the sample. $\boldsymbol{\Sigma}$ is the p x p covariance matrix, and B a scaler, chosen to make the total area unity under the normal curve or p-dimensional hypersurface. Figure 2 illustrates the joint scatter of two species under non-linear and linear responses on a long (A) and short (B) ecosere.

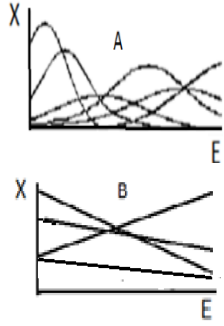


Figure 1. Two types of species response (X) to environmental forcing (E) on an ecosere. The graphs are best considered as response trends around which nature superposes performance points (not shown). Inspect Figure 2 for stylised examples. Two response types are shown. Type A is the Whittaker-Groenewoud type response, expected on long gradients, where the breadth of forcing causes species substitutions in the plant community. Examples of long ecosere include soil moisture from hygric to xeric, precipitation from arid to humid, elevation from low to high altitude across biotic zones. Response type B depicts linear species responses, expected on short ecosere that capture an ascending or descending portion of the response curve.

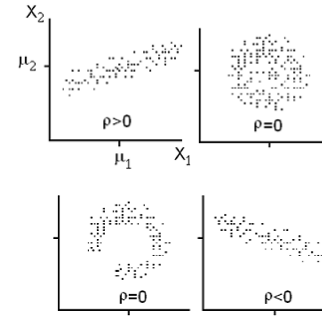


Figure 2. Two-dimensional joint scatter of responses for two randomly chosen species in four cases. The case of linear response is pictured in the first and the last graph. The joint scatter, in the second and third graph, represent cases of the Whittaker-Groenewoud type response. Symbol ρ is the product moment correlation.

The normal distribution model has two analytical traits, shape and density. Shape refers to the joint scatter (Figure 2), assumed to be elliptic. Density is measured by the density function $f(\mathbf{X}_j)$. The density is assumed to be maximal $f(X_j)=\frac{1}{B}$ at the centroid μ and declining from that point outward in all directions according to the normal law. Explicit or implicit, these are regularity conditions that come with any application of the normal model in statistical analysis. The same conditions must be true for the data set whose analysis is contemplated. If these had mismatch, science is not served.

Mathematics allows us to describe the joint scatter in reproducible terms. The description can be a centred p x p product matrix such as the covariance ($\boldsymbol{\Sigma}$) or an n x n distance matrix $\mathbf{D}$ with a characteristic element,

$$d_{jk}=\left[(X_j-X_k)'\boldsymbol{\Sigma}^{-1}(X_j-X_k)\right]^{0.5}$$

The point to be made regarding the mathematical model and its application is simplest if based on Figure 1 and 2. Two questions:

1. Does an elliptic shape appear an acceptable proxy for the shape of any of the point swarms displayed in Figure 2?

2. Does the normal density distribution (highest at the centroid, declining outward) appear a good choice in any of the cases shown?

The answer to the both questions is 'yes' for cases of linear response. The answer to both is 'no' for cases of Whittaker-Groenewoud type response. In the latter case (third graph in Figure 2), the natural shape is a hourshoue. The path between the poles are I sthe best fitting curvethrough the point swarm. As pictured, the centrois in the void and

density increases outward. Any suggestion to straighten the curve by transformation is misguided. So is to pretend that the lacuna in the center remains unoccupied by chance, knowing it represents impossible joint states in nature. If ignored and the ellipsoid model is forced, much ecological information what remains for analysis is an artifact.

To an engineer, the question of responsibility for the conclusions drawn and decisions made on that basis come naturally. I connected responsibility with the local validity of the necessary regularity conditions. Should we link validity of results and conclusions coming at large from FS to the local validity of the underlying regularity conditions? If so, the results and conclusions therefrom would be left invalid when the necessary regularity conditions were not substantiated?

Statistics is the science we use when we want to study large populations based on a limited sample of population units. The conventional solution to taking such a sample is by letting chance rule the selection. The population unit can be anything. In some cases, my units are individual plants of a species population. The units can be entire stand of the vegetation complex, whose elements are the species within areal limits. In neither of these two cases can we expect there to be a complete directory of the individual units available in natural vegetation. This implies that not all units are labelled and are at an accessible address. Can random selection be done without a complete directory? No, it cannot be done!

Therefore, the phytosociologist's preference has shifted onto another method of sample selection. That method leaves all decision in the surveyor's hands. I refer to the method supervised or preferential selection of sampling units. In such a case, the unit selected must be typical of its type. Such a plant must be a complete specimen in which case it carries all traits of its type. It may be a species, a functional type, or something else. I make a comparison in the broadest sense to biological systematics' type specimen to synsystematics' homogeneous vegetation stand, the elements of which are individual plants, usually of many types. Since the plant types assemblage into stand is environment mediated, the first rule of unit selection in vegetation sampling is environmental homogeneity within the stand's area unit in pattern theoretical terms. Both floristic and structural homogeneity are required, but it is subordinated to environmental homogeneity. Without the homogeneity concept the 'typical vegetation stand' cannot be defined and 'preferential sampling' cannot be practiced.

Preferential sampling is the only alternative under most survey conditions in vegetation ecology. But we know, not leaving selection up to chance is not new. It was not in the cards when Darwin developed his theory of species evolution, nor when Kerner von Marilaun worked on the theory of vegetation dynamics by facilitation, or when Dokuchaev discovered pedogenesis. They were keen observers able to recombine isolated fact involving typical case.

On the way well into my work with statistics I decided that FS' idea of sample efficiency has to be extended to bring it in line with what I do on the holistic level of vegetation complexes. Efficiency is a condition measurable on a continuous scale. Therefore, it is practical to phrase the question another way: when to stop sampling? This mandates an operational stopping rule. To make clear what I just said, I consider two ways of reasoning. One uses the sampling error, while the

other has in focus the sample's structural stability. The former is the key concept in statistical sampling in FS, the latter in statistical ecology in holistic studies.

An example should clarify the two ways to determine when to stop sampling. One is using an a priori decision the other is a decision made in the sampling process. Both give us n the optimal sample size, but the property optimised is totally different. How do we determine the value of n in FS? It is the sample size at which a chosen confidence interval W captures the population mean μ with 1-α probability. Assumed to be known is $\boldsymbol{\sigma}$, a p-valued vector of the n^{th} fraction of eigenvalues of the p x p population covariance matrix. Perhaps never so, in natural populations. According to statistical theory, the normally distributed X has sampling variance σ/n. Based on this, the lower limit of the 1-0.05 confidence interval is $\bar{X}$-1.96$\sigma/\sqrt{n}$ and the upper limit is $\bar{X}$+1.96$\sigma/\sqrt{n}$.

We now take a deep breath and ask the question. Where would you get $\boldsymbol{\sigma}$ for p-populations when p may run into the tens or even hundreds and population size is uncountable? I ditched the whole thing and I designed a stopping rule in sampling which uses available information and works. It measures the samples structural stability in an expanding sample.[18] My method requires process sampling and monitoring the target structures stability level. When a preselected stability threshold is reached, the sampling is stopped and at that point n is considered optimal. 8 Where to go from here?

With background in engineering, familiarity with ecological theory, and much hands-on field survey experience, I could not square FS's regularity conditions and my perception of reality in community ecology. Forcing unproven regularity conditions in practice appeared to me nothing less than a straitjacket that can lead to fake results in the wrong hands, and even worse, it can dampen the spirit which thrives on thoughtful innovations in the pursuit of originality in development of statistical ecology.

In my world the user clarifies the natural complexity's local uniqueness and did not start by statistical techniques whose valid application requires the knowledge of the attributes' probability distribution.

I had a motto and still use it: "verify the expected and reveal the unexpected". I found FS a tool kit to verifying the expected. The exploratory objective was important to me. I found no trace of that in FS. It is indeed not suitable to address the objective to reveal the unexpected.

I started to follow the exploratory line and develop a statistical dialect for myself with chief objective to probe the data for the unexpected. I did not reject FS, but used as my litmus test for the validity of any FS-based conclusion: are the assumed and necessary regularity conditions verifiable? If not, I could see no alternative to recasting the problem in a frame without FS' regularity conditions.

With the dilemma circumscribed and not willing to follow the bandwagon, I was prepared to work for paradigm change. I knew about Monte Carlo methods which use random sampling to study the distribution properties of empirical scalars. Permutation techniques do this. Probability distributions are generated based on which the mathematical result's probability of occurring by pure chance under the local conditions can be determined. David Goodall, assisted by T.E. Binet, computed his probabilistic similarity index this way in the

60s. The idea of taking the statistical techniques' mathematical barebones and coupling them with Monte Carlo techniques appealed to me. This allowed to make a mathematical outcome's significance measurable on a probability scale and generalizations with maximal local relevance.

