An Interview with John Maynard Smith[1]

John Maynard Smith FRS (1920-2004) was born in London. He was educated at Eton and Cambridge, where he studied aeronautical engineering. After the Second World War, during which he worked on military aircraft design, he changed career direction and studied fruit fly genetics under J.B.S. Haldane at University College London. In 1965 he became the founding Dean of Biological Sciences at the University Of Sussex, where he stayed for the rest of his career. He was one of the great evolutionary biologists, making many important contributions, including the application of game theory to understanding evolutionary strategies, and a clear definition of the major transitions in the history of life. He won numerous awards and honours, including the highly prestigious Crafoord Prize in 1999 and the Kyoto Prize in 2001.

This is an edited transcript of an interview conducted on the 21st May 2003 in John Maynard Smith's office in the John Maynard Smith (Life Sciences) Building, University of Sussex. The discussion centred on John's interactions with people involved in Cybernetics and early AI.

John Maynard Smith: Shall I tell you about my meeting with Turing?

Phil Husbands: Please.

JMS: It was when I was a graduate student of Haldane at UCL; very soon after I started, so we're talking about 1952, and I was counting fruit flies. But one of the other things I had been doing, inevitable given my past in aeronautical engineering, was to think about animal flight. And I wrote various papers on that; I was thinking particularly about stability and control of animal flight. I was influenced by John Pringle's work of course; he did this very very beautiful empirical work showing that the halteres of the fruitfly, or indeed of any fly – all flies have halteres - are involved in control of the horizontal plane and the yaw[1,2]. Anyway, Haldane came into the lab, where I was sitting counting flies, with this rather nice looking dark small chap and said "Maynard Smith!" — no, "Smith!", he never got round to calling me Maynard Smith — "This is Dr…", and I didn't catch the name, "He would be interested in what you have been doing recently on flight." And I remember thinking, 'Oh God, not another of these biologists who doesn't know a force from an amoeba; I'm going to have to go very very slowly'.

So I started explaining to him some stuff I'd been doing on instability. I was at the time interested in the fact that primitive flying animals had long tails — you know, dinosaurs such as *Archaeopteryx*. This had always been explained away as just an evolutionary hangover: they had long tails when they were on the ground and hadn't had time to get rid of them. And that is part of the truth obviously, but it occurred to me that the more interesting truth was that they actually needed them for stability. I proposed that it was only after their nervous system evolved a bit to control flight

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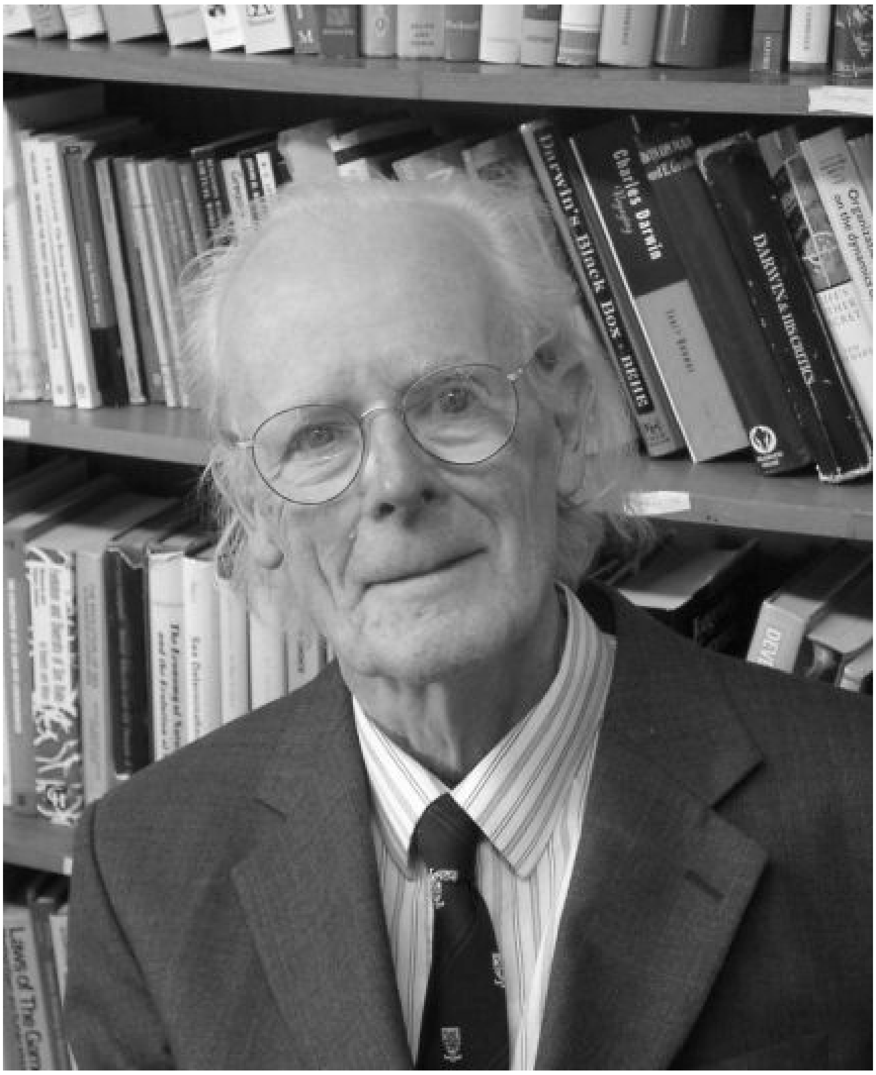
more, that they were able to fly with short tails. In fact my first published paper was called *'The importance of the nervous system in the evolution of animal flight'*[3], and it discusses this problem with a lot of criticism of previous claims, and basically I think I still believe it. Anyway, I started explaining this to this poor buffoon with some diagrams. He listened patiently without saying anything, then he held out his hand for my pen and changed the direction of one of my arrows. And when I looked at it he was obviously right; I'd made a mistake in a force diagram. I thought 'Oh Shit', because I'd really been talking to him like a two-year-old, and so I said "Look I'm so sorry but I didn't actually catch your name." And he said "Well my name is Turing." 'Oh Shit!' I thought again!

