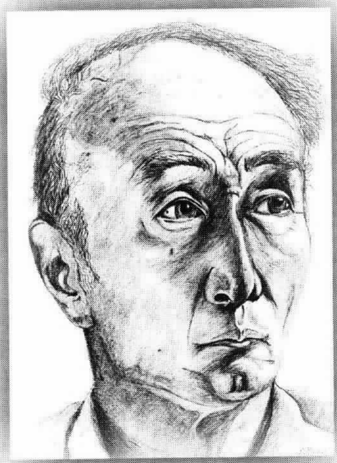
Tribute to Akira Okubo



Portrait of Akira Okubo by Deborah Bray

Preface

Malcolm J. Bowman
School of Environmental and Marine Sciences
The University of Auckland, Tamaki Campus
Auckland, New Zealand

Robert E. Wilson
Jeannette Yen
Marine Sciences Research Center
State University of New York, Stony Brook, New York, USA

Akira Okubo, Professor of Mathematical Ecology at the Marine Sciences Research Center, State University of New York at Stony Brook, died on 1 February 1996, a few days before his 71st birthday after a long, courageous battle with cancer.

Akira Okubo came to the United States as a young man in 1959, following a career as Chief of the Chemical Oceanography section of the Japanese Meteorological Agency in Tokyo. He completed his Ph.D. in 1963 under the tutelage of Donald W. Pritchard at the Chesapeake Bay Institute of Johns Hopkins University in Baltimore. He continued working at CBI until 1975, when he was appointed Professor of Mathematical Ecology at the Marine Sciences Research Center, State University of New York, Stony Brook.

Trained as a chemist and chemical oceanographer, Akira became the compleat theoretician . . .

He quickly established himself as a distinguished faculty member, not only in physical oceanography, but also in many other disciplines that incorporated his wide interests in oceanic diffusion, animal and insect swarming, and studies at the physical-biological interface.

Trained as a chemist and chemical oceanographer, Akira became the compleat theoretician – an applied mathematician with a keen sense of physics, biology, and a rare insight into where the significant problems lay.

Actively sought out by leading marine scientists worldwide over the years, Akira could identify almost 100 collaborators. As sole author and with his colleagues, he published well over 150 papers. His own remarkable contributions earned him numerous honors, including the prestigious Medal of the Oceanographical Society of Japan, and a Senior Visiting Scholarship at the University of Oxford. His studies ranged from dye diffusion in the ocean, circulation in oceanic fronts, Lagrangian dispersion, and midge swarming behavior from its chemistry to ethology. He applied his insights into turbulent mixing to subjects as disparate as seed dispersion, animal grouping behavior, and spider webs.

His text *Diffusion and Ecological Problems: Mathematical Models* (Springer-Verlag, 1980) remains a modern classic. In his preface, Akira talked about seven influential colleagues, whom he called his seven "lucks":

J.G. Skellam (for encouragement to write the book); H.C. Chiang (for midge swarming collaboration); J.R. Schubel (for broad minded leadership); K. Yano and S.A. Levin (for their Japanese and English publications of a book on ecological modeling); G. N. Parker (for translating the original Japanese text and later improving the English text of Akira's book); the Hashimotos of the Hana restaurant of Port Jefferson, New York (for keeping Akira from starving); and to all his friends for their help and encouragement. Finally Akira thanks Chiyo *"for her companionship in the long, long agonizing period of writing."* Chiyo was Akira's pet hamster.

Akira's greatest attributes were his warm humanity, generosity and love for science. Few scientists have touched as many other's lives and hearts. Through his own enthusiasm, his generosity with his time and ideas, and his unselfish way of allowing others to take credit for ideas that were originally his, he multiplied his influence amongst his colleagues and students many times over.

On 21-22 July 1995, the Okubo Symposium was held in his honor at the Marine Sciences Research Center of the State University of New York, Stony Brook. The invited speakers included Simon Levin (Princeton University), Thomas Powell (University of California at Berkeley), John Steele (Woods Hole Oceanographic Institution), Mimi Koehl (University of California at Berkeley), Trevor Platt (Bedford Institute of Oceanography), and Curtis Ebbesmeyer (Evans-Hamilton, Inc.). Each speaker presented seminars on topics where Akira had made substantial contributions, as well as paying him a personal tribute. Some of these participants have contributed to this volume. Akira was profoundly grateful for the symposium and actively participated in it. By all accounts, the symposium was a great success.

Everyone who came into contact with Akira found their life immeasurably enriched; he became their *sensei*. We present this compilation of articles and stories as a lasting tribute to a humble, gentle man called Akira Okubo who touched our lives in profound ways. We have tried to capture the essence of Akira's personality and his approach to science, his love of the inner structure and innate beauty of the physical, biological, and ecological processes which define the natural world, and his keen eye for the exquisite. We hope that this volume is a fitting tribute to this great scientist, humanist, and lover of nature.

Figure 1. Malcolm Bowman paying tribute to Akira Okubo in May 1995 for his participation in the MSRC graduate seminars over the previous 20 years.

We wish to thank all who have contributed to this volume in some way. And, thank you Akira for being a treasured colleague and special friend to each of us.

Akira Okubo: A Man for All Seasons and Many Disciplines

Donald W. Pritchard
Marine Sciences Research Center
State University of New York, Stony Brook, New York, USA

Jerry R. Schubel
New England Aquarium, Boston, Massachusetts, USA
and
Former Dean and Director
Marine Sciences Research Center
State University of New York, Stony Brook, New York, USA

Several other papers appearing in this volume are tributes or describe personal experiences with this exceptional man, Akira Okubo. Our contribution is distinct in that between the two of us, we were close to Akira over his full career in the United States. This started with his arrival at the Johns Hopkins University (JHU) in 1959 and continued with his appointment at the Marine Sciences Research Center of the State University of New York at Stony Brook in 1975. We had the opportunity to watch Akira mature and grow as a research scholar, teacher, mentor, and colleague.

First, a little history. In the late fall of 1958 one of us (DWP, then Director of the Chesapeake Bay Institute and Chairman of the Department of Oceanography at JHU), received letters from the Japan Meteorological Agency (JMA) and the Japan Atomic Energy Agency (JAEA), asking that the Department of Oceanography accept Akira Okubo, a chemical oceanographer and an employee of the JMA, as a graduate student. Included with these letters were copies of publications authored by Akira.

Among Akira's publications were two papers describing the analysis of data collected in a field study concerning the movement and diffusion of dye patches released into the surface layers of the ocean, using rhodamine-B dye as the tracer. These papers led to a special interest in Akira's research. Akira and another former student, Jim Carpenter, were both just completing their development of improved procedures and instrumentation for measurement of fluorescence at very low concentrations of rhodamine dyes.

Following an exchange of correspondence and receipt of Akira's formal application, a problem developed in that the US Embassy in Tokyo refused to issue him a student visa because he had failed the required oral English examination. Based on the earlier exchange of correspondence, including letters written in Akira's characteristic handwriting, it was clear that Akira could write and read English adequately. Akira was at the time a relatively mature individual to be starting a graduate student program, being 34 years old and only 3 years younger than his advisor Donald Pritchard. Akira was subsequently advised to have his agency obtain a visa for him as a scientific visitor using any available procedures, and the visa problem would be sorted out once he arrived at JHU. Somehow, this somewhat illegal procedure was pushed through the bureaucracy, and Akira arrived in Baltimore during the summer of 1959.

It is not easy to describe Akira's scientific career in terms of a limited number of clear-cut categories. We might consider that Akira had three distinct careers, with a few short-lived side trips in between. Akira's first scientific contribution, a paper "On the heat of mixing of seawater," published in 1951, was characteristic of eleven of the thirteen papers he wrote prior to coming to Baltimore. These eleven papers, extending over a nine year period, included descriptions of the chemical, physical and biochemical properties of certain constituents of nearby ocean waters and their related biochemical processes. These publications represented Akira's output during his first career as a chemical oceanographer.

Akira began paging through a small pocket dictionary, pretending not to understand. It suddenly became clear at that point that Akira really intended to change his career orientation.

As already mentioned, Akira had published two papers during the last few years of his employment by the JMA in which an analysis of the diffusion of dye patches introduced into the surface layers of the ocean was presented. These two papers represented the first tentative entry into his second career as a physical oceanographer. Here Akira specialized in understanding the physics of oceanic diffusion and of the development of clear, elegant theories.

When Akira first arrived at the JHU, he met with Pritchard to discuss his graduate program of course work and thesis research. Despite the fact that, in letters accompanying his application to JHU, Akira had described his desire to concentrate his graduate study on oceanic diffusion, he was still considered to be a chemical oceanographer who had used his knowledge of chemistry to select a dye well suited as a tracer for oceanic diffusion. Akira was therefore asked if he had met with Dave Carritt, the Department's senior chemical oceanographer. Akira began paging through a small pocket dictionary, pretending not to understand. It suddenly became clear at that point that Akira really intended to change his career orientation.

During the two year period 1960-1961, Akira worked diligently and quietly as a student. He excelled in all of his course work, but he spent most of his time analyzing data, collected for the most part by others, on the temporal and spatial distribution of both natural and artificially introduced tracers in the surface layers of natural water bodies. With the exception of 1952 and 1955, during his first career as a chemical oceanographer, 1960 and 1961 were the only years in which Akira did not publish. In 1962 he showed that he had in fact started his second career as a physical oceanographer specializing in oceanic diffusion, with the publication of three papers in this new field.

The first of these papers can be considered as a seminal contribution. Pritchard has often remarked that he felt both awed and proud that Akira had come to JHU to study turbulent diffusion in water and in just over two years had become the world's expert in this field.

The year 1963 marked two important events. First, in January, Akira received his Ph.D. The second was his publication of the first of a series of papers entitled "Gleaning from a field of dye release experiments." This first of the series had the subtitle: "1. Chaos from the beginning." This marked the first indication that Akira realized the importance of chaos in oceanic diffusion, although it would not be until the mid-1980's, during his third career, that Akira actually applied the emerging chaos theory to oceanic and atmospheric turbulent diffusion.

During the 12 year period 1960 through 1972, Akira produced 37 papers, all on the subject of the physics of diffusion in the ocean. This 12 year interval constitutes the period when Akira was active solely in his second career. He continued to contribute significant new knowledge on the physical properties and processes of turbulent diffusion in the ocean. This included theoretical explanations for observations of the variations in the concentration distributions of natural and artificially introduced tracer materials under various geometric constraints and/or various external driving forces.

In 1965 Akira published his first joint paper with Harry Carter, starting a close association which lasted for nearly 20 years (Figure 2).

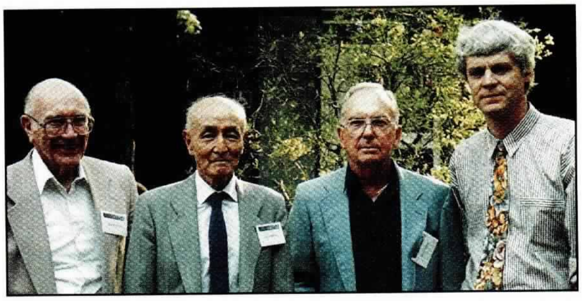


Figure 2. Professors Emeriti Donald Pritchard (left) and Harry Carter (2nd from right) share a joyous reunion with Akira Okubo and Robert Wilson (right) on the occasion of the Okubo Symposium, July 1995, held at MSRC.

Sometime in 1971 Akira came into Pritchard's office and announced that he wanted to become a biologist. Receiving an expression of surprise, Akira explained that he wanted to study the counteracting processes of biological attraction and turbulent diffusion in insect swarms. It was explained to Akira that his research support came from the Office of Naval Research (ONR) and the Atomic Energy Commission (AEC) and that it would be difficult to justify support from these sources for a shift to the study of terrestrial insects. It was suggested that perhaps if Akira instead studied fish schooling or zooplankton distributions, then financial support might be justified.

Akira rejected this suggestion, saying that useful data on these coupled biological - physical phenomena were only available from studies of insect swarming. Pritchard's concern was not assuaged by being informed that the midge species to which Akira would initially be applying his theories was named *Anarete pritchardi* Kim.

In spite of warnings of loss of support, Akira was not to be dissuaded. The first three papers he published in 1972 dealt with mathematical modeling of the swarming of small animals in motion. Thus began Akira's third career. For our purpose here the short description "Mathematical Biology" will serve to identify the subjects encompassed by Akira's third career.

It is of interest to note that Akira did ultimately find sources of data from the marine environment which could be used in the development of models describing the counteraction of biological attraction and turbulent diffusion. Starting in 1977, he published some 32 papers dealing with the interactions of physics and biology in the marine environment.

Akira led because of the power of his ideas and his ability to quietly get people to work together to find solutions.

Akira's first shift in careers, that of leaving behind his research as a chemical oceanographer to initiate research as a physical oceanographer specializing in the physics of turbulent diffusion, was marked by a sharp break. After his arrival at JHU in late 1959, he never again wrote a paper that would be identified as chemical oceanography. He appeared to simply want to forget this phase of his life.

The period, beginning in 1992, which marked the beginning of his third career was quite different. During this time he never abandoned his interests in the physics of turbulent diffusion as he was simultaneously increasing his time and effort spent on studies of mathematical biology.

Akira did not immediately develop his bent toward cooperative studies and his talents for helping others. During his early years as a student and then as a staff member at the Chesapeake Bay Institute, Akira was a friendly but quiet individual whose interactions were not extensive. He gradually changed. At first his interactions were limited to just a few individuals, notably Harry Carter and Don Pritchard, but they continued to develop as he worked with and helped a growing number of individuals, including Jerry Schubel. These interactions can be traced by noting Akira's co-authors on his various publications during this period.

Spanning three separate careers, Akira spent nearly all of his professional life in the United States at two institutions: the Johns Hopkins University's Chesapeake Bay Institute and the State University of New York at Stony Brook's Marine Sciences Research Center. During these years he benefited from a rich and diverse network of individuals and institutions with whom he collaborated and at which he spent extended periods of time.

Jerry Schubel moved to Stony Brook in 1974 as Director of the Marine Sciences Research Center (MSRC). During those early years MSRC was quite small but there was a commitment from the University's administration to develop a center of excellence. The focus was placed on the coastal ocean and on developing scientific solutions to problems resulting from society's multiple and conflicting uses of the coastal zone. Akira was Schubel's first appointment to the faculty of MSRC in 1975.

For the first time in his career in the United States, Akira enjoyed the security of a "hard money" position. With total freedom to pursue any interests he chose, he thrived in his third career and became a role model of a good professor for all his younger colleagues. Several of the administrators at the highest level of the University wondered how Akira with his new interests fitted into the model that had been proposed, but it soon became clear to everyone what a wonderful resource he was!

From the outset, Akira was a key element in the strategy to build MSRC as a multi-disciplinary center. His collegial and nurturing nature, his capacity to formulate complex problems of all kinds into tractable mathematical forms, and his ability to find order in chaos, simplicity in complexity and good in everyone, ensured his scientific leadership in the development of the MSRC community of scholars, practitioners, and students. And what a leader he was! Akira led because of the power of his ideas and his ability to quietly get people to work together to find solutions. Akira was the ultimate collaborator. He was one of the "golden threads" that made MSRC such a productive learning community. With his loss, important segments of the fabric seemed to unravel; no one has quite been able to weave them back together. It will come, but some time must pass before the void is filled.