I have set my sight on creating a study scenario in which statistical analysis treats ecological reality as it is found, and not what it should be under preconceived regularity conditions. I came to prefer process sampling in the spirit of ecological Poorean approximation [19] with stopping rules linked to degrees of the sample parameters' stability[20]. I relied on unit selection based on the traditional biological criterion of choosing units that are complete and typical, hierarchical analytical schemes which allow powerful multiscaling, [21] and Monte Carlo simulation for probability generation.[22]

Debated fundamentals

The vegetation stand

Vegetation science steps beyond the inventory stage when it asks:

1. What makes the vegetation stand, the unit plant community, a complex system that works?

2. Are vegetation stands natural units, linked through time in situ by some deterministic rule, or they are arbitrary segments ruled by randomness?

When answered, these define syndynamics and the study scenario in phytosociology. But the answers are diametrically different.

Classification

To impress on us students in Sopron that there is no science without classification, we were explained in class the relevant arguments by Aristotle. I remember the three steps to knowledge: classification, description, explanation of causality. To us students, the idea came through as being universal and timeless, not limited by the nature of objects considered.

A totally new intellectual experience waited for me in my early carrier in the New World. I found an alternative bandwagon philosophy afoot in Plant Ecology. It was pretending that vegetation classification is specious, not scientific, or outright useless. The logic behind it was offered in the early 20th Century Gleasonian dictum stating the impossibility of a logical vegetation classification in the want of exact duplicates. Therefore, it is futile to look for logical connections between plant communities. Obviously not enough attention was paid to Euclid, Kerner von Marilaun, or contemporary science where automatic classification has already taken centre stage in research as a conceptual subject and practical tool for understanding complex phenomena. Quantitative (Statistical) Ecology and Numerical Taxonomy were offering modus operandi for creating groups of vegetation stands by clustering or by assignment of individuals to parental groups.

I never felt the need to be confined by the bandwagon effect and found freedom to continue that way in Greig-Smith's lab. I spent a good amount of time studying the behaviour of automatic classification techniques. I was experimenting with decision metrics in search for well-balanced compact clusters, as opposed to those which create

chaining. I eventually settled on minimization of the sums of squared deviations within the rising groups. My algorithm was running on an ELLIOTT 808 smoothly by the end of summer 1964. I called my method optimal agglomeration and published a paper on it in 1967 in the Journal of Ecology.[23] Only later did I hear from Chris Pielou, when she was writing her "Mathematical Ecology" that J.H. Ward had used the same metric in 1963.

Reductionism

We were taught in school that the scientific method to understand a complex system is both reductionist and holist. The two are as different as day and night. They are supplementary. The question is which will return maximum information a complex system?

It is the objective of science to discover how a complex system works, in other word to reveal the rules. Reductionists dismantle the system into its component parts and hope to reveal the rules from what is learnt about individual parts. Holists look at a system intact, in its fullness. They know, only that way can they detect emergent properties owing to interactions.

We were also taught as students the classical dictum adopted to ecology: *The whole is more than the sum of its parts.* Put it another way the whole in nature has its synergy in the way of interactions that bring forth properties not discoverable in the parts. Ecologist call the result of 'interaction' an 'emergence'.

The question that intrigued me if the whole, the vegetation stand, is the top level for reduction and the lowest level is the individual plant, what is connecting the two. Clearly, there must be a design which rules the vegetation stand's assembly. When I started to read from chaos theory writers I started to see reductionism as a game with self-similarity. James Gleick[24] supplies a marvellous example in which Leibniz imagined that

"… a drop of water contained a whole teeming universe, containing, in turn, water drops and new universes within."

Self-similarity is telling me that each drop is a scaled down version of a larger drop, but it cannot be expected that analysis of a small drop could possibly reveal the contents of the larger drop. I can paraphrase the Leibniz idea in a syngenetic example. If the top level of organization is the vegetation stand and the lowest level to which it can be decomposed is the individual plant, then the apropos design that connects the two extreme levels is linked by the stand assembly process. If I follow Michael Polanyi's logic [25] I should recognise two types of realities in the vegetation complex. One is the complex level and the other is the individual plant. The lower level, the individual plants, cannot be used to specify the complex without knowing the assembly rules. This is true for whatever is the complex reduced to. In this, the controversy fanned by the band-wagon slogan 'back to the component parts' is vacuous.

Syndynamics

"… in nature there is no ending and no standing still, but only an ever coming and ever going."

Anton Kerner von Marilaun 1863

I begin with Kerner, my literary mentor in classical ecology. I consider him the father of Dynamic Ecology. Dr. Theodor Just of the Chicago Natural History Museum once wrote:

"On the strength of his beautiful yet scientifically correct description of plant communities, he [Kerner] is also justly regarded as

one of the founders of modern plant ecology."

Theodor Just's remembrance of Kerner is included in the first section in Henry S. Conard's 1951 masterful work "The Background of Plant Ecology", a translation of Kerner's "Das Pflanzenleben der Donauländer". Conard specifies the translator work this way:

"I have aimed to put the text into reasonably characteristic English while still preserving the flowery enthusiasm and florid style of the author".

As far as dynamic plant ecology is concerned, I see Kerner's work as a paradigm change in science. H.S. Conard is completely right when he writes in 1951:

"The original German of this book, published in 1863, is the immediate and direct parent of all later works on [dynamic] plant Ecology. All of the fertile ideas which have since been developed are here in embryo. Recent writers, however, are strangely silent about this basic source book".

I am convinced that H.S. Conard had in mind the direct flow of ideas from Kerner into benchmark work without much mention with little mention of Kerner by Hult (1881), Warming (1895), Cowles (1899), Clements (1916,1936), Cajander (1926), Braun-Blanquet (1927), Phillips (1935), Tansley (1946), and other 20th Century writers on plant community dynamics referred to as succession. I refer to bibliographic references in my 1978 monograph.[26]

It is true that Alexander von Humboldt (1769-1859) influenced Kerner, but Sukopp (1987) points out Humboldt's views of the vegetation are static, more aesthetic than scientific. Indeed, Kerner has the honour of being the first to put forward a comprehensive theory for plant community dynamics, i.e. Syndynamics, in his doctrine of plant community development propelled in situ through time by a mechanism for which I use the modern term "facilitation by action-reaction feedback". Kerner's doctrine gives recognition to the fact that plant communities in their site are developmentally linked in structure through time to the communities that preceded them. No inheritance implied. The linking mechanism Kerner identified from field observation of overlapping vegetation and environmental patterns in substrate age. Kerner's pioneered the method of space-for-time substitution. He discovered of the general rule that species already in the site prepare the ground for new arrivals from outside. We find the idea expounded in Conard's "The Background of Plant Ecology", Chapter 24, page 196 to 205.

What Kerner identified as community development has been described by others as "succession". Thoreau (1863, see also Anderson 1986) and Hult (1881, see Clements 1936) are usually mentioned as sources for the term. Priority on the term "succession" by itself has little relevance to me when I consider the originality of Kerner's comprehensive theory on level with the great biological theories of his contemporaries (Darwin, Mendel, and Dokuchaev) in natural science. I note only that by the time Conard's book was published in 1951 at the Iowa State College Press, the second time in 1977 by the Arno Press, out of fashion, vegetation 'succession' was no longer a bandwagon topic.

While Kerner's doctrine is the first scientific theory regarding the plant community's structural directed dynamics, the Darwin-Wallace theory of evolution, powered by random mutation and natural selection, is the first theory to unify biological thinking about the origin of species. The seminal take on the idea is Charles Darwin's 1859 book "On the Origin of Species by Means of Natural

Selection[27] After Darwin, the morphological similarities of species could no longer be viewed in the static terms of taxonomies past, but rather as branches of an evolutionary tree from which modern systematics emerged.[28]

The third of the great biological theories, Gregor Mendel's, is outstanding by having a comprehensive statistical definition. When considered in large numbers the traits inherited tend to the 3 to 1 ratio. One of the far-reaching messages in this is that there are laws that are statistical, true in large numbers or in the long run, but not true in all cases.[29]

The fourth theory regards soil development, known as pedogenesis. It was put forward by Vasily V. Dokuchaev in 1883. A keen observer of overlapping natural pattern of vegetation and soil types, Dokuchaev discovered that soils are specific to the natural vegetation under the local physical conditions of parent material, climate, and topography. He knew from different degrees of solum development that changes occur in situ in time. He concluded, when he observes a soil profile what he sees is created by the ongoing biophysicochemical process unique to the site. He called the process pedogenesis.

The forgoing invites more remarks. Darwin's theory became bible in all schools of biology, while Kerner's and Dokuchaev doctrines – the roots of the holistic dynamic view of the vegetation complex, a high-level notion very much at the core of modern ecological thinking, was and probably still is largely ignored. I have been fortunate to hear about Kerner's work as a student in Professor Ferenc Tuskó's information packed lectures and about Dokuchaev's contributions in Professor Károly Botvay's eloquent presentations in Forest Engineering in Sopron. It dawned on me, after taking a graduate reading course at the University of British Columbia with Professor Wilfred B. Schofield on North American ecological classics, that Kerner's ideas re-emerged decades later incognito in the succession theories of North American and British plant ecology. I wondered if the reasons are found not so much in the language, but in the overheated band-wagon effect and in the domineering of leaders of the invisible colleges. The syndrome is left sufficiently explained by Robert (Mac) McIntosh in his 1985 monograph "The Background of Ecology: Concept and Theory" published by the Cambridge University Press in New York.

