

SMB - CHILDHOOD, BOYHOOD, YOUTH

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At the meeting of the Society for Mathematical Biology, held in Mexico in 1995, Leah Edelstein-Keshet, suggested that I prepare a history of the Society for the *Newsletter*. It is natural, since the Society comes of age this year, its first post-charter meeting having been held in Bowling Green in 1975.

Leo Tolstoy, at some point in his life, seems also to have been impelled to rummage about in his memories. The title of the resulting trilogy, *Childhood, Boyhood, Youth*, aptly captures the main stages of social development of the SMB.

Actually the Society was born somewhat earlier than the meeting in Bowling Green, having been founded shortly before by a triumvirate consisting of George Karreman (first president), Herbert Landahl (second president), and Anthony Bartholomay. The founding was connected to the management of the *Bulletin of Mathematical Biology*. The journal, originally called the *Bulletin of Mathematical Biophysics*, had been initiated much earlier by Nicholas Rashevsky. When Rashevsky died, Mrs. Rashevsky transferred ownership to Landahl. A group of charter members were brought together by the founding triumvirate, and as soon as a charter was established Landahl transferred the *Bulletin* to the Society.

I think it fair to say that the childhood stage, the period of somewhat wide-eyed exploration, was consummated under Karreman and Landahl. John Stephenson and John Jacquez, the next two Presidents, brought the Society into its boyhood period, though perhaps it would be more acceptable and even more descriptive to use the term adolescence. Tolstoy described how, somewhere between this period of boyhood and youth, the protagonist daydreamed that for some reason everyone in society had a great respect and love for him. This was indeed the adolescence of the SMB. Obtaining professional respect for mathematical biology was the order of the day.

There are some reasons for this, which are important for understanding the dynamics of our field during this period, but which are still, well, shall we say, enveloped in a penumbra of delicacy. The main fact is that mathematical biology was not in good repute, and theoretical biology was in the doghouse as well. Of course, one can look back and say that Danielli had founded the *Journal of Theoretical Biology* prior to this period, and that Waddington's *Towards a Theoretical Biology* meetings were having effect. The success of these enterprises, I believe, was not just due to their high quality, but as well to the existence of a lacuna concomitant to the doghouse atmosphere.

This atmosphere was the natural aftermath of the truly great advances in molecular biology initiated in the fifties by the discovery of the significance of DNA. Though the argument might be made, retrospectively, that this discovery was infused with theoretical analysis and synthesis, the inexorable spin at the time was simple: a generation of theoretical "speculation" was being sent to the graveyard in body bags.

How does this tangle with the history of the SMB? At the outset I have to say that I never met Rashevsky. But it is clear that he was the name most closely identified with mathematical biology, because of the quantity of work, because of the school and journal he had founded, and because of definitive achievements. The latter included the first mathematical study of neural nets, early mathematical models of pattern formation, and mathematical analyses of cell fission that were rapidly absorbed by the U.S. military nuclear program. Overarching this was a spirit of relationalism that was highly disconnected from molecules and from the material substratum of life generally, despite the then claim of the *Bulletin* to be a venue for papers on the physico-mathematical foundations of biology. It was an approach that abstracted the "sociological" aspects of biological organization at a time when the molecular-material aspects had come to the fore. Suffice it to say that Rashevsky had become the Trotsky of mathematical biology. Landahl's wish for the Society to eventually encompass all mathematical and theoretical biologists could not come to fruition given the resulting polarization in the mathematical / theoretical biology community. The reins of leadership would have to move to the outside if this were to be preserved as a goal, and they did; but with the almost foreseeable consequence that other alienations inimical to the goal would ensue. (Unfortunately I don't see how any useful history of the Society can be written without acknowledging this point.)

Actually, the efforts to attain professional status began during the Karreman presidency, when Stephenson succeeded in placing the annual meeting in the framework of FASEB (Federation of American Societies of Experimental Biology), under the auspices of the American Physiological Society. Several subsequent meetings were held in this context, and later under auspices of the American Institute of Biological Sciences and the Society for Industrial and Applied Mathematics. By the time Stephenson had completed his presidency, the Society had accumulated quite a nest egg. This allowed for a curious annual meeting experiment in 1986, effectively during the presidency of John Jacquez. The meeting that year was held in conjunction with a Symposium on Pattern Recognition in Natural and Artificial Systems, hosted by the Center for Adaptive Systems at Boston University. It was just at the moment when neural networks were emerging from a long dormancy. The SMB contribution was highly appropriate, given the seminal work in the field that appeared in the *Bulletin* (in particular the work of McCulloch and Pitts in 1943).