Somewhere in heaven, we suspect that Akira is smiling and working to answer once and for all the age old question of "how many angels can dance on the head of a pin?"

Seeing Double – Akira Okubo and the Midges *Anarete pritchardi*

Alan J. Elliott
Marine Science Laboratories, University of Wales
Bangor, Menai Bridge, United Kingdom

When I first travelled to The Johns Hopkins University in Baltimore, Maryland, in 1972, it was to join the Department of Earth and Planetary Sciences and work with Professor Owen Phillips for a period of a year as a post doctoral fellow on problems related to surface waves. However, I was given an office -- not in the same building as most of the Department – but at the far end of the campus in the building occupied by the Chesapeake Bay Institute (CBI). It was further suggested that I might take a good look at the Institute and see if there were any active research projects that looked interesting.

As a consequence I found myself settled in an office on the ground floor of the CBI building, wedged in between Francis Bretherton and Ray Montgomery, with Don Pritchard's office located at the end of the corridor. Bretherton and Pritchard quickly convinced me to consider preparing a proposal for submission to the National Science Foundation (NSF) on the development of depth-resolving estuarine models. The proposal was funded and led to the collection of current, sea level and wind data over a year long period and revealed the variability in the "classical" picture of the residual circulation within an estuary. Thus to my good fortune, by the end of my post-doctoral year, with the successful grant application to NSF, I was to spend a further three years in Baltimore.

The CBI building was a fine example of a stratified society. With the exception of myself the ground floor was generally filled by full professors and the status of the occupants on the other floors decreased as one moved upwards. The lowest members in the pecking order (the graduate students, among whom were counted Alan Blumberg and Breck Owens) were grouped together on the fourth floor. The top floor also housed the data processing group and contained a number of machines that were used to generate punched cards for programming and data entry. These machines were also connected to a couple of optical/mechanical devices that converted the data photographically stored in current meters into digital form. The current meter digitizers were manned by a rota of casual staff hired from a local secretarial agency.

The same room also contained in one corner a light table at which a very quiet, slight gentleman was busily examining images projected by a light source onto a grid of graph paper. This, I was to discover later, was Akira Okubo, one of the finest physical oceanographers of his generation. It was typical of Akira's gentle personality that I was not introduced to him on my first days in the department but had to discover him informally at coffee time (I can also remember with wry humor that the finest office in the building – the only one with a reclining leather chair – belonged to an administrator whose main actions involved handing a set of keys to new staff members. Akira was awarded no such sumptuous office or leather chair; he was banished to the badlands of the fourth floor where he did not even have an office - just a table in a corner of the data preparation room).

I spent many hours on the top floor during my first year at Johns Hopkins, punching data and programs onto cards, and Akira was always there bent over the light table. I think that it was quite a while before I got to know him sufficiently well to enquire about his activities – and I then received an account of his midge swarming analyses. The main points are now well known since this work is described in his extremely readable book *Diffusion and Ecological Problems: Mathematical Models* (Springer-Verlag, 1980).

In essence, a white sheet was placed on the ground in a corn field at midday and this attracted a swarm of midges that hovered in a nearly horizontal patch at a height of about 5 cm above the ground. The midges, which only measured about 2 mm in length, were then photographed by a high speed cine camera. Akira's industry at the light table led to individual midges being identified on consecutive frames and their positions determined in the x-y (horizontal) plane. The data set could then be analyzed to determine the velocity of individual insects and the statistics of the patch and the characteristics of the swarming behavior.

To digitize just one of these data sets must have been a formidable task. But Akira had the tenacity to do them all twice. This action was not entirely of his own choosing but was a consequence of serendipity. The analysis was intended to be two-dimensional since the midges were known to swarm above the sheet. However, during his analysis Akira was frequently puzzled by the manner in which a single midge would suddenly separate into two individuals. Conversely, two individuals sometimes combined to form a single midge. When he realized why, it was not good news: he had been plotting the shadows of the midges along with the positions of their bodies! The two merged (separated) whenever a midge landed on (took off from) the white sheet. As a consequence his data sets were worthless: bodies and shadows had been confused in the digitization of the cine images. At this stage I suspect that most of us would have given up and moved onto a new and less tedious problem.

But Akira was not like us. He realized that, by knowing the height and bearing of the sun at the time each film was exposed, he could use the shadow of a midge to determine not only its x-y coordinates, but also its height above the ground. He could therefore solve the three-dimensional problem by redigitizing his films. The second reading of each film must have been significantly more difficult than the first because not only did consecutive midge and shadow positions need to be identified but each midge had to be put into a correspondence with its shadow - and the two sets (midges and shadows) had to be kept separate.

Such tasks occupied Akira during the months of 1972. He left Johns Hopkins during 1973 and travelled to Seattle, Washington to work on problems related to the ecology of deer. This probably marked his formal transition from a physical oceanographer to a mathematical ecologist.

The last time that I saw Akira was during the summer of 1990 when he spent some months at the Mathematical Institute of Oxford University and came to visit us for a couple of days. He was researching the spread of the grey squirrel in Britain and using it as a possible model for the spread of AIDS within a hypothetical human community. It was certainly one of the few times that our physical oceanography group has sat through a seminar on a largely ecological problem – and enjoyed it.

There was, several years ago, a sudden enthusiasm for "multidisciplinary" studies within the UK. It was rumoured that the funding agencies would look more favourably on applications that combined, for example, an analytical chemist with a benthic biologist with a physical oceanographer. I was not convinced by the argument: neither was the funding agency, for when the applications were ranked the "multidisciplinary" ones came near the bottom of the heap. This experience suggested to me that it is not groups that can be made to be multidisciplinary - but only very special individuals who have a flair for such work.

Akira was one of the finest examples of the multidisciplinary scientist: a man with many talents and immense physical insight. The Okubo oceanic diffusion diagrams (graphs of the horizontal diffusivity versus length scale in the ocean) serve as a reminder of the legacy left to us by an extraordinary scientist.

Phytoplankton and Turbulence: A Vignette

Trevor Platt
Ocean Sciences Division, Bedford Institute of Oceanography
Dartmouth, Nova Scotia, Canada

In 1970, I made some measurements, using continuous fluorometry, of the short-term variations in chlorophyll concentration at a fixed depth on an anchor station in the Gulf of St. Lawrence, Canada (Platt, 1972). The idea was to compute the variance spectrum in wavenumber space to see if anything could be learned about mechanisms responsible for the known heterogeneity in chlorophyll concentration at the scales accessible to the measurements (from 10 to 1000 m, under the frozen field assumption).

Figure 3. Akira Okubo and Trevor Platt at the Okubo Symposium, July 1995 held at MSRC (photo by Lita Proctor).

The working hypothesis was that the fluctuations in chlorophyll concentration were under turbulent control: the ideal result would be a Kolmogorov spectrum.

In 1971, Akira Okubo spent several weeks visiting and working at the Bedford Institute. And so it happened that he was present when the results from the measurements described above were being analyzed. At that time, the computations required for a spectral analysis were more of an adventure than might seem possible now. But eventually, a variance spectrum for chlorophyll was produced and plotted on the line printer. A cursory glance showed that it was linear on logarithmic axes.

As I carried the hard-won prize back from the computer room to my office, I met Akira in the corridor. He looked at my output, took it from me, laid a ruler on it and penciled a rapid calculation of the slope.

"It is minus five thirds," he said.

We stared silently at the spectrum and at each other, as if we had seen something that we were not meant to see. As this drama was being played out, Akira and I were, perhaps, the only people in the world interested in this problem. Later, it was to create a minor growth industry. To be able to discuss my results with a sympathetic and competent colleague was invaluable for me: it is a debt that can never be repaid.

REFERENCE

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Akira Okubo and the Theory of Blooms

Lawrence B. Slobodkin
Department of Ecology and Evolution
State University of New York, Stony Brook, New York, USA

A PERSONAL NOTE

Akira Okubo was an elegant intellectual who became part of our lives because of a combination of Jerry Schubel's scientific good taste in hiring him to the Marine Sciences Research Center at Stony Brook and the remnants of feudalism that still haunt Japanese universities. By studying in the United States in his youth and working here for several years, Akira had stepped irreversibly out of the Daimyo system – by which a senior Japanese professor passes employment opportunities on to his own students only. To reenter the system is extremely difficult.

There are sometimes ways of getting around this – Koichi Fujii, a former Stony Brook student, got a professorship on his return to Japan but only after waiting several years and only in a new university in which there were no Daimyo. It was our good fortune that Japan failed to take Akira back. Later, when he was clearly internationally eminent, he was almost too close to retirement to be hired (the mandatory retirement age in Japanese public universities is 62 years).

Akira never really became assimilated into the American culture. He followed a very personal code of behavior characterized by hard work, abstemious habits and sensitive intelligence. His English was fluent but not spontaneous. In conversation each sentence was a carefully prepared statement. He never drove a car. He said that two thousand pounds of metal was too dangerous a mass for one person to move about. His physical regimen was inventive - before going on vacations he would increase the weight and distance he carried on his bicycle to get into condition for hikes and distance bicycling.

He had affection for many people and many things. My personal sense is of the loss of a lonely, sensitive and irreplaceable friend. I wish the friendship could have been even closer. He played the Japanese chess game Shogi, but not Go. I play Go but not Shogi. I don't know if he found Shogi players at Stony Brook, but I am sure I would have enjoyed a game with him.

Fronts of this kind also occur in terrestrial invasions, as in the squirrel populations invading England (which incidentally fascinated Akira).

A SCIENTIFIC CONNECTION

My relation with Akira was colored by references in his delightful and immensely readable book *Diffusion and Ecological Problems: Mathematical Problems* (Springer-Verlag, 1980) to of one of my early papers (Kierstead and Slobodkin, 1952). The paper described a mathematical model which set the hydrographic limiting conditions for the growth of a plankton bloom (phytoplankton, dinoflagellate or ciliate) in a chemically and physically suitable water mass, surrounded by water in which growth is impossible. Quite simply – the size of a water mass which is just big enough to permit increase of a plankton population is proportional to the square root of the diffusion rate at the edges of the water mass divided by the population's reproductive rate.

The lower the diffusion rate and the higher the reproductive rate, the smaller the minimum water mass size required for a bloom. The idea was derived from critical mass theory of nuclear chain reactions. The English statistician, Skellam, simultaneously published a small book in which he described the spatial pattern of a freely growing population (Skellam, 1951). The two systems were effectively equivalent.

THE SIGNIFICANCE OF KISS

Since Akira managed only with difficulty to pronounce my last name, he constructed the fine acronym KISS standing for Kierstead, Slobodkin and Skellam as the name for this sort of model. Later this became part of what may be called front theory.

Front theory superficially means what it says and is not an acronym for anything, but actually has at least two disparate meanings. To population geneticists, and one large group of mathematical modelers, front theory refers to the waves that are promulgated by certain kinds of reproductive processes.

A new mutant initiated in a population dispersed on a flat surface may spread through the population. Its frequency may prove highest near the edge of its range of dispersal - producing a front that moves through the population. Fronts of this kind also occur in terrestrial invasions, as in the squirrel populations invading England (which incidentally fascinated Akira).

Another use of front theory, which is closer in spirit to the theory of red tides, is the common observation that oceanic productivity may show a local maximum at the contact line between two discrete water masses. The rationale is that water masses differ in their chemistry, so that if one of the adjacent masses is short in, say, phosphorus and the other in, say, usable nitrogen, the contact zone between them, where mixing is occurring, will be a better place for phytoplankton than either water mass. Following whale migrations has shown that their migratory patterns follow fronts of this sort; if they did not food would be inadequate (Philip Dustan, personal communication, 1994).

The first kind of front theory supplies the opportunity for mathematical theories, while the second is not as interesting to mathematicians.

My paper with Kierstead in its original form was of very little mathematical interest, although it became of greater biological interest when it had been enlarged and modified by Akira and others. It was built to enhance understanding of a narrowly defined biological problem by ignoring what was then considered the normal approach to problems of phytoplankton. It substituted geometric and hydrodynamic properties for biochemistry. This substitution made certain questions unanswerable in any detailed way but in return permitted extremely practical conclusions about specific plankton blooms.

Writing in a memorial volume to Akira Okubo provides an almost unique opportunity to expand on this distinction and its significance. It is not merely a review of a problem that vanished decades ago, but rather is still current as can be seen in recent studies of brown tides in estuaries and front problems in the open sea.

THE ORIGIN OF KISS

I finished my Ph.D. in 1951, having completed a laboratory study of *Daphnia* population dynamics. I was immediately appointed Chief of Red Tide Investigation for the US Fish and Wildlife Service. I had never cultured a phytoplankter (other than *Chlamydymonas* which I raised to feed to *Daphnia*) nor had I studied any biochemistry or plant physiology at all. I still believe my appointment occurred because the mystery of the red tide was a matter of some public relations importance to the Fish and Wildlife Service and they had no desire to see that mystery dispelled.

Prior to 1952, the traditional way to study any planktonic organism was to identify it and if possible, characterize in what ways it was unique. For microorganisms, one proceeded to examine the organisms in culture tubes, following the model of medical bacteriology. By altering physical and chemical circumstances one could make the organisms grow, encyst, or die. In the case of pathogens of humans, the conditions which caused these alterations could be considered as indicative of potential methods of curing patients infected with the organisms.

In limnology and oceanography, pathogenicity was usually of less significance. Phytoplankton and water chemistry were studied from the standpoint of biological productivity. Photosynthetic micro-organisms lived in culture media which were generally simple compared to the requirements of bacteria. It was of course difficult enough to get the precise mixture of nutrient chemicals, temperature, light regimen and trace elements (and as was later discovered, trace organics and vitamins) to make a phytoplankter grow. Nevertheless, skilled hard work could make it happen.

Once the isolation, identification, and growth conditions of a particular organism were established it was quite reasonably hypothesized that when that phytoplankter was found in nature the same, or similar, conditions were being fulfilled in nature.

This investigative process provided fundamental information on physiology and biochemistry for many organisms. It took on special meaning when concern was focused on blooms, such as red or brown tides. By extension from medical microbiology, it would seem that the organisms ought to grow in nature under the same conditions that permitted them to be cultured in the laboratory, and conversely, altering conditions in nature so as to make growth or survival impossible was presumably one way of dealing with toxic and nuisance blooms.

While efforts in the laboratory focused on physiological properties of the organisms, the overwhelming majority of field studies, at that time, consisted of what were called surveys. The classic wisdom of biological oceanography, going back to the 19th century, stated that pure water and sodium chloride were obviously inadequate media for algal growth. Phytoplankton required nutrient salts – usually called nutrients. It was generally understood that there were probably a fair number of these and they had to be present in the correct concentrations.

Phosphorus as dissolved and particulate phosphate, nitrogen as nitrate, nitrite and ammonia and dissolved oxygen and sodium chloride were the usual materials analyzed in each sample of seawater. The state of analytic chemistry was such that it was impossible to analyze samples as quickly as they could be collected. Therefore, any normal oceanographic laboratory had a vast collection of stored water samples waiting for ultimate analysis.