The reader in this age of mechanistic, impersonal approaches in science should not miss observing that Kerner's doctrine of community development by facilitation, Dokuchaev's theory of environment-mediated pedogenesis, and Darwin's theory of evolution by natural selection have had nothing to do with mechanistic, impersonal sampling and at arms-length inference, but everything with able, unrestrained minds observing pattern regularities in nature and drawing logical conclusions obeying common sense.

To summarise, I see Syndynamics as a complex system's multiscale functionality, governed by natural rules diagnosable by signals from historic phylogeny, current environmental mediation, and traits emerging from interactions.

I look on the process as determinism in chaos[30] It starts from somewhere and it heads in the direction of an elusive attractor on an erratic course, reigned by phylogeny, environmental mediation and the always present chance effects.

Diversity

Diversity partitions

I already mentioned my compatriot Pali Juhász-Nagy earlier with whom I had six months overlap in 1964 in Greig-Smiths lab. When I arrived in Bangor, Pali was working diligently on his contingency tables, developing an intricate scheme for partitioning disorder-based species diversity, patterned on the information theoretical work of A.I. Khinchin[31]. Pali eventually named the partitions local distinctiveness, local valence, florula diversity, and mosaic or beta diversity. Pali's work on diversity was going one way while my work went another.

I was back in Canada, an assistant professor in Botany at the University of Western Ontario, teaching and doing research. My interest in the methodologies of information theory was rekindled when I came across the 1959 book of Solomon Kullback[32] on information theory and statistics and a 1961 paper by Alfréd Rényi[33] on the generalisation of disorder-based entropy and information. It happened on one of my browsing at the Social Sciences library. Kullback and Rényi opened new ways for me to see diversity and information theory as a tool of statistical data analysis.

Alfréd Rényi, a student of Kolmogorov, had a feel for the practical. In fact, his order-based generalization of disorder-based entropy and information, became the basis of paradigm shift in ecological diversity analysis. It allowed to see the generic relationships of ecology's diversity indices. Rényi's basic equations[34,35]:

$$H_\alpha = \frac{1}{1-\alpha} \ln \sum_{i=1}^{s} p_i^\alpha \qquad I_\alpha = \frac{1}{\alpha-1} \ln \sum_{i=1}^{s} \frac{p_i^\alpha}{q_i^{\alpha-1}}$$

Symbol p_i stands for an observed proportion, and q_i is a random expectation of p_i. Equation H_α defines disorder-based entropy of order α and Equation I_α defines disorder-based information divergence of order α. The generic forms are given above, from which Kullback's minimum discrimination information statistics 2I and several of the disorder-based H diversity indices can be derived.[36] I emphasize 'disorder-based' to distinguish H_α and I_α from Max Plank's energy-based entropy[37] and its Kaniadakis' generalised quantum entropy.[38]

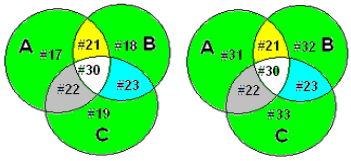
I developed thoughts on entropy and information-based data analysis early in my carrier. I presented a first overview in 1969 at G.P. Patil's interdisciplinary conference on Statistical Ecology at Yale.

I should be more detailed and specify H_α as a case such that $\alpha \geq 0$ but never exactly 1. The cases will cover several of the usual ecological diversity measures.[39]

Rényi's I_α is the tool of biodiversity partition in the canonical case. At α approaching 1, 2I is Kullback's minimum discrimination information statistic, the central scalar of his information-based statistical dialect. I charted a new direction in the analysis within an interaction-based structural frame. For the two-species case, the elementary partitions of H and I for α approaching 1 are marked on the Venn diagrams:

Complete partition sequences include #1 + #2, #2 + #3, and #1 + #2 + #3. Each elementary partition (#1, #2, #3), duplet (#1 + #2, #2 + #3) or triplet (#1 + #2 + #3) is a valid diversity measure. For synoptic reasons, I give a Venn diagram for three-

species case and point out that many more additive partition sequences can be defined based on seven elementary quantities[40] H and I:



Entropy Information

Extension of the scheme to higher number of species in the collection is possible, but increasingly cumbersome. Beyond a point only targeted sequences are considered.

Whatever is the size of the collection, the elementary partitions and sequences of these allow linkage of the biodiversity structure of the biotic community to identify factors of causality, the principal determinants of the community process as identified earlier.

As a postscript to introduction of Rényi's equations into Ecology, I should recall a manuscript on species diversity by Mark Hill submitted to the American Naturalist in the early 1970s. Sokal was the editor at that time. It found its way from Robert Sokal to my desk for review. I read the paper and in a signed review I suggested more up-to-date contents by recommending Alfréd Rényi's 1961 paper on generalised entropy and information for reference. I think I also mentioned Solomon Kullback's book as relevant source to be incorporated. I heard nothing about the manuscript until 1974 when students brought to my attention a paper in Ecology on diversity and evenness. It was by Mark. It appeared revised as my review suggested. I mentioned this to Peter Greig-Smith at a meeting in Nijmegen a few years later. He thought it is unusual not to withdraw the paper from the journal where it is under review before offering it to another journal, and to notify the reviewer who identified himself.

I would feel unaccomplished if I did not say more about my late friend, Pali Juhász-Nagy. I already mentioned the 6-month overlap I had with him in Peter Greig-Smith's lab, and I confess, not unfrequently in the remarkable rustic pubs of Anglesey. Six months are short in celestial time, yet sufficiently long for interactions to turn collegiality in friendship which lasted until his early passing in 1993. Pali, a walking encyclopaedia, assured the conversation move forward, and science made in the fertile debating ambient.

One of Pali's favourite topics was Science itself and the place of Ecology within science. He was taking measure of Ecology, and he did not like what he found. He questioned how concepts were developed. He took exception to how ecological theories were constructed. He felt past work paid insufficient attention to core concepts about which a general ecological theory could be constructed. To him, reformation of ecology's theoretical base was prerequisite to be called a 'science'. He gave a major seminar on the topic and short talks, but few in the audience new exactly what he was talking about. Pali presented what I believe is a most comprehensive account of his ideas in 1986.[41]

The appointed day of Pali's departure from Bangor has arrived. It was a rather grey, autumn day, Márta and I picked him up at his place in Bangor and drove him to the railroad station in Holyhead on the south side of Isle Anglesey. Why Holyhead, and not Bangor? He was ticketed on the Irish Mail, first class, a fast train from Dublin that comes by ferry into port at Holyhead on the Irish Channel. The Irish Mail did not have a stop in

Bangor, only grabbed and dropped off mailbags on hooks while the train traveling through the station at its normal speed.

Pali was quite a sight that morning: typewriter under one arm, some small things in a transparent plastic bag in same hand, dragging an over-size suitcase with the other. His suitcase was bursting with books, so heavy he could not lift it. He did not bring any food with him for the trip and end he had not a quid on him. We could not let him go that way. We bought him supplies and I gave him five pounds to see him through to the British Council in London. The train left, and the next thing I heard about Pali was a telephone call from British Council. The caller wanted me to verify the "five pounds" Pali had received from me. About three days later I had an envelope from Council with a crisp five-pound note inside. That was the way with us, with Pali and, of course, the always proper British Council in 1964.

Beyond diversity theory

I mentioned Pali's interest in diversity theory. To him a recasting of ecological diversity theory began with an analytical decomposition of disorder-based entropy (DBE) into ecologically meaningful components. It should be clear that DBE is not a stand-alone, isolated concept. The idea is not presentable in the fullest to ecologists without addressing the broader context not excluding holistic energetics. I presented ideas on this in several monographs, most recently in the paper "On statistical quantum ecology, a new paradigm for plant community energetics" which I mentioned earlier. [42]

My reasoning begins with the idea that ecology has no operative frame for community level energetics. What ecological energetics has is a well-practiced methodology to study calorific flow. The antithesis of this is energy-based entropy (EBE), as Max Planck used it within his theoretical frame, the resonator complex. The resonator complex level in ecology is the pant community, more appropriately, the stand level with energy-based entropy serving as proxy for potential energy.

The proposition that energy-based entropy, EBE, is proxy for potential energy is entirely linked with the proposition that energy quanta are distributed in the complex according to the normal probability law. This means to me that the energy connection of entropy cannot be limited to the nanoscale, the scale on which it was formulated by Max Planck. EBE, and its umbrella concept, the Kaniadakis generalised quantum entropy (GQE), is the basis of taking the analysis to the holistic level in plant community energetic. The basic EBE quantity is my scalar about which I developed a reasonably detailed statistical dialect.[43]

The EBE function is $E = -\ln P = \ln C$. In this,

$$P = \frac{1}{C} \qquad C = \frac{(T+n-1)!}{T!(n-1)!} \approx \frac{(T+n)^{T+n}}{T^T n^n}$$

Function E is parameterised at stand level by n, the total number of species (resonators), and by T, the total performance (frequency, mass) of the n species. Function E is an analytical tool for proxy-scaling stand-level potential energy. Based on this the EBE model of a vegetation stand of n species and T total mass is $E = E_{Phy} + E_{Env} + E_{Rnd}$. Read this "the EBE level in a vegetation stand is equal to the sum effect issuing from the amount of energy spent in three independent processes, such as historic phylogeny, recent environmental mediation, and ubiquitous chance events.