Anyway I have this other interest in Turing. As you know, he wrote this very remarkable paper on the chemical basis of morphogenesis describing a reaction-diffusion based model[4].



PH: Yes, that paper must have just come out, or was about to be published.

JMS: Yes, it'd come out just before. So we talked about that for quite a while, several hours. We talked about what kinds of observations might be made in the field in connection with the theory, and I've been interested in reaction diffusion systems ever since. When I came down here (Sussex University) it was one of the topics that I hoped we'd investigate – the relationship between chemical gradients and development and so on. And so that is why I invited Brian Goodwin to come here. It was really his interest in morphogenesis and development that led to the invitation. Anyway, I didn't get to know Turing as I only met him on this one occasion.

PH: So there wasn't really a scientific interaction, but some influence?

JMS: Yes, through his morphogenesis work he had a lasting influence on me and what I thought was important in biology. But there was no real scientific interaction. Various young people like myself were influenced by his ideas and followed them up later, and the ideas are still very much in currency. But at the time we were just postgraduates. I don't remember the idea being discussed at the time at mainstream biology meetings. Embryology was a *very* empirical science, a very non-mathematical branch of biology, back then. I don't think it had much impact at the time because of that.

Now Pringle I interacted with a bit more because he'd done this work on flight. But he was already a rather senior figure. He wasn't a lot older than me but he'd started younger and was already an established figure and I was just a graduate student. We talked about the control of flight and stability. It's interesting to learn he was a member of the Ratio Club, I hadn't realised.

PH: Yes, he was one of the founding members and it was he who suggested Turing, who he knew quite well, should become a member as they needed some mathematicians to keep the biologists in order.

JMS: Yes, I can well understand that! Of course most people who were doing theoretical biology at that time had worked on radar during the war and had worked with engineers and mathematicians and so appreciated what they could contribute.

PH: Indeed. Pringle and many of the other biologists involved in the Ratio Club seemed to have an inclination towards theoretical work before the war, but it was greatly strengthened during the war due to deeper exposure to and involvement with engineering. Pringle, for instance, worked on airborne radar development; in fact he was in charge of it for a while. This kind of wartime work seemed to profoundly influence the subsequent careers of quite a few biologists. But you were very close to the whole thing of course, does that seem right to you?

JMS: Oh I'm sure of it. Absolutely sure. Of course I came into biology from engineering, but on the other hand not from electrical engineering or control theory.

PH: You had studied aeronautical and mechanical engineering?