The Red Tide Laboratory of the Fish and Wildlife Service was located in Sarasota on the west coast of Florida. It had been established in response to the popular outcry that accompanied the red tide outbreak of 1947, which occurred in Miami on the east coast of Florida. After the establishment of the laboratory, red tide outbreaks did occur on the Florida west coast – but not with the same species that had been found in the Miami bloom.

The laboratory had a research vessel approximately twenty five meters long with a crew of three (including one sea-cook), two chemists, a biologist and a secretary. I was 23 years old and the laboratory director had never done any oceanographic research. Before I arrived, they had established a pattern of cruising along the coast, collecting samples for chemical analysis and microscopic examination of phytoplankton and zooplankton, plus storing them in a warehouse.

Microscopic examination of the water when there was no active bloom showed none of the bloom organisms at all. Underlying the chemical analyses was the tacit assumption that when chemical conditions of the water changed, they more or less closely approximated the optimal culture conditions for one or more of the phytoplankters in that water which then increased in abundance. The fact that when the bloom occurred, it was of an organism that was not present in the normal sea water was interesting.

Also, the nutrient concentrations of the normal seawater were very low and did not change very much with time and certainly did not change in advance of occurrences of blooms. It was known that chemical enrichment of surface waters could occur by upwelling of richer deep waters along continental shelves. However, the continental shelf on the west coast of Florida was about one hundred and eighty kilometers out to sea and there was no evidence that upwelling was significant (I believe that more recently some signs of upwelled water reaching the coast have been found). When red tides occurred, the total nitrogen and phosphorus in the water was orders of magnitude higher than that found in non-red tide water.

When red tide organisms had been collected during the blooms, and attempts made to culture them in seawater, they died. No serious attempts were being made at the Sarasota Laboratory to culture non-red tide organisms.

In short, the Florida red tides were being produced by organisms that did not occur in the plankton, in water that did not normally exist, laden with nutrients that could not have arrived by upwelling.

The situation was not encouraging, although the amenities were being observed by maintaining a sampling program and keeping the warehouse full of as yet unanalyzed samples. The citizens of the town could see that there was a busy laboratory on the main pier. Congress knew that the "Mystery of the Red Tide" was the subject of research and a kind of stalemate had been reached.

The paradoxical nature of these red tides disappeared when I focused on hydrography rather than water chemistry. The argument of a preliminary theory of red tides ran thus:

1. If the red tide cannot occur in normal Gulf of Mexico water, it must be occurring in abnormal Gulf of Mexico water.

2. By definition abnormal water occurs in smaller quantities than normal water and if the two kinds of water mixed, the red tide would be impossible.

3. In the absence of solid barriers, sharp density gradients resist the forces that mix water masses. I tentatively assumed that the abnormal water had a significantly different density from the normal.

4. Temperature and salinity differences are the major components of density differences between water masses in nature. Blooms should therefore be associated with either high or low salinity water masses.

5. Since Gulf of Mexico water is already of unusually high salinity, I guessed that the low salinity waters would be significant in the Florida blooms. When John Howell, the laboratory biologist and I examined the bayous inland from the shoreline, we found that the red tide dinoflagellates were normal inhabitants of the brackish pools.

6. The historical record, scientific literature during the twentieth century and newspaper accounts during the nineteenth, showed that the red tides on the coast of Florida were often associated with a recognizable pattern in which a relatively dry period was followed by very heavy rain, which was in turn followed by a wind and tide pattern which might be expected to send a pool of brackish water, containing the dinoflagellates, into the Gulf (Slobodkin, 1952).

Henry Stommel, a brilliant physical oceanographer who spent most of his career at the Woods Hole Oceanographic Institution, listened to the story and put me in touch with Henry Kierstead. Between us we developed a more formal model (Kierstead and Slobodkin, 1953).

Luigi Provasoli, at the Haskins Laboratory in New York, took our samples and mud from the bayous and succeeded in culturing the red tide organisms after diluting the seawater with approximately 10% fresh water. The essential ingredient in the mud was later shown to be vitamin B_{12}. Of course the abnormal, viz., bayou water has a reasonable amount of nutrients for the beginnings of the bloom, and once the first fish dies phosphorus and nitrogen are abundant.

During the next red tide we used the brand new seawater conductivity sensors aboard the MV *Alaska* (which apparently spent its life in the Gulf of Mexico) to cruise repeatedly through the edges of the colored water. Despite peculiarities of wiring and slight malfunction, it was immediately obvious that the red tide was in a discrete water mass differentiated by its salinity. Feeling vindicated, I immediately telephoned my superiors in Washington. I enthusiastically announced to them that the mystery of the red tide had been partially dispelled. They were not amused.

I was told to stop this sort of theorizing, to concentrate on the sample collecting cruises, phytoplankton examination of Gulf water and on the creation of a synthetic medium for raising the dinoflagellates. I was also told not to publish. The following morning I took my manuscripts of the two papers I was working on and quit my first post-doctoral job.

While the general theory was qualitatively correct, it was distressingly anecdotal and incomplete. I was thinking in terms of a small inoculum into an appropriate bubble, which then had to survive for around twenty days to permit bloom concentrations to develop. It has since been demonstrated in Florida, the Gulf of Maine, Milford Harbor (Connecticut) and elsewhere, that many red tide dinoflagellates have an elaborate system of alteration of generations which produces a heavy population of spores on the sea bottom. When a chemically and physically appropriate water mass moves over the spore bed there can be a mass emergence, very quickly producing a red tide.

There is now a vast literature on red tides, summarized in excellent symposium volumes (e.g., Taylor and Seliger, 1979; Cosper, Bricelj and Carpenter, 1989) and the symposia continue (e.g., Scottish Association for Marine Sciences, 1996).

WHY WERE THE FISH AND WILDLIFE OFFICIALS UNHAPPY?

Over the years I have been mildly resentful of what I saw as the Fish and Wildlife people's lack of perspicacity. Recently I have come to understand their problem a little better. Aside from their mendacity in wanting to preserve "The Mystery" with minimum disturbance or effort, they also had a clear goal which my attitude did not address. I was concerned with the paradoxical character of the red tides and therefore focused on how they might originate. The currently accepted story of the origin of red tides is essentially as I have described it. Brown tides, now a chronic plague of Long Island's Great South and Peconic Bays seem to need high salinity water, rather than low salinity but it still must occur in stable water masses. The basic expectation of an isolated water mass producing a bloom stays pretty much the same.[1]

There is an easily verified phenomenon in which a bottle of seawater or brackish water is loosely stoppered and kept at constant conditions for a fortnight. I believe that usually such a bottle will develop a bloom of something!

But, by analogy with medicine and with terrestrial agricultural pests, the research emphasis had, for the last seventy years, been on "curing" the "sick" water by adding something to the bloom to make the toxic organisms vanish or become harmless. It has been seriously suggested many times that copper sulfate, which in proper dilution kills algae in fish tanks without killing the fish, might be added to red tide. Occasionally it has even been suggested that large helicopters might stir the water in a way that would make the red tide disappear. Also, anti-fouling paints have also been suggested, since they are successful at eliminating many plankton organisms. But paints are not mixed into the water but form a toxic micro-layer on a hard substrate.

I can imagine nothing short of altering water flow patterns that could possibly prevent red tides.[2]

I find it impossible to imagine any non-living substance, however innocuous, that could be added in any practical way to ameliorate the effects of an existing red tide. The recent silly experiments on adding iron to the ocean to control CO_2 concentrations in the atmosphere demonstrated that this kind of thinking still persists. Even such innocuous materials as milk, sugar or sand, if used in appropriate quantities to make a serious difference to a water mass, would each present a major environmental impact.

Does this imply that there is no possible remediation for biological phenomena that occur on the scale of the red tides (or for example, terrestrial phenomena such as the spread of squirrels, which was also studied by Akira)?

What seems necessary is a procedure which is highly specific for the organisms concerned, so that there is no collateral impact, a procedure which can be designed to be applied as widespread as necessary to complete the job, but which need not be introduced in massive initial quantities. These criteria are just those that help define an excellent weapon in biological warfare (Rosebury, 1947)![3]

Thus a viral or bacterial infective agent may be the only practical way to "cure" a red tide (Slobodkin, 1989).

In this context it is particularly gratifying that viral infections of natural populations of brown tide organisms have recently been found. These are contagious, vary in virulence and may actually eventually serve as a brown tide cure (Benmayor, 1996).

Akira affectionately called KISS the "Keep It Simple Stupid" theory; a philosophy he carried into all his research.

KISS THEORY IN THE CONTEXT OF ECOLOGICAL MODELING

The KISS theory entered the main stream of ecological mathematics as Akira Okubo developed it from the minimalistic formulations of Skellam, Kierstead and Slobodkin (Akira affectionately called KISS the "Keep It Simple Stupid" theory; a philosophy he carried into all his research). Minimalistic theories typically deviate from past theories by shifting the emphasis of research to a new direction and sacrificing realistic details by focusing on just a few aspects of a particular problem (Slobodkin, 1994).

Specifically, as presented by Kierstead and Slobodkin, population growth is reduced to a single number, a growth (cell division) rate assumed constant during the course of early bloom growth.

The rate of diffusion is also represented by a single number without giving consideration to the very difficult problem of how turbulent diffusion can be measured and parameterized in particular cases. The size of the water mass is taken as a linear dimension, regardless of its shape. Also, no mention is made of the initial distribution of organisms in the water mass. There was a mathematical analysis in Kierstead and Slobodkin's paper which demonstrated that after sufficient time had elapsed, the proportionality constant in the equation was of the order of pi (3.14) regardless of the shape of the water mass or the initial distribution of the organisms.

The equation therefore was of extremely limited predictive use other than to alert investigators to the apparent fact that some kind of discrete water mass is involved and to give some estimate of the possible minimal sizes of the water mass – of the order of hundreds of meters. It did not predict the occurrence of cysts in bloom organisms.

The result specifically said nothing about the pattern of population growth in the bloom, or the physiological changes that occurred during the bloom. Finally, the water mass was tacitly assumed to have a clear outline, from the standpoint of the life or death of the organisms, even though the border was defined by turbulent diffu-

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[1] *For example the New York Times for September 19, 1996 (p.16) carried an account of a red tide near Port Aransas, Texas in which state biologists described it as a "typical red tide" with rain water and nutrients forming a water mass containing the tide.*

[2] *The usual pattern of seashore development in which the shore is developed for houses, docks and bridges, will impede water flow and tend to increase the frequency of red tides.*

[3] *Biological warfare weapons may be designed to kill crops or livestock but if they are aimed at a human population they ideally do not kill but merely incapacitate temporarily. Agents which destroy bloom organisms should be lethal for those organisms and otherwise harmless.*

sion and therefore should have been either a gradient or a statistical entity.

There is now a large literature on mathematical ramifications and complications of the minimalist theory, starting with Okubo himself (1980) and continuing through the many volumes of the Springer Verlag series: *Notes in Biomathematics.* A critique of these studies is well beyond what is suitable for this volume, except to note the important role of Akira in initiating and stimulating them. It is of interest that he was one of the very few investigators who was a key figure in all the different aspects of the study of diffusion in ecology.

In conclusion, Akira Okubo's sense of how mathematics can contribute to understanding the interactions between physical processes and the spatial properties of biological systems has generated a major, varied and fascinating stream of intellectual effort which will be his lasting monument even after his many friends and fellow travelers have passed away.

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Wildebeest and the Marine Environment: Gnus from the Front

Simon A. Levin

Department of Ecology and Evolutionary Biology
Princeton University
Princeton, New Jersey, USA

INTRODUCTION

It may seem strange to have a paper on wildebeest appear in *Oceanography,* but it is quite logical in an issue dedicated to the memory of Akira Okubo. Akira, after all, saw no reason to create theoretical boundaries between the terrestrial and marine environments; and it was a tribute to the Marine Sciences Research Center at Stony Brook that he was able to pursue his research on a wide range of organisms in a diversity of habitats, within a department whose mandate normally would seem much narrower. It is fitting, therefore, that *Oceanography* takes an equally enlightened view.

Akira first brought theoretical attention to wildebeest as a model system for studying patterns of animal aggregation. Wildebeest form a diversity of spatial patterns, depending on conditions, ranging from long linear files during migration, to striking wave-front patterns during foraging.

It would be a shame, though, to have his ideas disappear, since undoubtedly they will stimulate others to go on to demonstrate their richness.

The mechanisms controlling these patterns involve environmental factors, resource limitation, and inter-individual dynamics; but in general, they all appear to be mediated entirely at local scales. In this way, wildebeest seem analogous to other terrestrial mammals, as well as to birds, insects, fish, marine invertebrates, and a variety of other organisms. Thus they provide a superb model system, and one that through Akira's influence has attracted much theoretical attention.

In my files is a long letter in Akira's fine hand, dated 4 December 1980, in which he writes:

> *Enclosed is a note on the wildebeest problem. Sorry that I have no time for making a typed copy (leaving for Minnesota tomorrow morning). It is only the beginning. I hope that you can continue from the point I stopped, or even make a deviation into the right direction.*

Akira and I never finished this project; we both went on to other, complementary approaches to problems of aggregation. It would be a shame, though, to have his ideas disappear, since undoubtedly they will stimulate others to go on to demonstrate their richness.

OKUBO'S WILDEBEEST MODEL:

I will describe here Akira's ideas on modeling wildebeest front patterns. Technically, some of these ideas were jointly derived; yet collaboration with Akira was often a case of being made to feel that you had thought up things that Akira had worked out long before. Along these lines, much could be learned from the final lines of Akira's letter to me

p.s. I bring some 35mm slides for animal swarming. If you want to use them in your talk, it is welcome. A.

In Akira's approach, patterns arose from an interaction between wildebeest and their resource (grass). Equations were derived for the change in the number of animals per unit area and the density of the resource. Local wildebeest density varied in response to the animals' tendency to follow resource gradients, and to diffuse (it would have been irregular to have an Okubo paper without diffusion); grass disappeared due to wildebeest grazing. This leads to a quite standard formulation. Akira also proposed an alternative equation for P, the density of wildebeest.

$$\frac{\partial P}{\partial t} + \nabla\left\{\frac{aP}{S} \; \nabla S + b\nabla P\right\} = \beta\nabla^2 P^2,$$

Its form is preserved here as a testimonial to Akira, and is henceforth to be known as the Okubo equation. It involves more complicated dependence of movement on the number of predators P per unit resource S, rather than simply on the absolute amounts of each variable; a, b and β represent constants.

Analysis proceeded along standard lines, looking for traveling waves, finding fronts, and analyzing their stability to small perturbations. It was very much a work in progress, and remains so today.

INDIVIDUAL-BASED MODELS:

Inspired in large part by Akira's influence, a variety of other approaches have developed for explaining patterns in wildebeest (Gueron and Levin, 1993; Gueron et al., 1996; Chao and Levin, 1998), fish and marine invertebrates (Grünbaum, 1994; Humston et al., in press). These efforts share the common features of focusing on individuals, and writing equations for how their movements respond to local environmental cues, including the positions or velocities of their neighbors. From such Lagrangian representations, one can hopefully derive macroscopic population statistical mechanics, proceeding where possible to Eulerian descriptions, as in fluid mechanics. Akira was a strong advocate of such mechanistic approaches to deriving diffusion approximations, as is clear from a reading of his classic text (Okubo, 1980).