Simon Levin's presidency (circa 1988) was a takeoff period, in terms of infrastructure development and growth in

membership, including new members from the ecological and applied mathematics communities. The attempt to transcend the physiological identification that had developed was a somewhat deliberate sentiment at the time. My analogies are getting rough around the edges, but let us call this the late boyhood period. (Some might call infrastructure an early middle age sort of thing, but it is critical in the early life of a society.)

At about this time it was becoming clear that the Society had achieved its goal of professional recognition and had made some progress in building up sources of funding for work in mathematical biology. Of course, this was not entirely a matter of the efforts of the Society. Many of the problems coming to the fore in biology were calling for mathematical and computational methods: complex ecological systems, neural models, the new developments in gene sequencing, and so forth. But the Society was there and had been working like a mole on the milieu, which was now far different than it had been a decade earlier.

This success had its antithesis, though. The joint meetings that had contributed so much to forging links with the broader biological community had subjected the Society to centrifugal forces that were inimical to its own identity and to the type of community interchange which is so crucial to the advance of a science. Even at the Boston meeting, which was more limited in scope, the activities of the Society were rather a sideshow.

The sense took hold that it was time to move back to a stand alone meeting format, like that of Bowling Green. Stuart Kauffman, who took over the presidential reins in 1989, recognized this need and accordingly moved to organize the 1991 annual meeting in Santa Fe. It had positive effect, and led to the policy of holding stand alone meetings at least once in every two years. I like to think of this as marking the inception of the youth stage of the Society, taken in the sense of its being a stage of greater self-confidence.

The presidencies of Alan Perelson and John Tyson are too contemporary to be included in a history. But I think it is noteworthy that the recent stand alone meeting in Mexico, initiated and organized during Perelson's and Tyson's terms, and hosted by Francisco Lara Ochoa, was the best attended meeting held by the Society, and the most international.

This international theme has a long history in the Society, running back to Landahl's term. It is important to make a few comments on this, since it dovetails with the development of the Society and remains a pertinent issue. Landahl asked the present chronicler to chair an ad hoc committee for international cooperation. We made a worldwide survey, in the 1981-82 period, to assess interest in either international expansion or affiliation with other societies. The response from groups in over twenty countries was strikingly enthusiastic. The main difficulty was that the SMB was and is by itself international in its membership; some mechanism was required to cooperate with existing groups in other countries while at the same time maintaining our own

international profile. The Board passed a resolution authorizing the President to negotiate agreements with existing international groups and also to develop a chapter structure. But when the interests of the Society turned inward, to obtaining professional recognition from the established U.S. institutions, the desire to use this authorization disappeared. Levin and Kauffman, during their presidencies, made an attempt to revive the international agenda relative to Europe, but the receptivity had changed, probably in part because of the then emerging sense of European unity but possibly also because of the inward focus of the SMB during the previous period. New groups and new fora are now proliferating, reflecting new ideas and methodologies, and new regional networks. The relations between SMB and this world community—indeed the place of SMB in this larger community—is an issue that is bound to come up again and again.

No history of the SMB can be complete without noting the long term service of its Treasurer, Torcom Chorbajian. I, and undoubtedly all other past or present activists, can testify to the fact that Torcom has been the constant presence that has held the Society together day to day from its inception.

What about the future? Well, this I am afraid is a brief and of necessity selective social history, not a futurology. Undoubtedly there are many new young scientific personalities out there who would take exception to my belief that the Society is in its youth; they may think it is well into its middle age or even aged. I hope that they think this, for if so they are in the right frame of mind to keep the Society young.

Further articles about the history of SMB:

Our photo collection of past presidents and SMB officers is growing, but still incomplete. We hope to publish a complete set in an upcoming issue. Missing are some of our earliest presidents, including Landahl, Karreman, Bartholomay, Stephenson, (and Kauffman). If you own a good, well-focused B&W or color photo of these individuals, or of other events of historical interest, please lend them to us ! They will be treated with care and returned.

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Society Mail Bag

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