JMS: I was basically a mechanical engineer. Though, curiously enough, during the war it had occurred to me that, at least in principle, if an automatic pilot was sensitive enough and quick enough it would be able to control an unstable aircraft, whereas a pilot couldn't. Things would happen too quickly for a human; they would be dead before they'd learnt. You see there were certain real advantages, aerodynamically, in having an unstable aircraft. It wasn't just that such an aircraft could manoeuvre quicker, but also landing speeds could be increased and things of that kind. But it also became clear to me very quickly that at that time electrical control was simply not fast enough. But the idea of automatic pilots and control of instability were in my mind and so when I started thinking about insect flight, after the war, it came back to the fore.

PH: Obviously you became aware of cybernetics, but how underground or mainstream was it, as far you can remember, in the late 1940s, early 1950s.

JMS: Well, you know, I don't think I was explicitly aware of cybernetics until later. That early, I think only a small number of scientists were involved. So not mainstream.

In fact I remember being rather annoyed when I read about cybernetics a few years later. One of the problems we had in aircraft design was to predict, before the structure of the aircraft was built, what its natural modes of vibration would be. How was it possible to find out? Now — I'm rather proud of this actually — it occurred to me that you could build an electrical analogue of any mechanical systems if you know what the masses and stiffnesses and so on were. So we could build an electrical analogue of the structure of the aircraft that oscillated and get its fundamental modes from that. And we did! Anyway, it was rather useful at the early design stage. Of course I wasn't the only person it occurred to, mind you, or the first, but at the time we hadn't come across the idea.

So this was actually used. It was rather exciting, because what you then did was to build the aeroplane and discover what its actual modes of vibration were and if they agreed. By the way, we did the actual measurements using a variable speed electric motor that drove a wheel and you could shake the thing at any frequency you liked. You bolted this to the frame and you gradually speeded it up until you got the whole structure singing, very dramatic. Anyway, this was the kind of way that people with a little bit of mathematics in aircraft were thinking . Then some years later, after the war, I read Ashby's *Introduction to Cybernetics* [5] – an interesting book - and in it he describes electrical analogue computers. And I though 'Christ, I've been going along all this time without knowing what I'd done.' And it was rather annoying. (chuckles)

Anyway, that aircraft modelling work is typical of the kind of thinking and problem solving that was in the air. There weren't, on the other hand, too many of us with the necessary technical skills, and of course we brought some of what we learned into our work after the war.

PH: Yes, that's very interesting, and presumably this need to be imaginative, as well as the mixing of biologists and engineers and mathematicians, played an important part in developing theoretical biology, or at least pushing more theoretical thinking into biology?

JMS: Yes, I think that's right. Some biologists became more theoretical following their war work, but there was an effect on mathematicians too. That happened because people with mathematics, of which Turing is an example, had been drafted into all kinds of technical work during the war, applying their mathematics. In Turing's case, of course, this was mainly decoding work, and for others it was radar research and development. So they became used to thinking about practical problems and at the end of the war they had this interest in applying their mathematics to the real world, and biology was one of the obvious places to do it.

But now when it comes to theoretical biology I'm quite intrigued. Earlier I'd say it was mainly population dynamics. The first burst of theoretical biology was from those two guys Volterra and Lotka, making models of population growth. That would have been in the thirties or a little before. And the second burst was Fisher, Wright and Haldane's work on population genetics, and that was actually very important in biology at that time. That was also in the 1930s, and prior to that there was really a complete dichotomy between the Darwinists and the Mendelians. The Mendelians thought that evolution happened when a mutation occurred and the Darwinists were doubtful about Mendel and thought it was all a matter of selection. And the extraordinary thing is, certainly looking at it now, that these views were regarded as incompatible. And indeed Haldane, Wright and Fisher showed that actually they were completely compatible. That's a very important example of how theory and mathematical thinking can really advance biology. Now of course all this happened when I was a schoolboy, before the war; but the amount of theoretical work increased after the war. Of course there were parallel developments in neurobiology, but I think they were probably very largely independent

PH: Yes, I think that's right. Interestingly, many of those involved in the rich interaction between cybernetics and neurobiology had worked on radar in the war.

JMS: Yes, right. Now I never had any contact with the radar people during the war which is probably why I didn't get very involved in these things later. Eventually I saw myself as an evolutionary biologist working in population genetics.

The people who I had contact with, and who had influence, and I think were involved with some of the people in the Ratio Club, were Waddington and Needham.