In general, the theoretical literature has focused on the self-organizing aspects of patterns for these systems, ignoring both environmental factors and even the consumer-resource dynamics implicit in Okubo's approach. This is despite the demonstration (Segel and Jackson, 1972; Levin, 1974; Levin and Segel, 1976; Okubo, 1974) that such trophic interactions could give rise to patchiness. Indeed, in some cases it is because the patterns of patchiness in nature clearly do not match those generated by such models, and only individual interactions seem capable of explaining the observed patterns (Grünbaum, 1992).

In general, patchiness has different explanations on different scales, and models are needed that combine and compare the various influences. Little such work has taken place for wildebeest; but in the marine environment, interfacing fluid dynamics, environmental factors and inter-individual interactions has been a theoretical gold mine. Examples of such work are those of Hofmann (1988) for krill, Humston et al. (in press) for tuna, and Flierl et al. (1998) for a variety of organisms. In all of this, the shining star of Akira Okubo burns brightly overhead, showing the way.

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Akira Okubo as Colleague and Friend

Robert E. Wilson
Marine Sciences Research Center
State University of New York
Stony Brook, New York, USA

I want to relate some of my interactions with Akira in February and March 1995 before his health deteriorated. I wish to do this because they were so typical and because I have no doubt that the same types of interactions have been shared by many of his colleagues. They show Akira's generosity and the enthusiasm that he could bring to a problem. They also emphasize that Akira was a true modeller with an amazing facility for producing a quantitative framework which could be used to develop a problem.

Akira and I had discussed time series observations of water column temperature observations from a mooring in western Long Island Sound (Figure 4). The temperature difference between measurements taken at different depths in the water column exhibited large fortnightly and longer period fluctuations, presumably related to variations in the intensity of vertical mixing. We discussed whether these observations could provide quantitative information on this mixing associated with wind and tidal forcing.

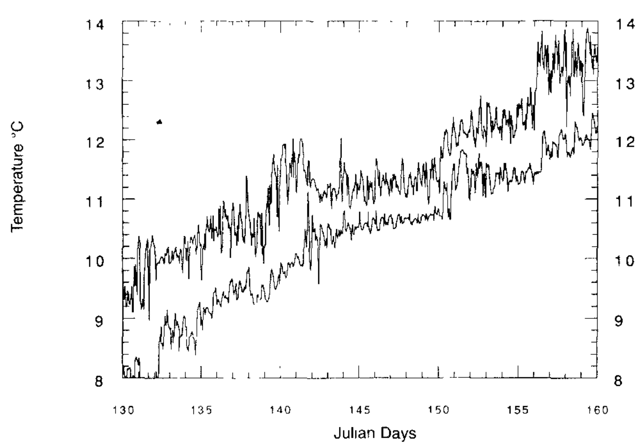

Figure 4. Time series for water column temperature at 4m and 16m at NOS Station F3 (73.18° W; 41.05° N) in western Long Island Sound.

Following our discussion, Akira produced overnight a first model which distilled our basic ideas. It was based on heat conservation for each of two layers. It was always easy to accept his model equations, often without appreciating the physical insight which had gone into the simplifying assumptions. The clarity of his logic was always so helpful and his analytical solutions could point to the controlling parameters or combinations of parameters, and the dependence on initial and boundary conditions.

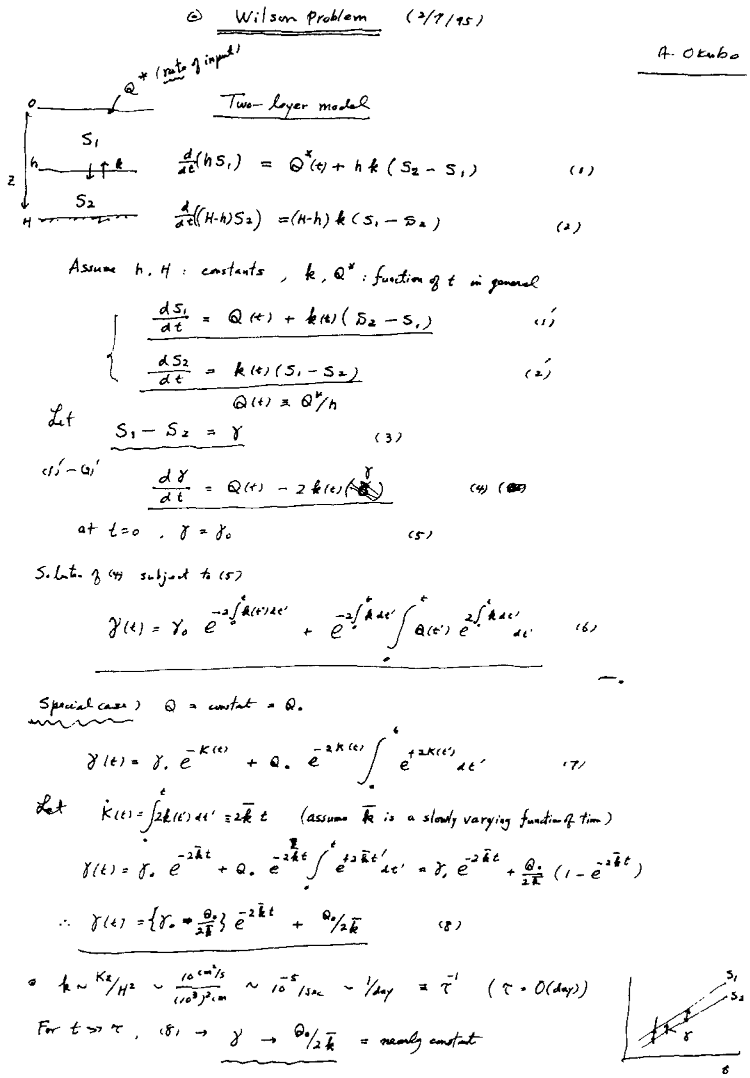

Figure 5. Okubo's refined two-layer analytical model, formulated while working on the "Wilson Problem" for vertical heat exchange in Long Island Sound, dated 9 February 1995.

After some discussion of this first model, Akira produced a refined model the next evening (Figure 5). It was mathematically more elegant and captured additional physics. Following the analytical solution there is his typical helpful discussion of the orders of magnitude of parameters involved. He never left you a solution without some helpful interpretations.

Akira produced a second refinement to this model which I have not shown here. Akira was very skillful in his abilities to make you feel as though the ideas were not so much his, just his articulation of your ideas. That is one thing that ingratiated him to those he interacted with. Friends and colleagues like Akira are very rare. I am sure many friends and colleagues, like me, keenly feel his absence.

Pumice and Mines Afloat On The Sea

Curtis C. Ebbesmeyer
Evans-Hamilton, Inc.
Seattle, Washington, USA

with a computer simulation by

W. James Ingraham, Jr.
National Oceanic and Atmospheric Administration
National Marine Fisheries Service
Seattle, Washington, USA

When distinguished scientists die before their time, many investigations invariably remain unfinished. In this memorial issue I will describe two of these endeavors with the hope that Akira's ideas will live on in the minds of young scientists.

I first spoke with Akira over the phone in 1969 concerning Snarks, the underwater clouds of anomalously warm water drifting through Dabob Bay, one of Puget Sound's inlets. As the years passed Akira made 50 trips to Seattle from the Marine Sciences Research Center at Stony Brook, funded to study physical and biological diffusion processes.

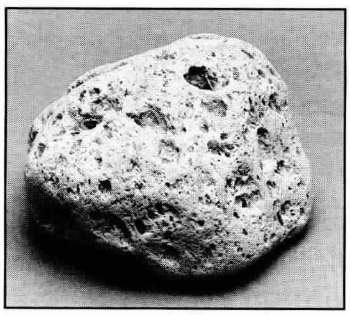

Figure 6. Gray pumice rock found floating near Antarctica in the 1960s by Mike O'Connell. In tank tests, it floated for almost exactly one year (photo by Nancy Hines, University of Washington).

In the off-hours we discussed topics centered around all manner of things flying and adrift, including the accelerations of swarming mosquitoes (Okubo et al., 1977), iceberg drift modeled like the migration of large animals (Ebbesmeyer et al., 1980) and a bottle that drifted from China to North America containing a plea for the release of Wei Jingsheng, China's famous dissident (Ebbesmeyer et al., 1993).

These and other studies we completed. However, two of Akira's favorite topics went unpublished: the origin of life in floating rocks (Figure 6) and the drift of World War II submarine mines across the North Pacific Ocean (Figure 7).

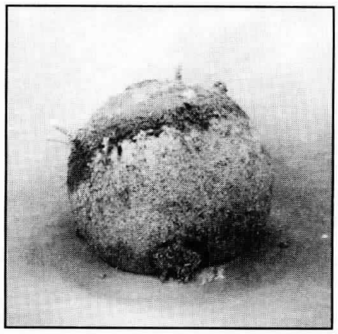

Figure 7. Sea mine from Japan washed ashore at Westport, Washington, during 1948-49 (photo by Brady Engvall).

ORIGIN OF LIFE IN FLOATING ROCKS?

Beginning in the 1970's Akira and I began studying many floating things of historic and scientific interest. Objects drifting on the ocean's surface are of considerable importance to the evolution of life in general and man in particular, but this flotsam has received little attention, and pumice far less. We theorized that life originated in floating pumice. We believed that publishing this idea would lead to lively and productive debates, thereby clarifying the conditions under which life on earth originated.

On July 25th, 1993, we submitted the following letter to *Nature* magazine. *Nature* never published our idea, so I take the liberty here to publish our theory in hopes that the debate will live on in our memories of Akira.

> *Dear Editor:*
>
> In the earth's early history, volcanic activity led to floating pumice as soon as the primordial ocean began forming. It is well known that volcanic eruptions typically result in large quantities of pumice and that pumice can float for years on the ocean. Furthermore, pumice, being a matrix primarily of voids and silicon dioxide, adsorbs a variety of chemicals as it floats around. Given that complex chemicals led to the origin of life on earth, and that pumice must have been present long before these chemicals evolved, it occurred to us that pumice drifting on primordial seas would have been locations where complex chemicals formed and concentrated. Therefore, they may have been the places where life originated.
>
> As the earth cooled and primordial seas formed, extensive volcanic activity may have resulted in pumice covering a large fraction of oceans that were much smaller than at present. Chemicals adsorbed to pumice interstices undoubtedly would have been exposed to high concentrations of energy via radiation from the sun and lightning strikes directly to individual pumice stones. After the pumice became stranded on surrounding land, evaporation and resultant concentration of adsorbed chemicals may have further aided in the formation of microbes.

Considerations of the earliest life forms evident in the sedimentary records suggest that diversity of life was rather great in those times. Given that the development of diversity requires sheltered or segregated environments, one of the most sheltered environments would have been the interstices in drifting/stranded pumice. We therefore envision microbes evolving in the various interstices and locations on the early seas. As soon as microbes left the protected pumice interstices, competition could begin in the turbulent ocean, thereby reducing diversity. If our speculation is correct, some corollaries should be explored.

Similar processes may be continuing at present. Pumice now drifting could be examined for the presence of adsorbed complex chemicals. Recovery of ancient pumice may reveal, with modern techniques, the presence of complex chemicals and microbes, much like the preservation of insects in amber. Useful laboratory experiments have been conducted, for instance, by applying lightning to sea water in the presence of ancient atmospheres.

We suggest that the addition of pumice and some oceanographic processes may provide interesting experiments. In the primordial seas, wind-generated waves undoubtedly produced foam which collected in windrows. In turn, the windrows also would have collected the drifting pumice, yielding a mixture of pumice and froth. The froth seems a realistic location for complex chemicals which in turn would find sheltered environments in the pumice interstices. Lightning striking pumice in this situation might have sparked the formation of complex amino acids.

The structure of pumice is undoubtedly fractal in nature (i.e. the effective surface area is very large). It is well known that chemical reactions proceed more rapidly on surfaces than in free water. Black pumice absorbs heat and promotes chemical reactions. Therefore, complex chemicals would be expected to evolve more rapidly within pumice than in the fluid ocean. As microbes evolved, undoubtedly some took advantage of sunlight. Cross sections of ancient pumice may reveal archeo-bacteria in the interstices thereby providing insight into the development of photosynthetic bacteria.

Many suggestions for additional experiments follow logically from our model for life's origin. We hoped our readers would pursue them.

NORTH PACIFIC MINE PLUME

The year Akira was born, 1925, led to a peculiar effect on his adult behavior. I first met Akira in person in 1972 over a large chicken dinner in our Dallas, Texas, apartment. My wife Susie, our daughters Wendy and Lisa and I ate modest portions of the chicken and rice entree while watching in respectful but disguised fascination while Akira finished his large servings.

As Akira's scientific reputation grew, stories of his appetite became legendary, yet all the while he remained rail thin. Only after Akira died did I understand how his curiosity about drifting mines was connected to this interesting gastronomic behavior. Akira, born and raised in Tokyo, turned 20 years of age when General Douglas MacArthur arrived aboard the USS *Missouri* to sign the treaty ending WW II hostilities. In a few more weeks or months the Japanese population might have faced widespread starvation, in part because the Allies' Operation Starvation had paralyzed Japanese shipping by air-deploying 12,026 submarine mines during April-August 1945 (Sallagar, 1974). Had the war continued another year it was estimated that 7,000,000 Japanese, Akira possibly among them, would have starved to death (Johnson and Katcher, 1947).

To counter the communists' promises to provide food in return for votes, General MacArthur had shipped in many boatloads of food. With each ship's arrival Akira related to me how he ate voraciously, usually in the evening. This initiated the habit I observed decades later.

Though the surrender on September 2nd, 1945 officially ended hostilities with Japan, damage to the large island continued during September and October from severe typhoons which also delayed extensive mine sweeping operations until December (Dimitrijevic and Ingram, c. 1945). Overall, although the number of cyclones for 1945 (35) was somewhat above normal for the western North Pacific area (31.4), the typhoons tracked northward over Japan more frequently.

Because these severe typhoons came at a critical juncture in WW II, the U.S. Navy issued a report prefaced by: " *. . . an enormous amount of material damage was caused by several of these storms; and, had not Japan surrendered when she did, these typhoons could well have had far-reaching and adverse effects on the course of the war (Dimitrijevic and Ingram, c. 1945).*"

Typhoon damage beneath the sea surface went mostly unobserved. High winds and waves tore free thousands of mines from their moorings, setting them bobbing with devil-like protruding horns in the ocean waves on the Kuroshio Current. As the typhoon season ebbed, mine sweepers removed 4,085 mines but discovered that most of the deadly steel spheres could not be located. Of the number of mines planted (15,130), 11,045 were left unaccounted for in seven harbor areas along the southern and eastern shores of Japan. Thus, only 27% of the mines planted were deactivated (based on computations for areas 3, 4, 5, 7, 9, 15, 23 in Lott, 1959).

All totaled, during 1943-1945 the Allies deployed 26,000 offensive mines around Japan and occupied territories, and Japan seeded an additional 51,000 mines (Johnson and Katcher, 1947; Hartmann and Truver, 1991). In 1948, the U.S. Navy officially estimated that 35,000 active mines were adrift on the North Pacific Ocean (Bristol, 1948).

