My Career in Mathematical Biology
A Personal Journey

Jim Keener

Over the course of my career, I have had the pleasure of watching and participating in a minor way in the emergence of this major field of study, the field of Mathematical Biology. When I completed my PhD in 1972, I was a card-carrying applied mathematician, but knew nothing whatsoever about biology (except what little I had learned and then forgotten from my ninth grade biology class). One of the hot topics of the seventies was bifurcation theory, and in an effort to keep abreast of the latest developments, I read the paper by Li and Yorke, "Period Three Implies Chaos" (perhaps the most influential and improperly quoted paper of all time). As supplemental reading, I also read a paper by Otto Rossler, in which he made an off-hand comment about the reason that the heartbeat could be chaotic. Intrigued and curious about the validity of his claim, I decided I needed to learn something about cardiology, so picked up an introductory medical school textbook on cardiology by Guyton. What I discovered was a lot of low-hanging fruit, namely interesting dynamical behaviors that had received no attention from the applied mathematical community. I quickly became the leading mathematician studying cardiology, an easy matter when you are the ONLY mathematician studying cardiology.

I didn't really recognize that Math Biology was a legitimate field of study until I moved to the University of Utah in 1978 at the invitation of Frank Hoppensteadt. It was his vision to start a new group in Mathematical Biology, and shortly after I came, he brought Hans Othmer and then Aaron Fogelson, Philip Maini, and Simon Tavare. When Philip moved to Oxford a few years later, Mark Lewis came as his replacement. Being in a group of critical mass with similar interests turned out to be quite significant for me.

Meetings are an important part of a developing career, and in my case, there were several that were memorable. Perhaps the most memorable was a 4-day backpacking workshop that Paul Fife and I organized in the summer of 1981 in Salt Creek Canyon in the Canyonlands of southern Utah. Art Winfree, John Tyson, Leon Glass, John Rinzel, Jim Cushing, and Joe Keller gave informal talks using a rollup white board on an easel constructed of dead tree limbs (no laptops!), in the shade of a large sandstone amphitheatre. Talks were in the morning and evening. Among the most memorable features of the meeting, aside from talking science under the stars until late at night, were the 100+ degree afternoons, the flies, and the ten mile hike out to the vehicles.

A second backpacking meeting two years later (Lee Segel came along this time) was made memorable by torrential downpour and muddy roads that made driving quite treacherous.

Gordon Conferences were also as important to me as they were formative. Sitting in the lawn at the Tilton School in animated discussions with Art Winfree, Leon Glass, and Steve Strogatz is unforgettable. The first Gordon Conference I attended I played basketball in my hiking boots. I didn't mind, but Bard Ermentrout still complains about his bruised shins from my tenacious rebounding efforts.

My collaboration with John Tyson started in the Fall of 1984. Jim Murray had just started the Centre for Mathematical Biology at Oxford and invited George Oster, Art Winfree, John Tyson, Gary Odell, and John Rinzel to spend the Fall there. Afternoon tea was memorable as we scribbled on the erasable table tops in the lounge of the Maths Institute. It was there that John Tyson and I started talking about spiral waves at the pub next door to the Institute, the same pub that was frequented by C.S. Lewis and J. R. R. Tolkien. Clearly, their work has had a much greater impact than ours, but it was fun sitting in the seats of giants, pretending. My collaboration with John Tyson had its zenith in 1986 during walks, visiting pubs around Oberwolfach, at a meeting organized by Willi Jaeger.

The work of Soviet scientists at Puschino has also had a major impact on me. It became personal in 1989 when I met a group of them, including Val Krinski, Sasha Panfilov and Arkady Pertsov at a meeting in Leeds. It was the first time they were allowed to travel to the West, although they were accompanied by not-so-inconspicuous non-scientific escorts. The following year Sasha was able to come to Utah, where he stayed for three years. As you probably know, Sasha can make a computer sing, and at Utah he did simulations of cardiac dynamics that I found wonderfully inspirational. I use some of these simulations in talks that I give to this day.

My career took its next big step when one day in 1994 I received an email from an unusual fellow named James Sneyd. It was an inquiry to see if I was interested in collaborating on a book. It seems that James had exhausted his list of famous potential collaborators, so had resorted to a mass emailing, an early example of a spam. He did not suggest that my share for signing on would be in the hundreds of millions of dollars following the death of his wealthy uncle, but only that he was willing to work hard (which, by the way, he did!). Not noticing that the email came from Nigeria (actually it came from UCLA), I agreed to participate in the project. Anyway, since I knew so little about physiology, I thought it would be a good way to learn the subject. The result, four years later, is something that I am quite proud of and am happy to take credit for. James did a fantastic job!

Other highlights of my career were more spread out. These are the many students that I have had the privilege and pleasure of mentoring. They taught me much more than I could ever teach them. But if you ever run into one of them be sure to ask them about the Hodgkin-Huxley Macarena.

I have had the opportunity to study and learn about many areas of biology and have seen how mathematics can help us understand some of the intriguing mysteries of life. The world of biology is incredibly fascinating, and I have only been able to scratch a tiny part of its surface. I am grateful to all you wonderful taxpayers for allowing me to put your tax dollars to work; it's much better than leaning on a broom. More than that, though, it's the people, my students, collaborators, and colleagues who have made the story what it is. And then there is this mystery that there is a mystery to be explored and the Source of that mystery, to Whom all the credit truly belongs.

Selected Publications:

J. P. Keener, Principles of Applied Mathematics; Transformation and Approximation, 2nd edition, Perseus Books, 1999.

J. P. Keener and J. Sneyd, Mathematical Physiology, 2nd edition, Springer, 2008.

J. P. Keener, A model for length control of flagellar hooks of Salmonella typhimurium, J. Theoretical Biology, 234, 263-275 (2005).

B. E. Peercy and J. P. Keener, Coupled cell model of border zone arrhythmias, SIAM J. Appl. Dyn. Sys, 4, 679-710 (2005).

N. Cogan and J. P. Keener, Channel formation in gels, SIAM J. Appl. Math, 65, 1839-1854 (2005).

J.P. Keener, Stochastic Calcium Oscillations, Math. Med. Biol. 23, 1-25 (2006).

J. P. Keener, How Salmonella Typhimurium measure the length of their flagellar filaments, Bull. Math. Biol. 68:1761-1778 (2006).

R. D. Guy, A. Fogelson, and J. P. Keener, Fibrin growth in a shear flow, Mathematical Medicine and Biology, 24, 111-130 (2007).

L. Copene and J. P. Keener, Ephaptic coupling of cardiac cells through the junctional electrical potential, J Math Biol, 57, (2008).

J. P. Keener, Invariant Manifold Reductions for Markovian Ion Channel Dynamics, J. Math. Biol., 2008