Considering that EBE is a special case of the Kaniadakis generalised quantum entropy (GQE), the utility of GQE in plant community energetic is considerable.[44] Clearly, any general theory based on EBE is holistic. Scalars under the QGE umbrella involve not the disorder-based component of diversity, but the potential energy component with links to phylogeny, environmental mediation, and random effects.[45]

Tüxen's symposia

Professor Reinhold Tüxen, the 20th Century's great facilitator in phytosociology, has been running annual symposia in Rinteln for years. Rinteln is a small provincial town on the Weser River in Lower Saxony. I become a regular attendee at the Tüxen symposia by invitation in 1970. Each year the symposium's focus changed to a different topic as decided at the end of the symposium the year before, but the venue remained in Rinteln. Tüxen's house nearby in Todenmann, doubled as symposium headquarters. The house accommodated the Tüxen 'Wohnhaus und Privatinstitut'.

Typical of Tüxen's preferences, scientists were invited from all corners of the world in all stages of their carrier, noted for contributions relevant to Vegetation Science. The presentation time was not enforced, only the presenters' sequence kept.

The old man Tüxen was a hands-on organizer. On one of my visits, I arrived in Rinteln from Hannover at the odd time, around one o'clock in the afternoon. I drove directly to the Gästhaus where I thought I had a reservation for lodging, but the place could not put me up and I had the dilemma of looking for different accommodation. I remembered that in his information letter to me Professor Tüxen foresaw this as a possibility and suggested that if I had any problems on arrival, I should go to his house directly and he would assist me. I took him up on his word but overlooked an ingrained German custom of Tüxen's generation: the after-lunch nap. Ringing the doorbell, I waited. A lady in her 50s appeared, Tüxen's daughter, and then an older lady, his wife, neither of whom could believe their eyes. They both talked the same time but saying the same thing: nobody should be so ill-mannered as to come to the house at this very private hour in the day. Hearing the commotion, Professor Tüxen came in quick steps to my rescue. Typical of him he did not let me apologize. He apologized to me. After a few minutes waiting, we were on our way to the correct destination where a reserved room waited for me.

The Tüxen symposia focused on vegetation science. I had no problem of coupling it with quantitative ecology and numerical taxonomy. I felt I could offer insight at the Symposium in the language of the phytosociologist.

There were earlier 20th Century attempts to introduce the European phytosociological methods into American ecology, most notably by H.S. Conard and G.D. Fullerton who translated J. Braun-Blanquet's classics "Pflantzensociologie" into English, by R.W. Backing who published a long account of the Braun-Blanquet technique, and by R.H. Whittaker who reviewed in 1962 classical work in "Classification of plant communities". But I could see novelty too in presenting quantitative ecology to phytosociologists.

It was at the 1971 Tüxen symposium where I met Bob Whittaker the first time. I found him the best organised and most articulate elegant speaker at Rinteln. He showed interest in my brand

of quantitative ecology, specifically ordination and classification techniques. He had in mind the development of marketable software, that was simple to use one technique at a time. I really could not care for marketability, it never crossed my mind. I preferred writing software to assist my research. I filled the code with options, calling for expert decisions in conversational mode. I involved a multiplicity of techniques. Some found my programs impossible to use - without lengthy tutorials.

My way was too complex for Bob's purpose, a fact that came to the surface very sharply when he saw my suggested revision for the Bray-Curtis ordination.[46] The paper offered an apparently minor but necessary correction to make orthogonal the ordination axes. Bob considered it a complication. Chris Pielou thought it was marvellous. I consider that paper as one that gave me the most fun to write. At long last I could make direct use of what learnt in Professor Alfréd Staszney's course on descriptive (parallel projective) geometric class in Forest Engineering. Eventually Bob had his wish fulfilled by Hugh Gough and Mark Hill.

I should recollect a conversation I had with Bob Whittaker. We were seating side by side at the Symposium reception in Rinteln, discussing personal histories. He knew I am Hungarian, and he knew about the 1956 Hungarian revolution. In response to his question regarding my family roots, I mentioned to him that one generation separates me from family farming, blacksmithing, and coalmining. He answered, "I come directly from the family farm". I think he mentioned a place in Kansas. "You are a farm boy? …" I asked in a tone of surprise and appreciation. Then I continued "… a farm boy WHO made it in big-time science". I remember him smiling and then we had more beer.

Bob Whittaker corresponded with me on technical matters that he thought I can help him with. The last time he called me was in early spring 1980. The call was, as usual about technical matter. I assume he called from his office at Cornel. I was at my desk preparing my lecture in my office on the top floor in St. John's Building at the University of Hawaii in Manoa. I knew nothing about his illness, but I could tell from his voice he was in discomfort.

My window gave me a clear view of the sea at Waikiki most days. That day there were unusually dark clouds brought in by the trade wind over the Koolau Range, drifting low over the Campus in the direction of Waikiki. On the usual day, there can be rain in Mano, but the clouds usually do not reach the shore at Waikiki. On that day when Bob Whittaker called the wind was strong and the clouds moved all the way out to sea.

In June Márta and I returned to London. It was in late October that year that the news of Bob Whittaker's passing reached me.

Italian connection

Tüxen's annual symposia were meeting place for phytosociologists where co-operations were forged, and joint researches planed. It was at the 1970 symposium where I met a group of Italian academics from the University of Trieste. They were interested in my work and I was interested in theirs. The group included Professors Duilio Lausi and Sandro Pignatti, and two fresh graduates, Enrico Feoli and Pierluigi Nimis. Duilio, an economist turned vegetation

scientist par excellence cultivated information theory on which I had several papers by that time beyond the diversity connection. Sandro, a botanical systematist was working on his opus magnum "Flora Della Italiana", a milestone in modern Italian plant systematics. Pierluigi had interest in ecological modelling, and Enrico was finding his way into quantitative ecology. Starting with joint research, our shared activities quickly broadened into complex projects extending across biotic regions in Europe, the Americas, and Africa.

I was asked in 1977 to organize, in co-operation with the British Columbia Forestry Service, the vegetation survey part of a belt transect in connection with the environmental impact study mandated in the proposed Foothills Pipe Line project. Survey sites were selected within a 3 km wide belt skirting an 800 km stretch of the Alaska Highway from Beaver Creek, on the Alaska boarder, to Watson Lake to the East on the British Columbia border.

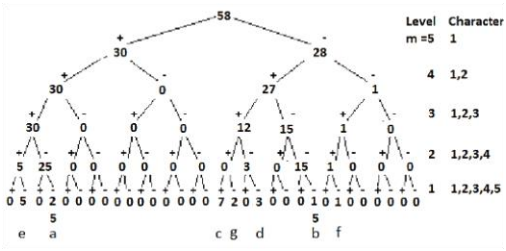
Duilio, Pierluigi, and their student (Paulo Merluzzi) joined the field work. They had keen interest in the biogeography of the region, but this was their first visit to the region they knew from E. Hultén's "Flora of Alaska and Neighbouring Territories" and other literary sources. Pierluigi arrived some weeks before the field work began. He immersed himself into and practically memorised Stanley L. Welsh's "Anderson's Flora of Alaska and Adjacent Parts of Canada". The field work was finished in 1978. Duilio and Pierluigi returned next year and completed a follow up study on ruderal plant communities and lichen synusia along the Highway. I should mention, I used information analysis the first time to probe a mega set of vegetation data for structural trends and environment connections. We published a report in 1989. [47]

Dealing with large-scale vegetation patterns matching environmental gradients in which the units are entire vegetation zones, or greatly disparate plant communities, basing the vegetation survey on plant species will not take the user much beyond conventional floristics. The analysis, targeting comparisons and comparison-based generalizations requires a plant taxonomy based on plant functional types. The methodology based on functional types started to interest me in the 1970s. In 1985 I offered an overview of my hierarchical scheme in a lecture in Rome.[48,49]

The classical functional-type based approach is exemplified by the joint paper of H.D. Knight and O.L. Louks published in Ecology 50,219-234. It is one of many which use sequential arrangement. A common feature is the use of character states, for example 'chamaephytes', to structure the plant multitudes into populations. This way the character states play the role of the taxa.
Such an arrangement did not satisfy me, for reasons I explained concisely in my 1991 paper[50], and in great length in my book **"**Statistical Ecology"[51]. I opted for different designs, appropriate for multiscale, hierarchical decomposition of the statistical scaler. My hierarchical schemes can highlight the effects that drive the assembly/disassembly process in the community. The methodology was tailor-made for the convergent community evolution project which Duilio, Enrico, and Pierluigi proposed. The new project has taken us on field work to the U.S. Southwest and California, to the Teide on Tenerife, the Monte in North West Argentina (with Valério DE Patta Pillar), the Drift Valley in Ethiopia, and Ngorongoro-Serengeti in Tanzania (with Enrico Feoli). These spurred the development of the hierarchical model

in character-based (species-free) plant community analysis.