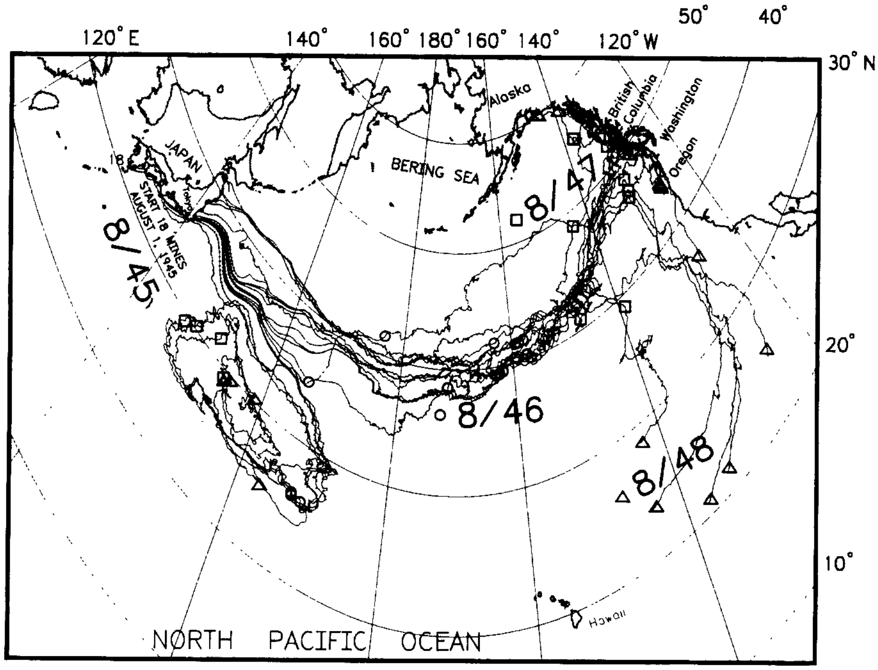

Figure 8. Trajectories of mines set adrift along Japan beginning 1 August 1945 according to the OSCURS model (by W. James Ingraham, Jr.). Drifters were released at 18 locations along coastal Japan (dots) and allowed to drift for three years. Symbols (circles, diamonds, triangles) indicate the mines' positions on the anniversary dates of the release.

Akira sometimes reflected as to what the fate of these mines might have been. Subsequently, we attempted modeling the advection and diffusion of the mines across the North Pacific Ocean. After Akira died, W. James Ingraham., Jr. of the NOAA Alaska Fisheries Science Center programmed the OSCURS (Ocean Surface CURrent Simulations) model to reconstruct the drift of the floating mines.

He selected 18 points at 60-mile intervals along the Pacific Coast of Japan at the beginning (1 August 1945) of the unusual typhoon season toward the end of Operation Starvation (Figure 8). Using OSCURS, the daily trajectories of drifters drogued at 20 m were simulated for three years using daily surface current fields computed with empirical functions. These were calculated from monthly (1 August -31 December 1945) and daily (1 January 1946-1 August 1948) gridded sea level atmospheric pressure data and long-term mean geostrophic currents (for OSCURS' description, see Ingraham and Miyahara, 1989; see also http://www.refm.noaa.gov/drafts/oscurs/default.htm).

These simulations assume zero windage because mines drift mostly submerged as a result of their high bulk specific gravity (note the water line evident on the beached mine shown in Figure 7). The OSCURS simulation illustrated that mines set adrift during August 1945 could have reached the coastal waters of North America two to three years later (Figure 8). The number of mines reported washing ashore on Oregon and Washington beaches was roughly proportional to the cumulative number of offensive mines laid by the Allies two years earlier: Nine washed ashore during October-May 1945-46 out of 2,000 mines planted by August 1943; 5 beached during 1946-47 from all 6,000 mines released up to August 1944; and 63 reported aground during 1947-48 of the total 25,000 planted by August 1945.

Model simulations indicated that those mines which did not come ashore in North America circled back toward Asia in three branches: (1) by recirculating immediately south of the Kuroshio extension; (2) along the California coast, then toward Hawaii in the great North Pacific gyre; or (3) in the Gulf of Alaska gyre along the Alaska Peninsula.

Extended OSCURS runs showed that after five years the currents spread them over the North Pacific, some having returned to Asia. To quantify the sightings of mines at sea, the mean and standard deviation of the sightings' latitudes within bands of longitude were computed from mariners' reports to the U.S. Navy Hydrographic Office during February 13th through August 31st 1946, as shown on North Pacific Pilot charts (U.S. Navy Hydrographic Office, 1946; Figure 9).

During this 6-month interval, the plume's center latitude and north-south width (two standard deviations) both increased with distance eastward across the North Pacific Ocean. Because the 506 mines observed represented only 1.4% of those estimated to be adrift (35,000), we were puzzled as to the whereabouts of the 98.6% of the mines not reported.

The probability P of sighting at least one mine from a vessel passing east-west through the mine plume is

$$P \cong 1 - [1 - (2w/4s)]^{n}, \tag{1}$$

where w is the distance at which sailors routinely spotted mines, s is the standard deviation of the mines in the north-south direction averaged over the North Pacific (i.e., the average of the dashed line in Figure 9), and n is the total number of mines adrift (derived from Hartmann and Truver, 1991, p. 193).

In this formulation the 4s width of the plume embraces 95% of the mines, assuming a normal distribution in the latitudinal direction. Substituting w = 100 meters, s = 481 kilometers (from Figure 9), and n = 35,000 mines, yields P = 0.974. In other words, observers aboard each vessel had approximately a 97% chance of spotting at least one mine during a single transpacific crossing. It appears that the number of mines reported was limited by the number of vessels observing the plume. Specifically, the 506 mines shown on the Pilot Charts were probably reported by approximately 522 vessels. In the first three years after WW II, 251 ships from many nations struck mines in the Atlantic and Pacific Oceans, and 116 either sank or were total wrecks (Lott, 1959).

For comparison, by redefining 2w as a vessel's beam width, equation (1) provides the probability that a ship might actually hit a mine. Assuming 2w≅10 meters, yields 17% as the probability that a vessel would contact a mine during a transpacific crossing within the mine plume. Assuming further that the rate of 522 vessels crossing the Pacific per 6.5 months applies to the three years following WW II, yields 2,891 vessel crossings, 491 of which potentially could have struck mines. The number of vessels actually striking mines was substantially smaller because crews maintained alert mine watches and the U.S. Navy swept the North Pacific shipping lanes.

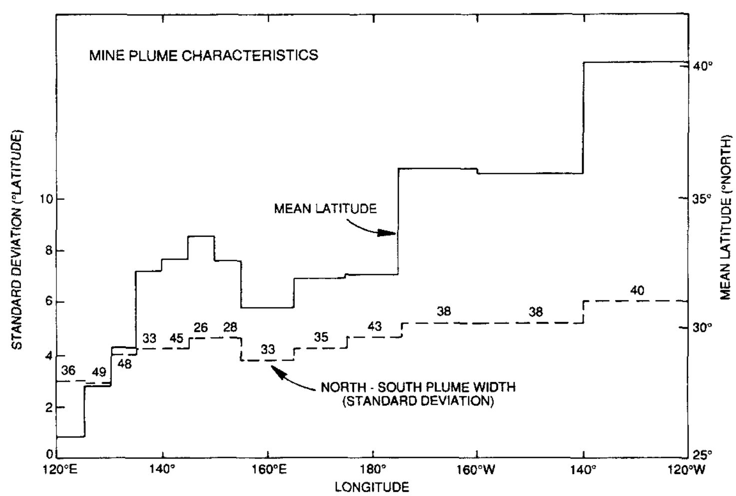

Figure 9. Mean (solid) and standard deviation (dashed) of 506 mines sighted during 13 February - 31 August 1946, within selected bands of longitude. The east-west width of the bands was varied in order to obtain a sample size exceeding 25 (corresponding number of observations given along the dashed line). The mine statistics were computed from sightings shown on the North Pacific Pilot charts for the period 13 February through 31 August 1946.

Despite great efforts, hundreds of mines went undetected and floated long enough to wash ashore on North American beaches (Figure 7). During 1955 and 1956, eight mines floated ashore in Hawaii, some of the last live WW II mines left afloat (Lott, 1959). By linearly proportioning the Oregon-Washington total of 63 during 1947-48, to 1 in 1955-56, I obtained a total of 267 mines washed ashore in Oregon and Washington during the decade following WW II. Applying Hata's (1963) result for drift bottles released in the vicinity of the mined areas with 44% found in British Columbia and Alaska and 56% in Oregon and Washington, yields a total of 477 mines estimated to have washed ashore in North America, equivalent to 1.4% of the total 35,000 mines thought to have originated around Japan.

CLOSING THOUGHTS

Because Akira studied drifting particles much of his life, it is fitting to close with an overview of the mine plume. As the mines drifted, they traced out a plume according to the mixing processes operating across the North Pacific during 1943-1946. From off Japan to the proximity of North America, the plume width increased from approximately 322 to 666 km (Figure 9).

According to the OSCURS model, individual mines could have drifted across the North Pacific for two to three years. Assuming that the mines lay within 2 standard deviations of the mean drift path, the plume widened at the rate of 1.5 kilometers per day. Of course, the plume expansion is not linear, but varies with the winds and currents. In the future, Jim Ingraham and I will continue exploring the thoughts of Akira.

By numerical experiments we plan comparisons of the mine plume and additional OSCURS simulations to explain the obvious differences between the present simulation and the statistical characteristics of the observed mines. We also hope to locate additional mine sightings as shown on Pilot Charts issued after August 1946, but which thus far have not been located. We hope colleagues knowing the whereabouts of these charts will contact us.

Picasso once said his greatest ambition was to view the world through the eyes of a child. Akira came the closest of any person I knew as evidenced by his continual association with young students from many disciplines, and his great curiosity about subjects as diverse as the origin of life, the history of WWII and his voracious dinner appetite.

ACKNOWLEDGMENTS

I thank Vincent J. Cardone, Oceanweather Inc., Cos Cob, Connecticut, USA for information regarding typhoons affecting Japan during August-October 1945 and Mike O'Connell for providing the pumice shown in Figure 6.

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Akira Okubo – The Man Inside the Man

Paul W. Sammarco
Louisiana Universities Marine Consortium (LUMCON)
Chauvin, Louisiana, USA

INTRODUCTION

Who was this man Akira Okubo? Who was this man whose work on the role of diffusion in ecology and oceanography influenced so many researchers around the world? Who was this man who was able to relay great wisdom and guidance with only a few words and the mere nod of a head? Who was the man inside the man? The purpose of this tribute to Professor Okubo is not to answer these questions, for I don't think anyone can do that but Akira himself, but to provide insight into the type of person he was – a great person.

REFLECTIONS

Embarrassingly enough, it was my fifth year as a Ph.D. student at the State University of New York at Stony Brook. I was struggling to keep up with assisting in two or three courses, while analyzing data and writing my thesis. Time was scarce, and the last thing I needed was additional courses. My supervisor, Jeffrey S. Levinton, asked me if I would enroll in a new course being offered entitled "Diffusion in Ecology," by a new faculty member at the Marine Sciences Research Center – Akira Okubo. The answer was "Thank you, but no thank you." The first request was rapidly followed by a second, with a similar reply.

Within a day or two, a short, demure, slight-of-build, gray haired, soft-spoken Japanese gentleman appeared at my office door. It was Professor Okubo. Bowing, he introduced himself ever so politely and invited me to take his course. Smiling, I explained my situation and respectfully declined. Bowing again, he ever so politely offered to allow me to audit the course, but implored me to register for it. I simply could not refuse this man, who overflowed with such kindness, respect, and humility. Needless to say, I submitted.

This turned out to be a very good decision, for that course changed the way I think about particle dispersal and transport processes in the ocean. I do not consider myself to be a mathematician by any stretch of the imagination. Professor Okubo, on the other hand, was a superb physicist and mathematician. While taking that course, however, I was able to understand with ease every concept presented. Why? It was because of Professor Okubo's approach to his subject, his style of teaching, and the principles upon which he focused, rather than the details.

He began the course by respectfully acknowledging that the students were comprised of non-mathematicians and non-physicists, and that the course was interdisciplinary. He firmly stated his belief that one should not need to be a mathematician or physicist to understand the concepts he was about to teach. He went on to say that he was determined to teach the sophisticated material in this course in a manner so that each student could understand each and every concept presented - as well as understand the essentials of the mathematics, irrespective of his or her background.

In that course, Akira taught each principle until he was satisfied that each student in the class understood it. The total amount of material covered in the course was less important to him than the complete understanding of what he taught to each student. I had never experienced this before in my previous ten years as a university student. He had the wonderful ability to present complex mathematical concepts in the most easily assimilable fashion. For someone whose first language was not English, he was an amazingly gifted and effective speaker.

Another time, I traveled to New York from Australia and visited Akira. We discussed our respective research projects at the time, particularly one of his regarding the relationship between particle size, shape, Reynolds number and sinking velocity in the ocean. The results of the study had astounding implications – and many potential applications. I then asked him about his research funding. Akira smiled and bowed and said he had received no funding for research that year. I told him I was sorry. He smiled again and said that there was no need to be sorry; he picked up the simplest of writing utensils which we humans have been using since the dawn of western civilization – a pencil and a pad of paper – and staring me in the eye with that look of peace, calm, and wisdom – said – "This is all I need to do my research. I do not need funding." This was a

Who was this man whose work on the role of diffusion in ecology and oceanography influenced so many researchers around the world?

man so self-sufficient and humbly self-confident, that nothing could come between him and his science.

Perhaps the one time I had the honor of getting to know Professor Okubo the best was when he accepted my invitation to be the keynote speaker at the Boden Conference in Australia (equivalent to the Gordon Research Conference in the USA). John Andrews, Malcolm Heron and I organized the meeting at a ski resort in Thredbo, New South Wales. Akira graciously accepted our invitation and made the long journey to Australia for the meeting of 23 invited researchers.

I sat and watched a man almost twice my age walk sprightly, cane in hand, at an accelerating pace up the highest mountain in Australia until he disappeared into the mist.

The topic of the conference was the bio-physics of marine larval dispersal. The meeting was a gathering of biologists and physicists working in teams on a variety of larval dispersal problems. An interdisciplinary approach to solving such problems was emphasized, an approach professed by Dr. Okubo for years. Five different phyla were examined, and two research teams were represented within each group. The meeting design, I thought, was ideal. What I didn't realize at the time was that Professor Okubo had planted the seeds for such an approach to scientific problem solving in my mind some 15 years earlier. He had a unobtrusive way of sowing ideas in one's subconscience, which might emerge years later in some project. The presentation of his keynote paper was, as usual, eloquent. It was subsequently published (Okubo, 1994) in the book based on the conference proceedings (Sammarco and Heron, 1994).

Akira was a man with purpose, determination, and great inner strength. The meeting was held in February, during the austral summer. Mt. Kosciosko, the highest mountain in Australia, was almost devoid of snow. We had reserved two half-days for the participants to relax, hike, experience the beauty of the local countryside and establish friendships with each other. Dr. Okubo, then in his mid-60s, decided to hike to the peak of Mt. Kosciosko. I offered to join him and he agreed. He wore a small green peaked felt hat, a flannel shirt, some hiking pants, boots, and a cane. He started at a fair pace and held that pace, walking continuously uphill. I kept up with him until about one half to two thirds of the way to the top of the mountain. I requested a rest. He agreed and waited. We started again.

