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## Towards A Theoretical Biology: Reminiscences

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In 1967 I had decided to broaden my activities beyond the theoretical physics I had been engaged in since the start of my career at the University of Chicago. Theoretical biology was among the possibilities I was considering, though I then had no deep understanding of what it was and what it might become. Jack Cowan, a physicist turned mathematical biologist, had recently arrived at the University to Chair the pioneering but then moribund Committee on Mathematical Biology. He urged me to join him in revitalizing the Committee and transforming it, ultimately, into a Department of Theoretical Biology. To attract me towards theoretical biology, he arranged for me to be invited to several conferences which influenced my decision to join him in the Committee and to become active in theoretical biology as well as in physics. The most influential was the third of four conferences entitled ‘Towards a Theoretical Biology’ organized by C H Waddington at the behest of the International Union of Biological Societies and held annually during the summers of 1965–1968 at the Villa Serbelloni in Bellagio on Lake Como in Italy. I attended the fourth as well, after I had joined the Committee and had started research on theoretical problems in biology.

Waddington, an important developmental biologist cum evolutionary geneticist, had long held that creating a stronger, encompassing theoretical framework for biology was essential to progress, lamenting the absence of a framework like that in physics. With clear ideas of what such a framework should entail – biological complexity, a more realistic view of the role of genes, and the centrality of evolution together with development – he was a natural choice to organize a series of conferences under the title ‘Towards A Theoretical Biology’, conferences aimed at stimulating the development of such a theoretical biology. The role the conferences and their published proceedings had in moving us towards that theoretical biology is discussed in Vidyanand Nanjundiah’s article in this issue of *Resonance*. In this note, I recall my memories of the conferences through the haze of four decades and tell of the impact the conferences had on me.

The Villa Serbelloni, owned and maintained at that time by the Rockefeller Foundation as a venue for conferences, is now a luxury hotel. It was a private estate then with a historical connection to Pliny the Younger. Occupying with its gardens most of a peninsula projecting into Lake Como which it shared with the small town of Bellagio, the Villa was an idyllic site for the conferences. Normally reserved by the Rockefeller Foundation for the humanities, Waddington somehow managed to procure it for his conferences. The conferees were housed, fed, and convened in the main building, which was elegant without ostentation. The small but



comfortable bedrooms, the excellent food, the beautiful setting, the walkways over the extensive grounds all together inspired a sense of well being in the conference participants sought for but not always achieved.

Waddington gathered an extraordinary collection of individuals drawn from all areas of biology and from the physical sciences, mathematics, computation, philosophy, and linguistics [1]. Only some had done theoretical work in biology, but all had contributed significantly to their own fields in ways which were relevant to the topic of the conference. The groups were small, averaging 19 over the four years.

In the two years I attended, we gathered in a small room wider than deep, with chairs before a blackboard and pacing room for the speaker between. A magnificent view of the lake and the mountains was largely ignored. The informal, relaxed sessions were held in the morning and late afternoon. Waddington set up a schedule of relatively coherent sessions and chaired them, but did not seem to me to be a strong presence. Most speakers talked about specific contributions they had made to their particular fields of interest. The discussion of each talk largely concerned its content. I remember few attempts to bridge or transcend the separate branches of biology. Of course, at the time I was greatly hampered in recognizing such attempts by my lack of detailed knowledge of any particular field of biology, especially during the first one I attended. In retrospect, however, significant themes did emerge: the importance of dealing theoretically with the complexity of biology and the importance for that of appropriate theoretical analysis of the networks of interacting entities which arise at each level of biological organization. The conferences were also significant for the development of complexity theory more generally.

One can debate whether the conferences and the proceedings moved science significantly ‘towards a theoretical biology’. Nevertheless I think they were valuable whatever the resolution of that debate. I found the individual talks and discussion very stimulating and the intellectual atmosphere at times electrifying. At the time of the 1967 conference, I had not yet defined a focal area for research attractive enough to trigger my commitment to research in biology. During that conference, Lewis Wolpert presented his notion of positional information in developing organisms [2], and Brian Goodwin described his phase-shift model for the control of development [3], an explicit realization of Wolpert’s notion. I found both talks thought-provoking and proposed to Goodwin that together we could augment his ideas along certain lines that had just come to my mind. The abstract of the paper resulting from the ensuing collaboration [4] was reprinted in the proceedings of the 1967 conference [5].



Thus my attendance at the 1967 conference led to my initiating research in theoretical biology, focused my attention on the control of development, and resulted in my joining Jack Cowan in the Committee on Mathematical Biology at the University of Chicago. Over the next six years I worked with him to strengthen the Committee and transform it into a Department of Theoretical Biology and merge that with the Department of Biophysics. With Anthony Robertson I created a joint experimental and theoretical group on the control of development with the cellular slime molds as the model organism. Nine graduate students completed their PhD research within that group under my or our supervision, carrying out experiments on the cellular slime molds and interpreting them via theoretical analyses very much in the style of theoretical physics. Among them was V Nanjundiah who was a graduate student in physics. Thus while I cannot testify authoritatively as to the importance of the conferences for the development of theoretical biology generally, I can assert that they had a major impact on the course of my career. I suspect that I may not have been the only one significantly affected by the conferences.

### Suggested Reading

- [1] <http://www.visual-chaos.org/complexity/background/serbelloni.html><http://www.visual-chaos.org/complexity/background/serbelloni.html>
- [2] L Wolpert, *J. Theoret. Biol.*, Vol.25, No.1, 1969.
- [3] B C Goodwin, *Symp. Soc. Gen. Microbiol.* Vol.19, p.223, 1969.
- [4] B C Goodwin and M H Cohen, *J. Theoret. Biol.*, Vol.25, No.49, 1969.
- [5] B C Goodwin and M H Cohen, in *Towards a Theoretical Biology 3, Drafts*, p.231, Edited by C H Waddington, Edinburgh University Press, Edinburgh, 1970.

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