Valério De Patta Pillar picked up the topic in the late 80s and worked up his own scheme, along the traditional lines. [52] He continued developing the theory to the present with links to signal detection in the community assembly process.[53] My models allow embedding the description of individual taxa, the character set types, called also functional types into nested hierarchies or hierarchical relevés. The levels of the hierarchy are logically linked by the natural connectedness of the characters.[54] I settled on two hierarchical scheme templates. My first scheme is balanced:



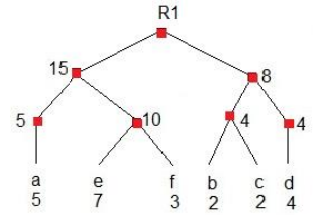
There are 5 levels in the hierarchy, the states are binary, and the number of base positions is 2^5=36. This is based on my desert hierarchical relevé from Tucson, Arizona with species re-coded as 6 binary (yes +, no -) character set types, based on 5 logically linked characters of the stem:

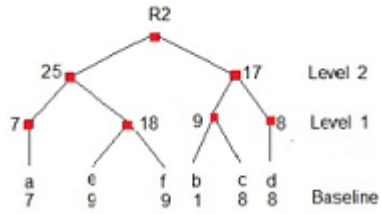
Carnegia desert relevé CSTs								
Character	a	b	c	d	e	f	g	Sum
1 T	+	-	-	-	+	-	-	
2 G	+	+	+	+	+	-	+	
3 S	+	-	+	+	+	+	+	
4 Fl	-	-	+	-	+	+	+	
5 Ft	-	+	-	-	-	-	-	
C/A	25	15	7	3	5	1	2	58

The characters: T stem tall, G stem green, S stem spiny, Fl stem fleshy, FT stem flat.

We start mapping the character set types at the top level (5) in the hierarchy. For example, character set type “a” has score vector [+ + + - -] and cover/abundance value 25. This number appears at the 4th base unit in the hierarchy. The numbers at the nodes are cumulants. The same template applies to all cases in comparison. Any stand-level statistical analysis keeps the hierarchical units or its levels as relevés.

My second scheme is unbalanced:





There are two relevés, which portray an identical evolutionary scheme for 6 distinct taxa **(**a to f). The hierarchy can be constructed for real plant populations mapped into a phylogenetic frame. The numbers at the base units are C/A estimates. Cumulants are shown at nodes on the systematic levels. In this case, the populations are plant species (baseline), genera, families, and order. I explain the detail in my “Statistical Ecology”[55] and in a separate book[56] how I used the combined schemes in statistical tests on self-organization and environmental transience in community evolutionary studies. The two schemes came too late to rescue an anaemic effort in the analysis of good evolutionary data for convergence within an IBP project.[57]

One of our major projects found initiation in 1983, under the name “The International School for Vegetation Science –

a school without walls". The occasion was the short course Sandro Pignatti organised at the Sapienza Università di Roma. The 1983 event was lavishly supported by UNIVAC. It included a lecture series, tutorials, and field trips. The school ran on an invitational basis at scientific institutions where local organisers used locally acquired funding. We brought the teaching faculty gratis. In 1985 we were guests of the Geobotanical institute at ETH in Zürich. Otto Wildi and the head of the institute, E. Landolt, did local organization with precision and rich content. In the same year, we had a short course at UNR in Rosario (Argentina). Pablo Louis honoured us with the organisation and acquired lavish support. In 1989 we were back in Italy at that the Università di Sassari on Sardinia. Ignazio Camarda was our host and organised the local activities.

The last time I was involved as director with the International School's program, we were at the Estation Agricola Experimental in León, Spain in 1989. Dr. Asuncion (Choni) Garcia-Gonzales and Dr. Florentino (Tino) Bermudez were our gracious hosts. It is with the warmest recollection that I express my thanks and compliments to Tino and the memory of Choni for contributing their talent and energy to make the Short Course in León an outstanding success.

As it happened, they invited me back to visit Choni's field research in 1989. The site in the high Sierra Cordillera Cantabrica in the Parque National de Los Picos de Europa appeared intriguing. I made a commitment for the next visit. Márta planned to go back with me to Spain and this was an ideal opportunity. Prior to arriving in Léon, Márta was visiting in Hungary and I was doing research on the highlands at the Great Rift Valley in Ethiopia, and on the Ngorongoro – Serengeti transect in Tanzania with Enrico Feoli. I arranged rendezvous with Márta in Madrid. On the appointed day I took the flight from Arusha. I was waiting for her at Barajas airport. Her flight arrived from Budapest on time. We had a week free time to enjoy Madrid and explore the region together before taking the long drive to León through the Oak Savannah of the arid plain of Spain. At one point we passed by wheat fields close to harvest time, they were sprinkled with red poppies and blue Centaureas. The sites were to Márta an example of nature's paintings in pastel colours. It reminded her of Claude Monet's famous "Poppy Field near Argenteuil" which she saw years before with me in the Musée d'Orsay on the Seine in Paris. But at the site on the Plane of Spain, the fields were not so lush as Monnet's at Argenteuil. We were in a dry, Oak Savannah environment.

We could not resist. I stopped for a photograph. Márta volunteered to be my model. She walked some steps into the wheat field, touching gently the flowers with her hands. Then turned facing me.

Márta on the Plane of Spain, 1989

Her hair, the colour of harvest wheat, her blue eyes, mirroring the colour of Centaureas, gentle facial features, shapely body, and her dress rich in hues of red, yellow and blue, blended into the natural matrix. I felt as I did many years before on our walks in the Sopron woods. I fell in love again.

We spent over a week with Choni and Tino, much of the time in the field at their permanent research site in the Parque National de Los Picos de Europa. I remember our field trip any food connoisseur would treasure as a challenging adventure. They choose Oviedo for fabada, mountain country restaurants for the meaty offerings of local gastronomy, and seafood served on the Playa San Lorenzo in Dijon.

I invited Choni and Tino to spend a month with us next year on discussions and a workbench program using Choni's data set for scrutiny. To our delight, they accepted the invitation. Their sweet little daughter Paula came too. The visit was highly successful. Choni developed further plans for joint research.

One afternoon we were seating in our living room, Choni in deep conversion with Márta. Suddenly tears started to flow from her eyes. It was not explained. I forgot about the incident. The time of their departure approached, we agreed on further co-operation, and parted as family friends. Only later did Márta tell me what made Choni cry. I realised how brave she was, not to break down completely. She discovered by touch, the return of her condition which remained in remission for an encouragingly long time. She passed away the same year.

A visit to Professor Josias Braun-Blanquet

I have many pleasing experiences from the long years in the profession, but few as treasured as the story how Márta, daughter Martha, and I met the very personification of the Zürich-Montpellier School of Phytosociology, the life time director of the SIGMA Institute[58] in Montpellier, the Professor Josias Braun-Blanquet.

We came to Montpellier in early autumn in 1975 to attend the annual Symposium and Field Excursion organised by François Roman at CNRS. The venue was near the SIGMA site, a walled off large park-like estate within Montpellier. Márta and our daughter Martha (9) with me, and my good colleague Jaroslav Moravec from the Czech Academy of Sciences in Pruhonice in company, we took the short walk to SIGMA estate. We found the gate wide open. A lady was picking figs from a solitary tree not far from us. We took few hesitant steps through the gate. As we were turning around to leave, the lady called out in a welcoming voice something like this: "Que puis-je faire pour vous"? We introduced ourselves and told her we came with the purpose to pay our respect at Professor Braun-Blanquet, if not inconvenient, and apologised for not having making arrangement in time. "Of course." She replied in English, the kind lady then offered fresh figs for Martha, which she accepted, and lead us to the larger of two building. She ushered us through the door into the study of her father. She told him whom we were. The frail, old man, looked up from behind his large desk smiling, stood up and extended his hands in a warm welcoming gesture. It was a cool September day. The fire was lit in the large fire place behind him. His large desk was cluttered with papers and books, an old cat seating near him. I thought of Professor Ferenc Tuskó's lectures he gave in class in 1952 in Sopron on the Zürich-Montpellier School of Phytosociology.

I found myself face to face with the Master himself, the iconic Professor Brawn-Blanquet. He wanted to know everything about us. He assured us how pleased he is with visitors, who in these days rarely come, and especially not an entire family. He was visibly pleased to

see young Martha. We took her out of school to come with us. We talked about the symposium where he was to appear, phytosociology, and other things as if we were his long-time acquaintances. He pulled out his visitors' book with entries and pictures of many who came to see him before us, going back decades into the colourful past. I wrote a short paragraph, expressing my esteem and thanked him for receiving us. He asked for a picture. Márta had one of me. I annotated it and inserted on the assigned page. Then Márta turned to him. "I am concerned, our visit may have tired Professor Braun-Blanquet unduly" she said. We were ready to leave. "No, no" – he replied. "On the contrary, please stay a little longer." We had tea with him, listened attentively to what he had to say. He invited us to see the laboratory in the next building where so many visiting scientists and students over many decades worked hard and studied under the master's direction. Once the gathering place of the ambitious young and the sagely old, from all corners of the world. "Now days, this place is rather quite" said Professor Braun-Blanquet, gesturing by his hand in the direction of a long wide desk which dominated the room. I recall, the large desk covered by manuals, maps, notes, some plant specimen, data sheets, and everything else that you would expect to find in a pace where phytosociology is being created. It was all there, the atmosphere, the photographs, all the tools of the trade, but no people - an eerie site as if the large room were waiting for the occupants to return from some interesting field excursion - students, professors, young and old from near and far, to continue the work they left unfinished years in the past.