Shortly after that, I needed another rest. I realized I was only holding him back and encouraged him to go ahead without me, as I would only be a hindrance. He smiled and agreed to leave me there while he continued up the mountain. And so, with mouth agape, I sat and watched a man almost twice my age walk sprightly, cane in hand, at an accelerating pace up the highest mountain in Australia until he disappeared into the mist. I expect that this is how he entered heaven.

A few more comments. The simplicity with which Akira presented complex scientific concepts was mirrored by the way he led his life, despite its underlying complexities. I learned so much from him whether by climbing a mountain together or chatting over a cup of tea by the fireside. He was a noble person who based his life on simple principles, holding true to them. He lived in the most humble of abodes – a one-room flat beneath a house – because he simply felt that was all he needed. Life challenged Akira continually, and he responded by meeting those challenges one after another with great inner strength. This was particularly evident in his last few years when he fell victim to cancer and exhibited a willingness to try any and all options available - no matter how risky or untried – to thwart this malady.

CLOSING COMMENTS

Akira Okubo was an amazing man and he had a profound impact on my life – both personally and professionally. He leaves behind an enduring legacy in his applications of diffusion theory to the fields of ecology and oceanography. These concepts he gave to science unselfishly, apparently risking his career by entering the field of ecology as a physicist, after completing a sabbatical to conduct research on what he considered a very exciting new interdisciplinary field – the application of diffusion physics and ecology to the study of insect swarming.

I miss Akira as a friend and a colleague. I am so thankful, however, for the good fortune of having known him and worked with him. I feel certain that, somewhere up there, he is walking with a cane in his right hand and a pencil and pad in his left – smiling and saying, "This is really all I need."

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Reminiscences of a Former Student

Brian G. Sanderson, *School of Mathematics*
University of New South Wales
Sydney, New South Wales, Australia

My association with Akira began in August 1979 when I undertook graduate studies at the Marine Sciences Research Center (MSRC) at Stony Brook. I had read some of Akira's publications on differential kinematic properties of surface currents and their effect on eddy-diffusion in the ocean. These works had been useful for interpreting drifter tracks I had measured in

New Zealand during my Master's degree research at the University of Auckland.

Akira was looking for someone to help with drifter studies for the so-called Lagrangian Eulerian Diffusion Study (LEDS) funded by the US Department of Energy. Malcolm Bowman, whom I had met on his sabbatical visit to New Zealand that year, invited me for Ph.D. studies at MSRC. He took care of all the formalities, put me in touch with Akira and within a month I found myself at Stony Brook.

I did not initially appreciate how accessible Akira was. As a beginner I was overwhelmed by Akira the renowned theoretician. But Akira practiced a very compassionate, very human, type of science. Working with Akira just ended up being such a natural thing to do.

Oceanography at MSRC was very multidisciplinary and it seemed that Akira could construct an elegant, fundamental mathematical theory to describe almost anything he was challenged with. Sometimes his theories were a little unconventional, even titillating, as in his manuscript: *Sperm Flux and Dispersal: A Diffusion Model* (which was never published for some reason). The point is that Akira saw the little twists and turns that make science fun; he always had a witty and imaginative perspective. Many at MSRC would tread the path to Akira's office, finding him hiding amongst his many books and papers.

Figure 10. Master and disciple. Akira and Brian Sanderson hiking over the hills of Newfoundland, Canada, ca. 1986 (photo by Deborah Bray).

One day I presented Akira with several pages of mathematical notes that turned out to be an erroneous proof of some now forgotten issue to do with flow field singularities. Akira used this as an opportunity to show me how some of my mathematics was related to a Hamiltonian formulation of a totally different problem; my error was exposed as an exciting process of discovery! From that day on I regarded Akira as my intellectual father.

Science could be described as a conservative and highly critical method of formulating and testing ideas that seem to work in explaining the mysteries of nature. It is fortunate that the scientific profession is occasionally graced by gentle people like Akira. There is something noble about a person who can undertake the critique, produce the ideas, and in the process make everyone associated with them feel good and that they contributed in a meaningful way to the study.

Thank you Akira, you made science joyful.

Akira's Visit to St. John's, Newfoundland

Ian Webster
CSIRO Land & Water
Canberra, Australia

During 1986 while I was a faculty member in the Department of Physics at Memorial University in Newfoundland, Canada, Akira paid the department a visit and presented an afternoon seminar there. After the seminar it was decided to take Akira out for dinner to a pizza restaurant, downtown, some distance from the university. The only problem was that of the nine of us who wished to attend I was the only one who had a car, and it was a small Japanese hatchback at that.

There was nothing to do but for all of us to squeeze into every nook and cranny of the car. Brian Sanderson and one of our graduate students being the largest members of our group had the luxury of occupying the trunk which left seven for the car's interior. I had a two year old daughter at the time, whose baby seat occupied some of the back seat. In order to squeeze everyone into the car it was necessary that the baby seat be occupied. Only Akira was slim enough to fit into it, and sit in it he did. Off we went.

It was a measure of the humility and serenity of Akira Okubo that he was totally unfazed by this bizarre and dangerous situation. To this day I have an image in my mind of an eminent Japanese gentleman dressed in jacket and tie sitting in the baby seat as if it was an entirely normal thing to do.

Notes on Simulation of Dispersion in an Eddy Field Covering a Wide Spectral Range

Gerrit C. van Dam
National Institute for Coastal and Marine Management/RIKZ
Den Haag, Netherlands

Akira Okubo must have been fascinated by the kinematics of dispersion. The number of reports and articles on the subject published under his name, quite often as the only author, is very large. He rightly paid much attention to the phenomenon of dispersion by the combined action of a shearing velocity and a mixing mechanism driven by velocity variations on a small scale, such that the latter can be treated as a "classical (Fickian)" diffusion process.

On the one hand Akira was rather fond of finding analytical solutions for well-defined but of course simplified cases; on the other hand he was well aware of the complex conditions in physical reality. This is reflected not only in his gathering and ordering of large sets of experimental data but also in formulating the restrictions for a hypothetical case, restrictions to make it analytically tractable.

Figure 11. Akira Okubo sitting on the first row (next to J. T. F. Zimmerman) at the international workshop on spectral characteristics of velocity variations in the sea and their significance for dispersion and mixing (The Hague, 1994). Behind him in the second row is his great Russian counterpart, the late Rostislav Ozmidov (next to Konstantin Korotenko).

As an example, in his paper (Okubo, 1966) dealing with an instantaneous source in a two dimensional velocity field with uniform shear combined with classical isotropic diffusion, Akira relates these conditions to physical reality as follows:

> To construct a working model of diffusion, we propose a hypothetical spectrum of turbulence which consists of two major parts: the large scale eddies and the small scale eddies. The two eddies are sufficiently separated in scale so that the scale of diffusion lies somewhere between the two scales of the eddies.

For the analytical solution to be worked out in the paper, the assumption is made that the 'small scale' is effectively of negligible dimension and the 'large scale' is in fact one eddy of infinite size (to obtain a uniform shear). Akira was well aware that in the physical reality one has a complete spectrum of eddy scales. The word eddy as well as the idea of a spectrum have no relevance to the rest of the paper if it is seen as a purely mathematical exercise. The introductory paragraphs clearly show that, in this context, Okubo was thinking much more as an oceanographer than as a mathematician.

The realistic case of simultaneous eddies of scales from small to large compared to the scale of the phenomenon to be studied, was undoubtedly often on his mind and he probably regretted that it seemed impossible to attack it analytically in a rigorous way. In a later paper (Okubo, 1968) he transfers, in a heuristic way, his exact solution for "the simple shear model" to the case of a growing patch in a field with a "continuous spectrum of turbulence."

Deriving in this way some classical cases of time behavior of patch size, Akira describes this result as an illustration of the power of the "shear-diffusion model"; it seems to explain everything. The paper ends with "Shear, shear everywhere!" (also see "Akira Okubo and Shear Dispersion" in this issue, by Konstantin Korotenko). But apparently he was still dreaming of a more explicit and more powerful concept for the case of a continuous spectrum.

When I visited Stony Brook in 1988 and presented the results of a computer program in which complete spectra were explicitly simulated by series of harmonic functions in space (generating a "synthetic eddy field"), he said that I had realized a dream of "every oceanographer." From this overstatement we might conclude that it was at least one of his own dreams. The basic idea of the model presented in 1988 in Stony Brook (Van Dam, 1988) and some early results are given in Van Dam (1980a); a first presentation took place in 1980 (Van Dam, 1980b). For an explicit description including equations refer to Van Dam et al. (1999).

In 1994 I organized an international workshop in The Hague on "Spectral characteristics of velocity variations in the sea and their significance for dispersion and mixing" and was very happy that Akira, on his way back from Italy was able and willing to present the opening lecture ("The historical view of oceanic diffusion," Okubo 1994) in which of course he gave due attention to the synthetic eddy concept. Okubo's contribution was followed by "Oceanic Spectra," presented by his great Russian counterpart Rostislav Ozmidov, with whom Okubo published an article on the subject during the time there still was an iron curtain between them (Okubo and Ozmidov, 1970). In Figure 11 the two are seen, sitting in the first and second row. This is an historical picture; a few weeks later Akira's serious illness was discovered; Ozmidov died in February 1998. I had the luck to see Akira one more time, at the Okubo Symposium at Stony Brook in 1995 (Figure 12).

Figure 12. Akira Okubo, Don Pritchard (left) and Gerrit van Dam, photographed at the Okubo Symposium at Stony Brook in July 1995.

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Random Walks with a Hana-Like Bias

James G. Mitchell
Department of Biological Sciences, Flinders University Adelaide, Australia

Among his many qualities, Akira was well known for his bike riding, not owning a car and not driving. It will come as a bit of a surprise to some then to find out that Akira was an excellent controller of cars and that on the occasional weekend evening he would slip into Manhattan, New York City, to help direct midtown traffic.

Akira's automotive talents were revealed to me the first time we visited Manhattan. We were on our way to the American Museum of Natural History. I had been at Stony Brook for about 2 months and Akira was not yet my Ph.D. co-supervisor.

Apart from the trip on the Cross Bronx Expressway, linking New Jersey to the bridges to Long Island, which I had taken when first moving to Stony Brook, this was the first time I had driven in New York City. Being from Los Angeles, many long hours spent creeping in straight lines along California freeways had prepared me for New York traffic, or so I thought.

Soon after arriving in midtown Manhattan. I was feeling a little bit daunted. Akira was sitting in the middle of the backseat, muttering something about all the people who lived in crowded New York City. But as always he was sensitive to those around him. Perceiving my tension, he poked his head forward as we entered Times Square and said something to the effect of: "Don't worry, just drive as if there are no other cars on the road." How much our car, cutting across lanes, contributed to the cacophony of horns in Times Square I do not remember.

Foreshadowing the letters I would find left on my desk over the next few years, Akira, as we drove, refined the Manhattan-car model with comments and crucial pieces of information. The thrill of driving in Manhattan seemed in part to spring from the helter-skelter dynamic of cars approaching very close together and then dispersing. It seems he relished investigating this kind of point and counterpoint, giving it, for example, the wonderful label of "interplay" in his "Fantastic Voyage" paper about microscale processes. He seemed truly delighted with that evening and our many subsequent trips to Manhattan, Cornell University in Ithaca and most frequently, the restaurant Hana, located in Port Jefferson, Long Island, not far from Stony Brook.

As a graduate student, the fantastic voyage for me was working with Akira on the motility of marine bacteria in a turbulent ocean. His analysis went to the crux of this interplay with characteristic sharp, unadorned identification and analysis of the vital principles in the process. During my early studies at Stony Brook I was working in Jed Fuhrman's lab, trying to count the number of bacteria in microzones (bacterial swarms around phytoplankton or particles). The results were always equivocal at best and in part this was because it was unknown exactly what microzones looked like.

A model was needed and Akira enthusiastically joined in, generously sharing his knowledge and skills of quantitative examination. Much to my surprise, it soon became clear how similar the microzones problem was to midge swarming [Alan Elliott; this volume] and wildebeest herding [Simon Levin; this volume].

In 1984, Howard Berg's *Random Walks in Biology* had just been published. The quantitative portrayal of the simplicity of bacterial motility fascinated Akira, as did the potential for analytically modeling creatures that lacked vision, feeding behavior, mating rituals and predator avoidance. Tracing the line of research on environmental motility led us back to the parent concept for bacterial swarming, the idea that soil bacteria congregate around legume roots, creating a "rhizosphere." Here was sweet serendipity.

In the soil beneath his beloved midge swarms, bacteria teemed to form their own tiny swarms. This realization was more than enough for a celebration at Hana. We teased apart the evidence for microzones over sashimi, adding back-of-the-napkin diffusion calculations for photosynthate excretion, phytoplankton movement, chemotactic drift and turbulence. As the research progressed, trips to Hana mounted. From them emerged the view of bacteria fighting against rapid spatial dispersal and separation in all but the most quiescient parts of the ocean.

When our paper on microzones appeared in the journal Nature (Mitchell et al., 1985), there was no special trip to Hana. The celebration and enjoyment had already taken place. Akira was such an excellent navigator in traffic jams and so enjoyed random walks because it was the voyage and not the destination that provided him the most exhilaration.

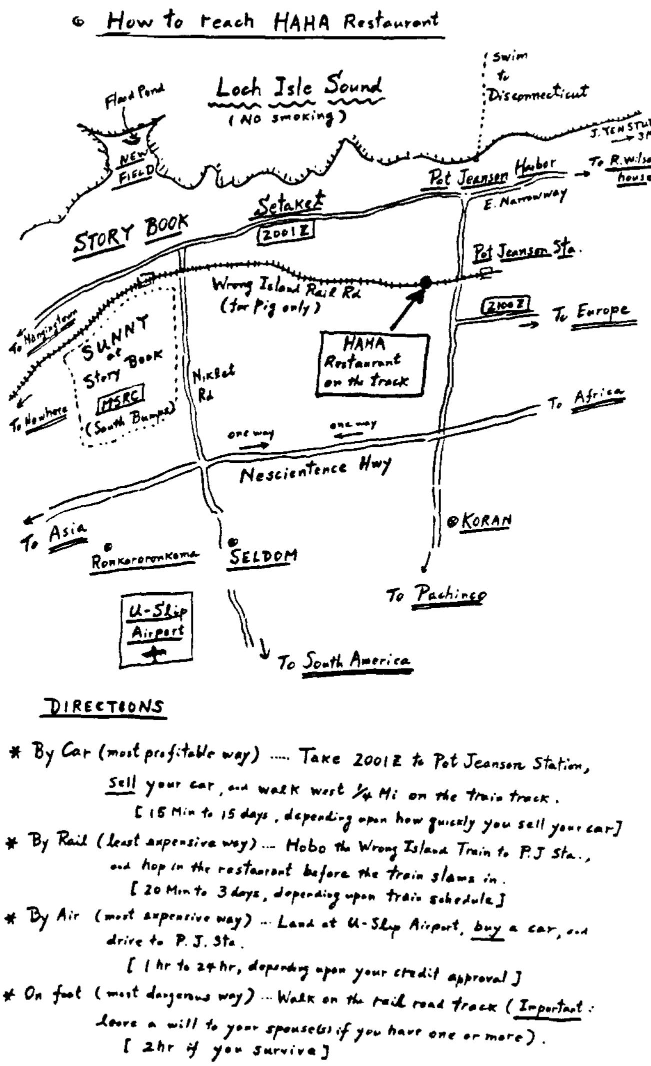

Figure 13. Akira's sketch map with travel instructions to Haha (Hana) restaurant in Pot Jeanson (Port Jefferson), Long Island, New York.