I met the Great Old Master once more at the 1980 CNRS symposium. He was 96. The audience received him with standing ovation. That was the last time for him to see so many of his onetime students and us, his recent acquaintances. He passed away a few days later. Márta still has the signed reprint she received as a parting memento from him.

My take on synthesis

Phytosociological data are presented in structured tables. Classical pre-computer age synthesis had to contend with repeated rearrangement of species and relevés in the table until the desired block structure emerged. It was all done by long-hand. The advent of computers and specialised software empowered the automation practically limitless mathematical sophistication.

My take on the synthesis of structured tables is a contingency table analysis CTA. It is a highly flexible adaptation of Lancaster's method. It shows hidden structural patterns and trends invisible to the classical table rearrangement techniques. I have written an interactive code (CA) to automate CTA. Briefly described, CA produces two sets of co-ordinates (eigenvectors) on each eigen (efficient) ordination axis, all in a single analytical sweep. One set is for the row entities of the structures table and the other for the column entities. The same code has provision for computation of dispersion profiles for the row or column entities, portraying deviations from random expectation. I cite my 2014 edition of "Statistical Ecology"[59,60] for detailed description of CTA and list of relevant references.

CTA is not the same as Mark Hill's detrended correspondence analysis (DCA). Once highly fashionable among practicing ecologists, I took exception to it knowing that DCA purges the table from a compound non-linear trend by

'de-trending' the data set. It was in this connection that Enrico Feoli came to visit me at UWO in 1977. He wanted to experience my code (CA). Enrico obtained with CTA remarkably insightful results from his phytosociological data set from his Pre-Alps data, which he published in 1979.[61]

Trajectory analyses

My 1964 work on syndynamics[62] in Vladimir J. Krajina's Coastal Western Hemlock Zone followed the phytosociological tradition. Half a Century later I suggested what can be a second step in the study scenario in the manner of trajectory analysis.[63]

Early in 1987 I arrived on sabbatical leave in the Biology Department at NMSU. Márta followed me in the spring. Professor Gary Cunningham, the head of the Department and other members received us with typical southern hospitality and outmost helpfulness. Professor Walter Conley and Dr. Marsha Conley were my academic hosts. Several members of the Department were working on an IBP long-term ecological research project on a long transect from playa to rocky ridge on Jornada del Muerto in the Chihuahua desert. Márta and I re-surveyed the vegetation on the 2700 m stretch of the earlier transect by contiguous quadrats and used the data in our research concerning edge detection. [64]

Transect studies to reveal vegetation pattern have been on my research agenda from earlier days on in my career.[65] A noted contribution came in our pattern analysis program by graduate student János Podani who created and analysed a vegetation map of a microsite in the sagebrush vegetation in British Columbia's Okanagan Valley. János came to us for a second Ph.D. He is probably the sole candidate who could complete a Ph.D. with us in 16 months in the annals of U.W.O.'s Faculty of Graduate Studies. János returned to Hungary and had a distinguished academic career in numerical taxonomy and quantitative ecology at ELTE in Budapest. For his direction setting research, János was recently elected academician member of the Hungarian Academy of Sciences (MHAS).

I learnt from the Jornada del Muerto project that I need new conceptual tools that would allow me to take the next step in the analysis of long coenoseres, chronological or spatial. The following puts the chronological into focus. What was offered by conventional statistics for analysis of general data series, I did not like for reasons I outlined when I discussed the Fisherian dialect (FS) of statistical analysis of data from surveys of the vegetation complex. I approached the analysis from another direction. I called it multi-scale trajectory analysis.[66,67,68] I focus on scale-dependent parameter oscillations in the long-term vegetation process. The technique is multivariate and by scale change also hierarchical.

I started from what I knew about trajectories and projectiles from military science, a compulsory program with its own curriculum for engineering students. We were trained as prospective officers in reserve in infantry-attached artillery. The military science program was run parallel with the forest engineering program.

The bulk of the conceptual development for the trajectory analysis was done in the Botany Department of the University of Hawaii at Manoa in Honolulu, in Valério De Patta Pillars' lab at UFRGS in Porto Alegre, Brazil, and to some extent on visits to Madhur Anand's lab in Sudbury. I

realised early that I need a model for trajectory analysis in which the target is not fixed, and the objective was not to make the projectile hit the target dead on in the least number of shots. Indeed, in my model, the target had to be assumed receding into the future without an end, while undergoing chaotic oscillations under the complex effect of a continuously changing environment in situ. My first interest turned to the behaviour of the projectile in 'flight', propelled on an ever-changing trajectory. In other words, my model could not be described by a single Newtonian equation with provision made for fixed 'gravity' and some random effects of the Normal type. It had to be made a completely dialectic exercise based on holistic empiricism and chaos theoretical principles fitting the chronosere for which the trajectory is written.

The 'projectile' in the model vegetation chronosere is the actual state of the vegetation stand in situ at any point on the evolving temporal trajectory path. The target, called 'attractor', is a future state of the stand in situ.[69] My model's target is an attractor not fixed, but chaotically mobile, propelled by the vegetation stand's assembly-disassembly process. In these terms, the attractor is a set of conditions, in continual flux, specific to the local state of the biogeoclimatic complex.

I took interest in the trajectory's behaviour, manifested by regularities in the trajectory's parameter oscillation. My choice for parameters includes process velocity, acceleration, phase structure, complexity, directedness, and others, such as the stability/instability conditions in the plant community's energy-based entropy level.

Governance rules

It so happens that much of vegetation sciences effort is spent on the search for the rules that govern the vegetation stand's assembly/disassembly process. I suggest that trajectory analysis is easily adopted to perform specific tasks in the search for governance rules in vegetation chronoseres.

The idea of using a signal theoretical analogy in a phylogenetic context in the search is well-expressed by Valério De Patta Pillar in plant communities.[70] I picked up the topic in the context of how to identify and quantitatively express stand assembly rules.[71,72,73,74] I tackle the problem by way of analytical isolation of components of the total signal corresponding to phylogeny, responsible by self-organisation, current environmental mediation, responsible for transience, and signal garbling by unidentifiable (chance) effects. My phylogenetic base is the hierarchical taxonomic system into which the community taxa are mapped by modern plant manuals.

The physical function partitioned is sums of squares and products, replaceable by entropy and information of order one, or generalised quantum entropy of order one.

Prologue

I know the view I created about my life is not complete, yet I feel my account is detailed enough to reveal 'me'. If I have time I may return with more in yet another version of "Looking back…". What else should I say for last before I quit? Perhaps something very general but centrally relevant in my early life, emphasize the important of family to me in my professional life, and mention the strong ties to Sopron, the city of loyalty and freedom.

I grew up in an age of momentous change, instigated by geopolitical ambitions of empire builders. I was not an outside observer; the unrestrained violence over much of my early years directly affected my family and me. By the age of 10 I saw my family's world of civility and middleclass-values disappearing. By the time I reached 14 the old order was shuttered, total terror descended on the people in the country on the heels of the plundering hoard who called himself our "liberator". Freedom was lost, proconsuls ruled, and the occupation army stayed on at the cost of the occupied population for four and a half decades. The nightmare for Hungarians who stayed when the perpetrators' system imploded in Moscow.

I realised early in my youth that nothing is permanent around me, peace and freedom can be no more than fleeting moments, and life or death a matter of command. I had to learn to be street-smart, daring but not mindless, keep my thoughts and think before speaking, and have trust in few and never in the system apparatus. I learnt to observe what is happening around me and think of what I see.

How much worse could it become became my motto "Have I taken a wrong step?" became a central question. Márta and I failed in exactly that. We sided with the revolution. We wanted to be free, live a normal life at home in peace among our people, in our culture. We lost the fight and still paying the price: a life in exile.

It was Canada who took as in. We reciprocated with our best effort to lead a life of exemplary citizenship. Measuring our life's every-day difficulties on the scale of how worse could be, convinced me I had clear sailing in the free world. I never felt the need to ask for 'more'. Recognition came my way in all forms. I was promoted through the ranks to full professor in seven years and received many coveted awards in the scientific world. But most importantly, Márta and I we were free to make our choices. I was allowed throughout my career to accept visiting professorships and visiting research scientist positions in academe and other scientific institutions worldwide. It is with a feeling of indebtedness hat I express my sincerest thanks for those who invested in me their trust and confidence, to far too many only in memoriam.

The separation of family life and profession my experience has no sharp separation. Scientific meetings become family events, and so do my short courses, workshops, field research, and attendance of events of recognition. I should recall my induction to the Hungarian Academy of Science in Budapest. I completed my talk, remembering that Márta too planned to say a few personal words. There were many friends and family members in the audience from Hungary and overseas. They expected Márta to speak from earlier experience. I turned to the presiding academician and asked him if he would consider permitting Márta to say a few personal words compliment my presentation. He smiled, then turned toward Marta and with a wide gesture offered the podium to her. Márta was well prepared, brief, and well-received. Her daring act became the topic of conversation at the Academy. We were told she is the first academic wife ever to speak at an induction ceremony since the Academy's establishment in 1825. She may have pioneered a new tradition. Why not?

I should not leave unmentioned Márta's favourite story from 1997 at my induction to the Canadian Academy of Science

of the Royal Society in Ottawa. New Fellows are inducted at the annual general meeting in November. The Society chose the Château Laurier for venue in 1997 and the Confederation Room in the West Block on Parliament Hill for the induction ceremony. I had the larger family in attendance including granddaughters Kathryn (three) and Ruth (three months old). Kathryn walking with Márta in the Château and calling Márta Nagymama as they converse, attracted the attention of the others among the passers-by in crowd who caught the phrase "Nagymama". To Márta's delight, much conversation pursued, and acquaintances forged. The General Meeting's banquet was attended by the granddaughters. Ruth's broad smile in a pram carried by her mother Martha received much attention. People walking by stopped and inquired about her. One of them, Robert Haynes, the president of the Royal Society at that time, observed Ruth in the pram, stopped at the group. Introductions done, Márta turned to him. "Dr. Haynes, this is Ruth, my granddaughter, a future prime minister of Canada." Dr. Haynes agreed saying "She certainly got an early start among the Fellows and has a parliamentarian experience too". All laughed, and the banquet rolled on.

The last topic I plan to present is of great importance to me. Many memories tie to Sopron, that wonderful City in the setting where the piedmont of the Alps meets the Lesser Hungarian Plane. The latest is the City Government's memorial medal "For Sopron 1956". It was awarded to me in 2016 for demonstrated exemplary valour during the 1956 Hungarian Revolution and War of Independence. I include in the Appendix photocopies of the medal, certificate, and letter informing of the award by the Right Honourable Dr. Tamás Fodor, Mayer of the City and County of Sopron.

Appendix

1. Letter of notification of award

A hűség és a szabadság városa

Sopron Megyei Jogú Város
Dr. Fodor Tamás polgármester
Polgármesteri Hivatal - Sopron, Fő tér 1.

Ügyiratszám: 40806-11/2016
Ügyintéző: Dr. Brummer Krisztián
Tel.szám: 99/515-109

Tárgy: október 23-i ünnepség

Orlóci László
akadémikus részére
3-575 McGarrell Place
London, Ontario, Canada N6G 5L3

Tisztelt Orlóci László Úr!

Sopron Megyei Jogú Város Önkormányzata nevében sajnálattal vettem tudomásul, hogy nem tudott részt venni az 1956-os forradalom és szabadságharc 60. évfordulója alkalmából rendezett ünnepségünkön. Természetesen megértéssel fogadtam ezt, jelezte, hogy egészségi állapota nem teszi lehetővé az ide utazását.

Kérem engedje meg, hogy ezért mellékelten juttassam el az Ön részére az 1956-os forradalom és szabadságharc évfordulójának alkalmából készített és átadott 56-os emlékérmet, valamint az azzal járó elismerő oklevelet!

Ezzel is szeretném köszönetem és hálámat kifejezni a forradalom időszakában kifejtett bátor magatartása előtt, amely követendő az utókor számára. Példaértékű tevékenységével mindent megtett azért, hogy Sopronban vérontás nélkül érjen véget a forradalom és szabadságharc. Múlhatatlan érdeme, hogy városunkban és környékén vér nélkül zajlottak a forradalmi események.

Sajnálom, hogy személyesen nem vehetett részt jubileumi ünnepségünkön. Felemelő és emlékezetes volt az egykori 1956-os hősök és hozzátartozóik, egyetemisták találkozása, visszaemlékezései.

Kívánok Önnek jó erőt és bízom benne, hogy egészségi állapotában mihamarabb javulás következik be!

Sopron, 2016. október 24.

Tisztelettel:

Dr. Fodor Tamás
polgármester

Sopron Megyei Jogú Város Önkormányzata

postacím: 9401 Sopron, Pf. 127 • telefon: +36 99/515-101 • telefax: +36 99/330-452
internet: www.sopron.hu • e-mail: polgarmester@sopron-ph.hu

Literal translation:
The city of loyalty and freedom

The City and County of Sopron
Dr. Tamás Fodor, Mayer
Mayer's Office:
File number: Subject: October 23 Celebration
Manager:
Telephone number:

László Orlóci
Academician
3-575 McGarrell Place
London, Ontario, Canada N6G 5L3

Esteemed Mr. László Orlóci

In the name of the Government of the City and County of Sopron I acknowledge with regret that you could not be present at the Jubilee organised for the 60th anniversary of the 1956 revolution and war of freedom. Naturally, I understand that your health do not make it possible for you to travel this far.

Please do allow me to forward to you as an attachment the Commemorative Medal and associated Certification Document prepared for you on the anniversary of the revolution and war of independence.

With the Commemorative Medal I would like to express my gratitude for demonstrated valour, which serves as an example for posterity. With your exemplary activities you have done everything that in Sopron the revolution and war of independence reach conclusion without bloodshed. It is your eternal merit that in our city and vicinity the revolution remained bloodless.

I am sorry that you could not receive this medal in person at the jubilee celebrations. It was memorable and uplifting to experience the meeting and reminiscences of the heroes of 1956, families and university students.

I wish you Sir good strength and I hope for improvement in your condition soon.

Sopron, 2016 October 27

Respectfully,

Dr. Tamás Fodor

Mayor

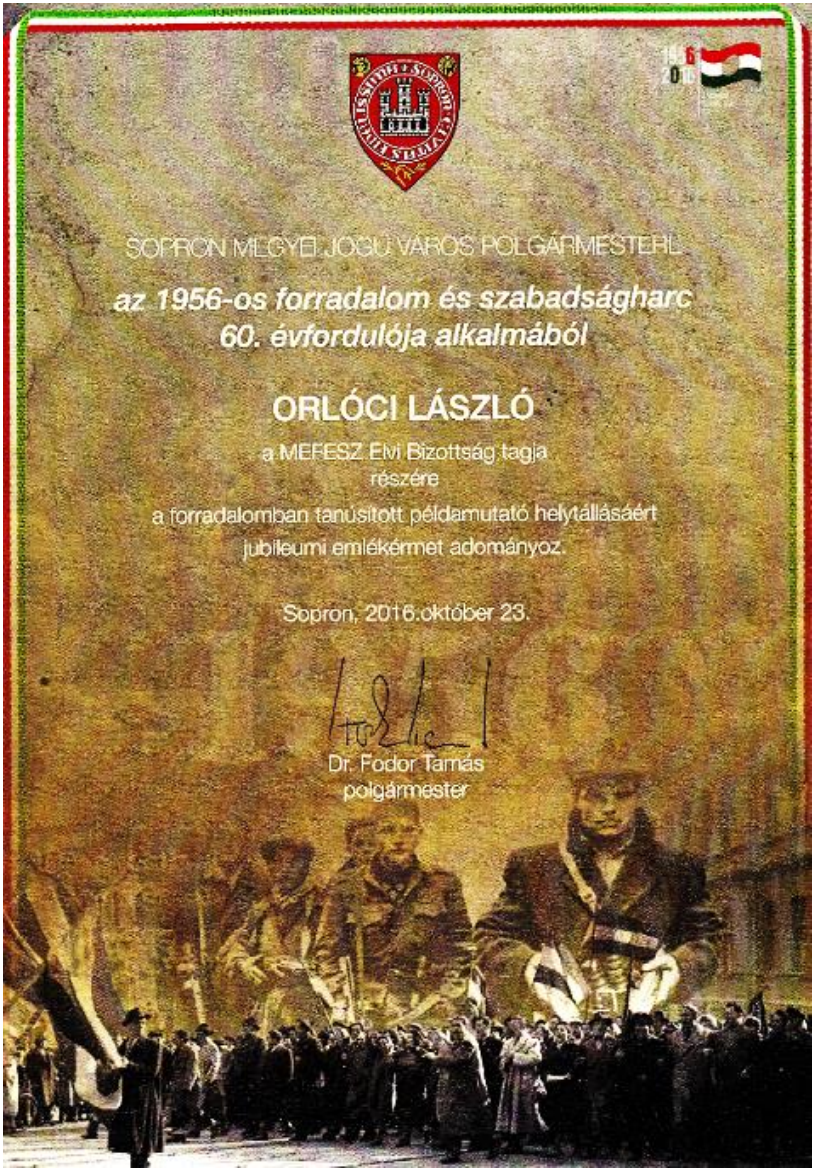
2. "Sopronért 1956" memorial medal for valour

Left: Within shield, "Sopron Civitas Fidellissima" (Sopron the Most Loyal Town). Lower encryption: "For Sopron". Right: encryption: "Pro Patria 1956".