We did not make it to the American Museum that night, or ever for that matter. Despite enjoying many random walks together, thoroughly exploring the city's local environment, that particular location eluded us. Like marine bacteria, however, there was a bias to our walks, and as with bacteria the bias was towards a fundamental energy source, food! sashimi! This was fortunate for while we celebrated our bacterial discoveries, we always agreed that none of the distant microzones had the quality, atmosphere or memories of Hana (Figure 13).

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Okubo Sensei

Lita M. Proctor
Department of Oceanography, Florida State University Tallahassee, Florida , USA

Akira had the corner office in Endeavour Hall at the Marine Sciences Research Center (MSRC) at Stony Brook. You would always see him working there late each night, surrounded by his books and papers. The Fuhrman laboratory was also in Endeavour Hall. I am quite a night owl myself, so Akira and I quickly developed a routine where we either talked in his office in the evenings or went for a Japanese meal at the Hana Restaurant in nearby Port Jefferson. During my Ph.D. studies with Jed Fuhrman, that routine proved to be very important to me, both personally and professionally, in the months and years that followed.

Shortly after I started at MSRC, my father suddenly passed away. I needed some time at home with my mother but after three months, I started to feel that this kind of time wasn't good for me. I came back to MSRC the following spring but was probably ill-equipped psychologically and emotionally to pick up where I had left off. However, I knew I needed some way to focus my thoughts, something I could concentrate on.

That's when Akira began (what appeared to be at the time) these pointless conversations with me. He would drop by my office in the evenings and start talking about the tree outside his office, some childhood memory or eating Maryland blue crab. He never asked about my father. He simply kept up these one-sided conversations. I would sit there, not really taking anything in, but wondering how in the world this busy fellow had so much time to chat. Of course, over the next few months, I began to laugh at his stories and feel myself surfacing out of the depths of grief I had been in. Only years later did I recognize how Akira, in his own way, had been trying to help rehabilitate me that year.

Akira Okubo made other contributions in my life and these were as a teacher and a scientist. In my doctoral research, I studied the role of viruses in controlling bacteria in the ocean. When Jed first suggested this project, I could not imagine how I would relate the abundance of marine viruses to an estimate of how many marine bacteria are killed by viruses. Yes, free viruses were clearly abundant, I was getting a million to ten million viruses in a milliliter of Long Island Sound seawater. But how to estimate virus-induced death in bacteria?

I took John Sieburth's lead and began to thin-section marine bacteria and look at them under the electron microscope, the idea being that I would have a "snapshot" of marine bacteria with viruses. As many readers may know, electron microscopy is very tedious so I was eager to show off any new micrographs I managed to get. I would run down to Akira's office and show him my latest results. Of course, he could not make heads nor tails of the electron micrographs, and so began my attempts at teaching Akira microbiology.

These sessions became important lessons to me because Akira forced me to hone my ideas in order to explain them to him.

Figure 14. Lita Proctor, Akira and canine companion resting on the Pacific Crest Trail in the Sierra Mountains, California, 1990 (photo by Val Gerard).

As I explained microbiology to Akira, he would frequently ask me, "Is that so? And why is that important?" Each time he asked this question, I would refine the point I was trying to make, in order to reduce it to its most basic elements. I felt at that stage of my studies that my understanding of microbiology was weak; sometimes I felt I had explained well enough for him to understand and sometimes I didn't. These sessions became important lessons to me because Akira forced me to hone my ideas in order to explain them to him. In fact, Akira took enough interest in my work early on that he and I together wrote a small paper on the dynamics of virus/host encounters (Okubo and Proctor, 1989).

In the early stages of my research, I was initially disappointed at one apparent result that was beginning to emerge from my microscope work. A very low percentage - on the order of 1-5% -- of the bacteria in my samples had viruses. With papers appearing in the literature that protozoa were eating nearly all of the bacterial production in the water column, I thought this probably meant that viruses were not very important in the food web of the ocean.

This is where Akira's insights again proved crucial. As with many earlier conversations with him about microbiology, I would use diagrams. One day, I started to draw the classical "life cycle" of a virus when I suddenly realized why the percentage of infected bacteria in my samples was so low! He had often asked me how this or that part of the virus cycle was different. His questions finally registered and I realized that, unlike living organisms, most of the life cycle of the virus is invisible. It is only when the virus is ready to be released that it becomes visible. What I was seeing in my micrographs was only the visible part of the virus cycle. This meant that many more bacteria than were visible to the eye (and to the electron microscope) were potentially infected with viruses.

Akira immediately understood the implications of this point and helped me develop a simple model for estimating the actual number of bacteria in the sea which are infected with viruses. Later, with data from laboratory experiments conducted at the University of Southern California, we found that something like 10-15% of the life cycle of a marine virus is visible by microscopy, which meant that on the order of tens of percents of bacteria in the ocean are infected by viruses and that viruses represent a major impact on bacteria in the ocean (Proctor et al., 1993).

Akira's child-like curiosity and interest in all things biological played a major role in my scientific training. I still wonder today whether had he not quietly insisted on those explanations, would I have recognized what I was not seeing in the microscope? I feel lucky to have had Akira as a friend, grateful to have had him as a teacher and privileged to have had him as a colleague.

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Reflections

Stella Humphries
CSIRO Corporate Human Resources
Dickson, Australia

Akira Okubo was my external Ph.D. examiner. My thesis, submitted in 1983, was on the quantitative relationships between turbulence, stratification and primary productivity of algal populations. As an ecologist delving into the field of fluid mechanics, I remember clearly the trepidation I felt when my supervisor (Jorg Imberger at the University of Western Australia) suggested Akira as an examiner! To me he was one of the great masters in building bridges between ecology and physics, with clarity that only profound insight and wisdom brings.

But I am now glad that Jorg encouraged me to go to the top, for my next impression was one of a man who took a deep personal interest in people. His words as examiner were inspiring and uplifting and consequently gave me confidence to venture forth professionally. I had the great privilege and joy of meeting Akira on a short visit to Stony Brook in 1993. I was left with the impression that here was a man of great kindness, humility and cheerfulness – he was an inspiration and a master in life as well as in academia.

To me he was one of the great masters in building bridges between ecology and physics, with clarity that only profound insight and wisdom brings.

Akira and Insect Aggregation

Terumi Ikawa
Department of Liberal Arts and Sciences, Morioka College
Takizawa-mura
Iwate-gun, Japan

It was in the summer of 1990 when I first met Akira. During this visit to Japan, he visited the Tanashi Experimental Farm attached to the University of Tokyo. There my collaborators and I were working on three-dimensional measurements of mosquito swarming. Akira enormously enjoyed the day he spent at the Experimental Farm. Just like a curious small child, he was greatly excited, biking around the farm, munching on sandwiches, peering at our instruments for three-dimensional measurements and watching the mosquitoes swarming in the dark.

Insect swarming (or more widely, animal aggregation) was one of Akira's most favorite scientific topics. Akira loved to observe small insects swarming in the air. He was fascinated that the insects were never diffused but kept their swarming station, although they appeared to be flying randomly. He often told me that he was so interested in insect swarming that he once gave up his own career as a physical oceanographer so he could focus his attention on insect swarming. For some time he desperately tried to find an entomologist who might collaborate with him. The fruits of his enthusiasm and searching are his pioneering research on three-dimensional trajectories and behavior of midge swarming conducted with the eminent entomologist H.C. Chiang.

From 1991 to 1993, I had the pleasure of visiting the Marine Sciences Research Center at Stony Brook. I am always thankful that Akira and Jerry Schubel, the former Dean and Director of MSRC, gave me this great opportunity, welcoming me, even though I am an entomologist, not an oceanographer. It was my first visit to a foreign country. Akira gave me much helpful guidance about living in the United States; I know he did this for many foreign visitors. He enjoyed telling me about his life experiences, filled with many interesting anecdotes from thirty years of living in America. His stories incurably infected me with his love for New York City.

At MSRC, I continued to analyze three-dimensional data of mosquito swarming. From these analyses, Akira and I started to construct a mathematical model for mosquito swarming.

However Akira left his earthly home forever before we finished it. I still have my notes on the model; I hope to complete this project at some point in the future.

Aside from research, Akira and I enjoyed chatting about many things; insects, ecology and about his many adventures. Akira always loved to chat. He seemed to become more eloquent when talking in his mother tongue, Japanese. Usually, good Japanese food accompanied these conversations. Akira would say, "I cannot live without rice! Having sashimi is even better!" Looking at Akira eating and chatting pleasantly, I often wondered how he could be so productive both in mathematical ecology and physical oceanography, fields that seemed so far apart!

One day, Akira showed me a paper about the oceanic insects *Halobates*. Like many entomologists, I did not know that the insects could exist in the open ocean. *Halobates* are water striders which belong to the family Gerridae. The genus *Halobates* has 44 species and all of them live in marine environments. Five species of *Halobates* live in the open ocean. They are the only true pelagic insects out of more than one million species of insects in the world.

I was so impressed with the distribution map of these five species of *Halobates*. It looked like a huge tropical belt girding the three major oceans. I have never seen such a vast distribution area without any discernible break. It was almost impossible for me to believe that these insects could survive, find mates and reproduce on the ocean surface, with no shelter from wind and storm.

I pestered Akira and the other oceanographers at MSRC, asking many questions about the properties of the ocean surface and factors that could affect the survival of *Halobates*. Finally, I was successful in convincing Akira to take an interest in studying ocean striders. We made calculations about how far *Halobates* could drift apart by oceanic diffusion and how often males and females could meet on the ocean surface. We also calculated the steady state condition among the population growth rate of *Halobates*, the distribution size, and diffusivity. Based on the results of our calculation, we surmised how oceanic diffusion could influence *Halobates'* life history strategies (Ikawa et al., 1998).

I wish I could sit and share with Akira my recent work on *Halobates* and mosquitoes. I cannot, but still I am very grateful to have had the opportunity of working with Akira on some of his favorite topics.

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Akira Okubo and Shear Dispersion

Konstantin Korotenko
P.P. Shirshov Institute of Oceanology
Russian Academy of Sciences
Moscow, Russia

Along with Ken Bowden of the University of Liverpool, United Kingdom, Akira Okubo was a pioneer in the development of ideas related to the processes of horizontal shear dispersion in the sea. Akira's mathematical skills led him to develop enlightening analytical representations for the concentration fields for substances diffusing in shear flows. These representations elucidated the dependence of the diffusion process on some basic flow parameters. Because of their clarity they received wide acceptance and found considerable practical use.

Akira's work influenced my own research too. I have, for example, examined the horizontal spread of a substance due to the interaction of an unsteady current shear with transverse mixing in three spatial dimensions. My analyses have closely followed the derivations presented by Akira in the 1960s for a variety of flows. Akira's basic development of the advective-diffusive equation provided a firm basis for further refinements. His decomposition of the local advective flow velocity into a mean flow and a uniform shear (both of which could be time varying) proved to be very fruitful. His methodology also permitted the application of a wide variety of initial and boundary conditions.

The solutions he obtained provided a mechanism for understanding the nature of the interaction between current shear and transverse mixing in a particular flow. His use of moments to interpret the behavior of the analytical solutions and field data proved to be quite powerful. More generally, his solutions provided a basis for scaling and interpreting shear dispersion processes for a wide variety of flows encountered in nature.

His works will endure, and I wish to thank Akira Okubo in this tribute for the opportunity of meeting him on both sides of the Atlantic Ocean and learning from his great wisdom.

From Coast to Coast: Akira Okubo's Trek Across America

From one dimension: the man,
to two dimensions: the path,
to three dimensions: the life well-lived.

Jeannette Yen
Marine Sciences Research Center
State University of New York
Stony Brook, New York, USA

It is a fine summer's day in the month of June 1994. Rudi Strickler and I wait at the station in Milwaukee, Wisconsin. The whistle announces the approach of a chugging train. Out pops our intrepid visitor: Akira Okubo - with a beautifully tanned smiling face, khaki shorts revealing strong legs with well-worn hiking boots. No luggage except for his sturdy backpack and

First Recipient of Okubo Award Announced

Professor Martin Novak of the Institute for Advanced Study
at Princeton University, New Jersey, USA has been selected as the winner
of the first Okubo Award sponsored by the Japanese Association
for Mathematical Biology and the Society for Mathematical Biology.
The Okubo Award, which is offered the first time this year, is to be awarded
every other year to honor alternately a distinguished work by a scientist under 40
and lifetime achievement, with the first award going to a scientist under 40.
The selection committee cited Professor Nowak for his paper in Theoretical Population Biology
on stochastic strategies in the prisoner's dilemma. In addition to its own merit
of introducing the effects of error or uncertainty in strategies,
this paper triggered a stream of related work on the evolution of cooperation.

his trusty umbrella. "This umbrella," he waggles at us, "is so-o-o useful. Its shade keeps the hot sun from burning me. As a walking stick, it steadies my descent down a craggy mountain path. When passing through a field of tall grass, I can search out snakes ahead (he demonstrates by sinuously shaking the umbrella). And of course, it keeps off the rain." He smiles that multifaceted smile. And we smile, witnessing once again the multidimensionality that characterizes this great man.

Where was he going? Where did he come from? This tribute describes only a fragment of a life well-lived. His cross-continental walking journey provides only a few clues, a skeletal framework. His intent was "to walk across America." We couldn't imagine how this man – who did not drive, who had ridden his bicycle to work at the Marine Science Research Center – familiarly clad in his big yellow goggles, helmet, windshell – through sun, rain, sleet, and over and into potholes (once breaking his nose!) – thought that he would accomplish this? Of course, never in the conventional sense.

Akira would often travel on Thanksgiving, taking the train to Huntington, a town west of Stony Brook on Long Island, New York. He would then walk the 20 miles back home to join his landlady's family for Thanksgiving dinner. That was one leg of his journey he repeated every November. On other occasions, he would visit his colleagues, including Simon Levin [this volume], at Cornell University, in Ithaca, upstate New York, then take off walking from there – in various directions. Over the years, Akira made more than forty visits to his favorite American city: Seattle, where he wanted to retire (if he ever would really retire from the fascinating career he led). There, in the Pacific Northwest of the United States, Akira would hike in the rain forests, mountains, meadows, steep rivers and gorges.

Then there were those fortunate times when he would come to visit wherever we lived or worked. If it were summer, we could inquire into his itinerary and find several days with no activities planned. These were the days he would escape from his four walls and continue his trek, but not necessarily where he left off the year before. Akira's journey was noncontiguous. Stretches were completed here and there. We believe that he was nearly finished with this walk of a lifetime, left only with the long stretch across the central plains - a journey he may undertake now.

This tribute describes only a fragment of a life well-lived.

What did he do on the last leg of his adventure, this one of so many? After completing work with colleagues in Cornell, Akira walked from the Finger Lakes of upstate New York to the shores of Lake Michigan. Rudi Strickler and I then met him at the train station. We sped him through the streets of Milwaukee, dropped off his gear and - with our own multifaceted grins - led him into the darkened laboratory of the Center for Great Lakes Studies at the University of Wisconsin.

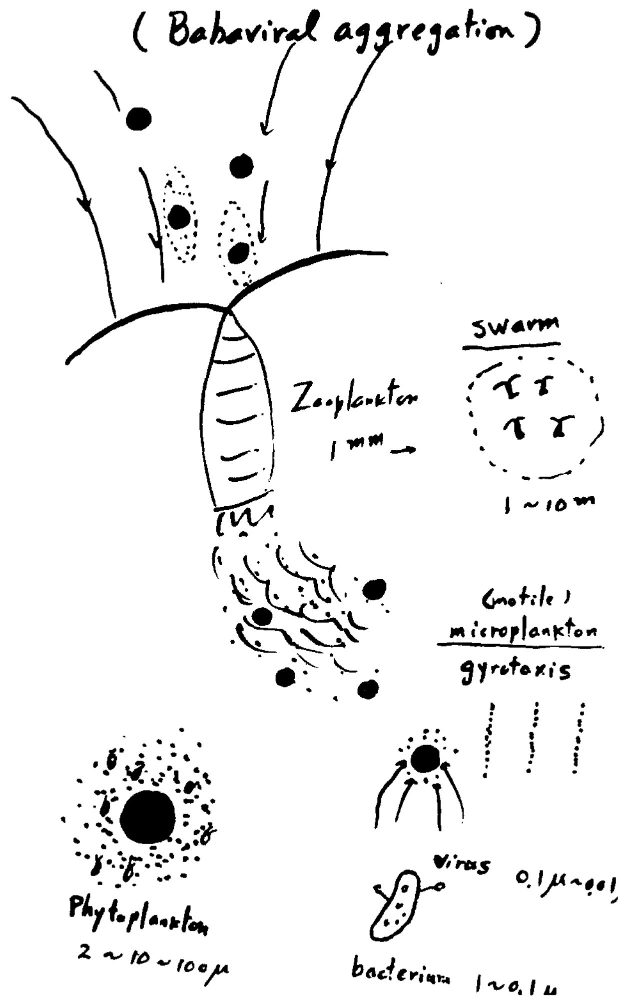

Figure 15. A page from Akira's sketchbook, illustrating various biological-physical interactions involving plankton. Clockwise from upper left: a copepod (~ 1 mm) entrains water, signals and food within its feeding current and releases particles and metabolites; a swarm of copepods can occupy a space of 1 to 10 m; the movement of motile microplankton can display gyrotaxis; a virus and bacterium rely on molecular diffusion; phytoplankton, ranging in size from 2 to 100 μm, also release various kinds of exudates into their surroundings.

As our eyes adapted to the darkness, he stepped carefully forward toward a miniature whirling discotheque light flashing red and blue. He watched and looked, cocking his head to catch the glint off the tiny spherical dancers reflecting off those red and blue lasers. "Here they are, Akira! Those pesky zooplankton, swarming as you modeled in 1984 and as you imagined 20 years ago when you watched those midges in the cornfield." The blue light caused a dramatic display: wild little cladocerans ran out of their corner of the vessel to line up on the laser beam. Akira was captivated, just like those little plankters.

"Can you set up another laser?" Akira asked. "I would like to be able to change the distance between the swarm markers to see if these animals can demonstrate the effects of another strange attractor." Rudi and I looked at each other, knowing what a great time we would have working with Akira on this joint project: to study swarming by zooplankton supported by the National Science Foundation (Figures 15 and 16).

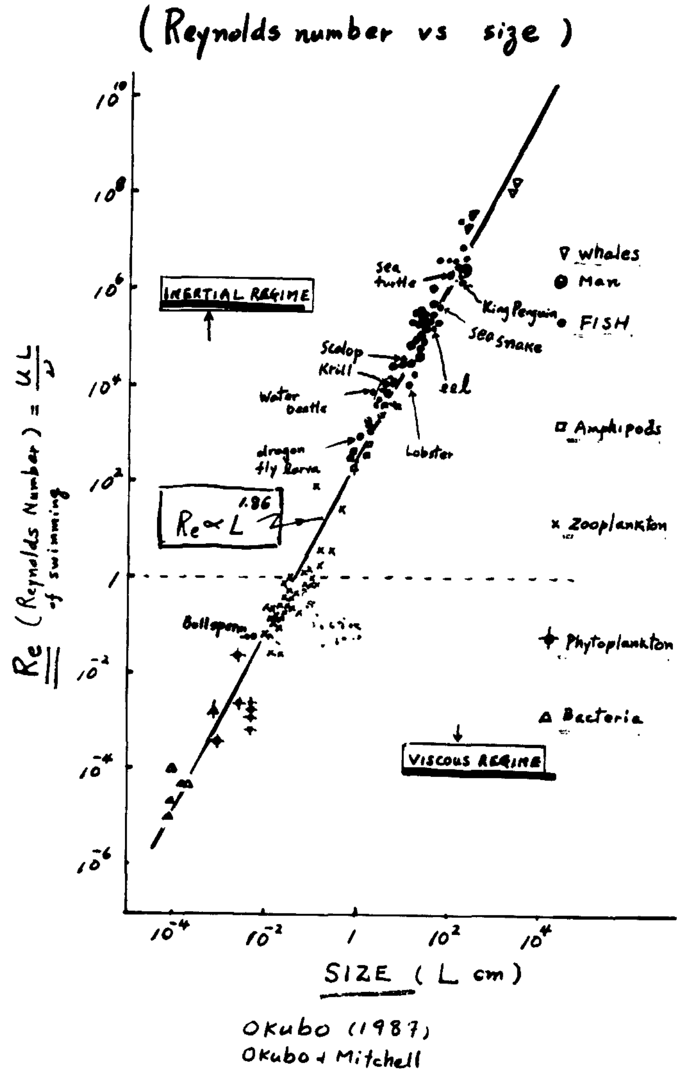

Figure 16. Reproduction of an overhead transparency drawn by Akira showing the relationship between body size and Reynolds number appropriate for swimming motions across taxa and kingdoms, from the small (bacteria) to large (whales) scale.

Excerpts from reviews of our grant proposal show how strong the appreciation was for this great man: "Okubo is internationally one of the top level scientists working in the area of dynamics of biological organisms and biological oceanography...Okubo is a well-established leader in the field of analysis of animal motion...Dr. Okubo is a long-standing expert in the topic of swarming behavior. Indeed, one could say without any hesitation that he is one of the founders of this whole fascinating line of research."

Following is the fragment of our NSF proposal that Akira Okubo had been working on when he passed away. Not only is it clear to the reader the love he had for teaching, but he obviously liked to work in a team, collaborating with experts in other topics. As disciples of this great mentor, we continue to strive to work together in the style of a man whose own style was inimitable.

Mathematical models for swarm maintenance (NSF proposal contribution written by Akira Okubo, funded from 1993 - 1997)

INSECT SWARMING

A central question of aggregative behavior is how a swarm is maintained despite apparent randomness in the motion of swarming animals so as to balance the tendency of spreading by random motion. Analyses of insect swarming behavior, recorded on movie film by Chiang (1968) addressed the objective of determining how long an insect (midge, ***Anarete prichardi*** *Kim) stays in a swarm.*

By tracking individual insects and analyzing their movements in several segments of the film, a number of statistical characteristics of swarming individuals were calculated, including the mean, variance, standard deviation, skewness, and kurtosis of the insect coordinates; insect number-density distribution in space, frequency distribution of insect velocities, velocity autocorrelations, and diffusivity associated with insect random motion (Okubo and Chiang, 1974).

The results were enlightening not only in the kinematical aspect of swarming but also in the biological interpretations of swarming behavior. The same basic data also were used to analyze the acceleration and force fields of midges in swarming. The existence of the deterministic attractive force was proven (Okubo et al., 1976). Further detailed behavior of midge swarming permitted the construction of a simple mathematical model for the dynamics of swarming (Chiang et al., 1978; Chiang et al., 1980; Goldsmith et al., 1980; Okubo et al., 1980; Okubo, 1986).

Hence, a swarm is maintained by the balance between deterministic and stochastic forces experienced by swarming individuals. Once a swarm forms, it remains intact for some time with little change in its spatial dimensions, constituting a "quasi-stationary aggregation." The stochastic force alone makes a swarm spread out to occupy a larger area or volume as time goes on where this tendency of spreading is simply a consequence of diffusion.

To adequately model swarming, therefore, it will be necessary to introduce certain regularities in motion superimposed upon the random motion. From the analogy to insect swarming, the nature of the regular, deterministic force is expected to be attractive toward the swarm center. Detailed tracking data of zooplankton swarming behavior will enable us to evaluate the force field as a function of distance from the swarm center.

Also the random component of the movement of the swarming animal may be estimated from the trajectory of the animal.

As a statistical measure of the swarm dimension, the variance of the animal displacement will be calculated by averaging the squares of the distance from the center over a sufficiently large number of individuals in a swarm. Kinematically the variance is related to the velocity autocorrelation of the animal motion. We predict that the velocity autocorrelation should oscillate about zero in such a manner that a long term average approaches zero. This behavior of the velocity autocorrelation characterizes swarming that is distinct from diffusion. The variance, i.e. the spatial size of swarm, can be estimated from the velocity autocorrelation. This variance model will be tested with experiments through the determination of the velocity autocorrelation of zooplankters in a swarm.

We also will consider a dynamical model for swarm maintenance. Newton's equation of motion will be applied to swarming animal motion. Four main forces are assumed.

1. *The frictional force or drag of zooplankton as they swim in water.*
2. *The deterministic force of an attractive nature which is working toward the center of swarm and depends not only upon the distance from the center but also upon the density of conspecific members.*
3. *The stochastic force due to randomness of zooplankton motion.*
4. *The gravitational force arising from a higher density of zooplankton body than the density of the surrounding water.*

An approximate analytical solution of the dynamical model equation may be obtained by the method of equivalent linearization (Bulsara et al., 1982). From the analytical solution we can calculate the velocity autocorrelation, the variance of animal displacements, speed frequency distribution of swarming animals, among other kinematical characteristics. These theoretical results will be compared with observed data.

Assuming the stochastic force is of white noise type, we can formulate a Fokker-Planck equation for the probability density function of velocity and displacement of the swarming animal. The solution of the Fokker-Planck equation enables us to obtain the theoretical distribution of velocity (and speed) and the spatial distribution of swarming animals. It is conceivable that the deterministic attractive force is a manifestation of density dependent advective velocity toward the swarm center. It is a built-in mechanism to maintain a sharp boundary of concentration despite a general tendency to spread by the random component of motion.

The net effect will produce a more or less uniform density in the central region of swarms. Thus, the zooplankton spatial distribution is predicted to be platykurtic. These theoretical predictions will be tested against data. If the comparison is favorable, we then will proceed solving the nonlinear model equation numerically to obtain more exact solutions.

This excerpt from the NSF proposal illustrates his thinking about the way mathematics can be used to describe the dynamic interactions between physics and biology in zooplankton behavior. Unfortunately, Akira did not live to complete this research project.

Akira Okubo was a man whose innate curiosity made him so great. Can you just see him sitting in the middle of a cornfield patiently waiting and watching insects as they swarm? One time, I asked Akira to help me: "Do you want to go and collect copepods with me?" Akira said, "Ooo, noo, no. I get seasick!" I replied, "Well, don't worry – we won't be going out on a boat!"

So I took Akira and our plankton net over to Stony Brook Harbor, near to both his home and the historic Three Village Inn where many Marine Sciences Research Center functions were held. It was a warm spring evening. He lowered the net into the water and pulled it along the sea wall. "Ehhh," Akira said, "this is like walking the dog." We walked and talked and watched the sun set with red clouds over Long Island Sound. It was so much fun working with Akira. I tried to show him everything we saw copepods do. I would show him the behavior, and he would respond with mathematical equations.

Teaching in the classroom with Akira was always inspiring. His logic was impressive. Even my old letters from him were often written in a different language, that of mathematics, depicted in symbolic logic. Starting in one dimension, he made a point, then extended it to the line of thought, adding depth with a knowledge that was fathomless.

He studied the two-dimensionality of swarming of the water skater, *Halobates* [T. Ikawa, this volume]. He solved the three dimensional spatial distribution of aerial insects by tracking two-dimensional shadows [Alan Elliott, this volume].

Teaching in the classroom with Akira was always inspiring. His logic was impressive.

When Akira first went into the hospital in 1995, I brought a videotape to entertain him and keep his mind on different thoughts: the tape was on mate-tracking by swarming copepods, where the male chases the female! That got him thinking... I met his colleague, Dr. Chiang, who was visiting Akira that day too. This new movie was so exciting to him. Akira could now fuse his keen interests in diffusion and swarming, and mathematically model the use of pheromonal trails for mate-seeking in pairs of copepods.

Figure 17. Jeannette Yen and Akira at Hana's Restaurant, Port Jefferson, Long Island, New York, celebrating Akira's 70th birthday, 5 February 1995. The fresh flower leis were brought from Hawaii by Jeannette to celebrate the occasion (photo by Jerry Liu).

And now - while he is wandering the Great Plains, his smile for us only a memory but his thoughts still so alive, we mortals struggle to fit nature to his equations because gone is the man who fit equations to nature.

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News Item: New Edition of Okubo's Classic Text to Appear in 2000

Simon A. Levin
Department of Ecology and Evolutionary Biology
Princeton University
Princeton, New Jersey, USA

Akira Okubo's elegant monograph, *Diffusion and Ecological Problems: Mathematical Models* (Springer-Verlag, 1980), is a classic of the literature. I take some pride in it, because it came about in part because of my inability to read Japanese. Akira had presented me with an earlier version of the work, but I could understand no more than the mathematical equations that were surrounded by Japanese prose. It was clear that I needed to know the contents of the book; hence, I took advantage of my role as Editor of the Springer Biomathematics Series to invite Akira to contribute a new edition, this time in English. As each chapter arrived on my desk for comments, it was clear that I had hit a gold mine. The rest is history.

Akira's book stimulated a tremendous amount of work in applying diffusion models to ecological problems. The subject has advanced a great deal in the nearly two decades since its publication. Hence, a few years ago, I went back to Akira and encouraged him to consider a new and updated version. Unfortunately, he did not live long enough to complete it, but he left copious notes about what he intended to do. His friend, Keiko Parker, sent me those notes, with Akira's wish that I see them through to publication.

Regrettably, Akira's symphony was unfinished, so it is left to his students to fill in the details as best they can. Springer-Verlag was eager to publish an update, and Akira's admirers and disciples have stepped forward. Each chapter will be revised by one or more of Akira's colleagues, updating the old material in the light of Akira's written blueprint and expanded as appropriate.

The volume promises to continue Akira's legacy, and will appear in early 2000. Editing the collection will be a labor of love for me, a last opportunity to honor Akira, who cannot be replaced.