Citation at presentation from Mayer Fodor's letter (see item 1 below):

"With the 1956 Commemorative Medal I would like to express my gratitude to László Orlóci for demonstrated valour during the revolution which serves as example for posterity. It is his eternal merit that in our city and vicinity the revolution remained bloodless."

3. Certificate of award



Literal translation: The Mayor of the City and County of Sopron awards on the 60th anniversary of the 1956 revolution the jubilee memorial medal to László Orlóci, member of the MEFESZ Revolutionary Committee, for exemplary valour demonstrated during the revolution.
Sopron, 2016 October 23.

Endnotes

[2] View live the extemporaneous presentation on YouTube (LASZLO ORLOCI) in five short segments under the title "How I become a statistical ecologist".

[2] Orlóci, L. 2018. On statistical quantum ecology. A new paradigm for plant community energetics.

https://www.researchgate.net/publication/327781028

[3] Orlóci, L. 2014. Quantum ecology. Energy structure and its analysis. SCADA Publishing, Canada. Online edition: https://www.amazon.com/dp/153716788x

[4] Orlóci, L. 2017. The energy-based entropy structure of natural plant communities. Causes, measurement, statistics. SCADA Publishing, Canada. Online edition:
https://www.researchgate.net/publication/321495308

[5] Mihály, M. 2016. "Reflections". SCADA publishing, Canada. Online Edition:
https://www.amazon.com/dp/1534968172

[6] I should mention, Kathryn has graduated from the Natural Resources Management (Forestry) program at Lakehead and Ruth entered fourth year Geology on a Presidential scholarship at the same university in 2018.

[7] "Slave of the Huns", https://www.amazon.ca/láthatatlan-ember-Géza-Gárdonyi/dp/1784352292

[8] Attention! Attention! Air raid warning.
[9] Railway station.
[10] Elementary school.
[11] Don Bocce Church
[12] To down load, go to the web address below, click Alternative Links for Downloads on Márta's side on my webpage, then click title #4:
https://sites.google.com/site/statisticalecology/

[13] To down Ziva interview, go to
https://sites.google.com/site/statisticalecology/ click "Alternative Link for Download" on László's side then click # 1001 in the list.

[14] Orlóci, L. 1966. Geometric models in ecology. I. The theory and application of some ordination methods. J. Ecol. 54:193-215.
https://www.researchgate.net/publication/267096435

[15] Orlóci, L. 1967. Data centring: a review and evaluation with reference to component analysis. Syst. Zool. 16:208-212.
https://www.researchgate.net/publication/263697380

[16] Rényi, A. 1961. On measures of entropy and information. In: J. Neyman (ed.), Proceedings of the 4th Berkeley Symposium on Mathematical Statistics and Probability, pp. 547-562. University of California Press, Berkeley.

[17] Orlóci, L. 1980. Non-linear data structures and their description. In: L. Orlóci, C. R. Rao and W. M. Stiteler (eds.), Multivariate methods in Ecological Work, pp. 239-275. ICP, Fairland, Maryland.

[18] Development of the sample stability sampling rule benefited greatly from joint research with Otto Wildi and Valério De Patta Pillar. The reader will find the detailed exposition of technique and original references in my Statistical Ecology (2014) already referenced in the text.

Orlóci, L. 2014. Statistical ecology. Quantitative exploration of Nature to reveal the unexpected. SCADA publishing. Online Edition:
https://www.amazon.com/dp/1453760520

[19] Orlóci, L. 1993. Complexities and scenarios on in bioenvironmental analysis. In: Costa, G. et al., Conceptual tools for understanding nature. World Science, London. https://www.researchgate.net/publication/263766025

Orlóci, L. 2001. Prospects and expectations: reflections on a science in change. Community Ecology 2: 187-196.
https://www.researchgate.net/publication/240763243

[20] Orlóci, L. and V. De Patta Pillar. 1989. On sample size optimality in ecosystem survey. Biométrie-Praximétrie 29:173-184.
https://www.researchgate.net/publication/233835712

[21] Orlóci, L. 2009. Multi-scale trajectory analysis: powerful conceptual tool for understanding ecological change. Frontiers of Biology in China 4: 158-179.
https://www.researchgate.net/publication/226491574

[22] https://www.amazon.com/dp/1453760520

[23] Orlóci, L. 1967. An Agglomerative Method for Classification of Plant Communities. Journal of Ecology 55:193-205. Download from:
https://www.researchgate.net/publication/264898705

[24] Gleick, J. 1988. Chaos. Making a new science. Penguin Books, U.S.A.

[25] Polányi, M. 1968. Life's Irreducible Structure. Science. 160 (3834): 1308–12.

[26] Orlóci, L. 1978. Multivariate analysis in vegetation research. Online edition:

https://www.researchgate.net/publication/263766205

[21] The idea of evolution was not new to the times of Wallace or Darwin. Other scientific theories already existed before them. Buffon (1707-1788) and Lamarck (744-1829) should be mentioned. Substantial critique has been voiced against the Darwin-Wallace theory since it formulation, particularly against the idea of slow, gradual evolution. Kozo-Polyanskiy (1937), the proponent of symbiogenesis, argues that without moments of explosive evolution the increase in the number of species at the rate as it happened in the angiosperms during the Cretaceous would not have been possible. Hitching (1982) makes the points that Darwinism has been tested and failed on several counts: (i) gaps in the fossil record suggest evolutionary leaps; (ii) genes are stabilizing mechanisms and tend to minimise the chances of new forms; (iii) random mutation, occurring at the molecular level, cannot account for the explosive increases in the diversity and organisation of life.

[28] I refer to J. Podani's (2009) essay on the unresolved problems of systematics as road map to appreciation, interpretation and comparison of different taxonomic systems using the modern phylogenetic approach as reference.

[29] It should be mentioned that about the same time as Mendel was doing his experiments with pea plants, Galton (1822-1911) and Pearson (1857-1936) were developing new ways to manage chance-related errors in the analysis of sampled biological data. But beyond the statistical connection, Mendel's main significance is in the seminal nature of his work. A relatively short, one and a quarter century later – short when compared to the time elapsed in astronomy before an explanation of planetary motion was found – genetics managed to assemble sufficient knowledge to map the human DNA. The regularity revealed in the structure of the DNA has, in all consequences in cracking the human genetic code, a level of significance comparable to Kepler's mathematical map of planetary motion that lead to Newton's explanation of why do the planets do not fly off their orbits, or very relevant to ecology, Planck's idea that energy is transported in discrete units and the potential energy level of complexes are measurable in entropy terms.

[30] https://www.amazon.com/dp/1453760520

[31] Khinchin, A.I. 1957. Mathematical foundations of information theory. Dover Publications, New York.

[32] Kullback, S. 1959. Information theory and statistics. Wiley, New York. --- Kullback, S., Kupperman, M. and H.H. Ku. 1962. Tests for contingency tables and Markov chains. Technometrics 4: 573-608.

[33] Rényi, A. 1961. On measures of entropy and information. In: J. Neyman (ed.), Proceedings of the 4th Berkeley Symposium on Mathematical Statistics and Probability, pp. 547-561. Berkeley University Press, California.

[34] Rényi's equation got me into a highly embarrassing situation when it appeared in a highly corrupted form in my 1978 book "Multivariate analysis in vegetation research". The mistake is corrected in the internet edition. See my ResearchGate page.

[35] Definitions and relevant remarks are collected from earlier papers in my 2006 article on diversity partitions in Community Ecology (see below) and in my 2014 book on "Statistical Ecology" already referenced in the text. See also:

Orlóci, L. 2006. Diversity partitions in 3-way sorting: functions, Venn diagram mappings, typical additive series, and examples. Community Ecology 7:253-259. https://www.amazon.com/dp/1453760520

[36] https://www.amazon.com/dp/1453760520

[37] https://www.researchgate.net/publication/327781028

[38] Orlóci, L. and K. Özkan. 2018. Energy-based entropy and its generalisation. Part 2. Extensions of concept and application in regional vegetation survey. Downloadable from https://www.researchgate.net/publication/325269808

[39] Hill, M.O. 1973. Diversity and evenness: a unifying notion and its consequences. Ecology 54: 427-432.

[40] https://www.amazon.com/dp/1453760520

[41] See Juhász-Nagy Pál in catalogue of the Országos Széchényi Könyvtár (National Szécsényi Library), Budapest:

[42] https://www.researchgate.net/publication/327781028

[43] https://www.researchgate.net/publication/327781028

[44] https://www.researchgate.net/publication/325269808

[45] https://www.researchgate.net/publication/327781028

[46] Orlóci, L. 1974. Revision for the Bray & Curtis ordination. Can J. Botany 52:1773-1776. https://www.researchgate.net/publication/237159097

[47] Orlóci, L. and W. Stanek. 1989. Vegetation survey of the Alaska Highway, Yukon Territory: Types and Gradients. Vegetatio 41: 1-56.
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[69] I am presenting thoughts on a chronosere. The model can be formulated for toposeres or ecoseres with or without space for time substitution.

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https://www.researchgate.net/publication/44528408