PH: Waddington was certainly involved a little in the British cybernetics scene. For instance, he gave a talk at the Ratio Club on development as a cybernetic process.

JMS: Waddington, who was an interesting man, didn't actually use his mathematics at all. I knew him fairly well, from those curious meetings he used to run on theoretical biology (the *Towards a Theoretical Biology* series[6]). He encouraged young mathematically inclined biologists and by bringing us together he helped by making us feel less like loners. He was interested in relating development to evolution, and he liked ideas. But the point of mathematics is to use it. What good is it unless you are doing something that couldn't be done without it? I don't think that ever filtered through to him. Needham I hardly knew. I met him as an undergraduate and he was an awfully formidable figure. But again it's not quite clear to me what he actually did. I think there are these great grey eminences who people don't understand so they think their work must be very important indeed. He was rather like that.

Now what about Donald Michie? We worked in the same lab for years and years when he was a geneticist, and then he became very involved in Artificial Intelligence, as you know. Of course he was a close colleague of Turing during the war. He told me many entertaining tales of those times. Turing's Gold, for instance. According to Donald, in nineteen thirty-whatever, when it looked as if the Germans were going to invade, Turing decided that what he was going to turn all his money into gold. And so he did and he buried it in the corner of a field somewhere. Donald didn't know about this at the time, but he became involved much later, in the war, when it was fairly clear the Germans were not going to invade, and Turing decided he was going to dig it up. Donald has this dramatic story about how they built a home-made metal detector and spent their weekends tearing around the Home Counties looking for this bloody gold. As far as I know they never found it. He wouldn't tell me what area it was in! But it gives you an image of Turing. Great story.

When we were all at UCL, Michie and his then wife, Anne McLaren, who is a very distinguished, but not theoretical, biologist, were working on perpendicular fertilization. One of the happiest evenings of my life was spent with these two in a pub after they had first managed to take an egg out of a mouse, fertilize it, pop it back into the same mouse and get a baby mouse! Now to do something for the first time is bloody hard. So they were basically experimental embryologists. That work couldn't have been done without Anne, who is someone for whom I have an immense admiration.

I don't know how Donald got into Bletchley. He's an extremely bright guy, no question about it, but not formally mathematical, not back then. I think he was a classics scholar at that stage. But right from the early days he was interested in artificial intelligence. Donald and I played one of the very first games between two chess computers. You see we both had an interest in inventing rules to govern games and processes. During the war we had each produced a set of rules, an algorithm, to play chess. If you carefully carried out the calculations, which you could do by hand, the rules specified what your next move should be. And mine was called SOMA, for Smith's One-Move Analyzer; it didn't look at all deep. His was called MACHIAVELLI, for reasons I'm not quite clear about. MACHI because it's like Michie, and his collaborator was someone whose name ended in VELLI, or something like that, and the obvious reference to Machiavelli I suppose! Anyway, we spent a long weekend playing these two sets of rules against each other with my older son as referee[7], because neither of us trusted the other one! You know, because if the obvious move was pawn to king, it had to be the rules that made the moves, not the humans. It was even published[8].

PH: How much do you think science had changed from those heady post-war days when there seemed to be a tremendous energy and an enthusiasm for innovation?

JMS: Well obviously the particular part of science I work in has been dramatically transformed by technical advances, many involving computers, so that it is easier as well as cheaper and cheaper to obtain data. So we get submerged in data these days.

PH: What about the way science operates? Maybe this is purely illusionary, but it seems to me there might have been a bit more freethinking around at that period. Do you think people were less hemmed in by discipline boundaries or very specific kinds of methodologies?

JMS: I'm not sure. There are plenty of young people today who seem to me to be very capable and imaginative and able to tackle these sorts of problems and are not too constrained; and one way or another manage to get the job done and the message out, with some fairly way-out topics and speculative research. However, there is much more money and that brings red tape. But money, and the need to get it, takes over more and more today and that is a great pity. Our relationship to the funding has changed a great deal; we hardly had to think about money at all then. We weren't constantly brooding about how to keep research funding up. So in that sense we were freer to get on with the science.

Notes and References

[1] Fraenkel,G. and Pringle, J.W.S. (1938) Halteres of flies as gyroscopic organs of equilibrium. *Nature,* **141**:919–921.

[2] Pringle, J. W. S. (1948). The gyroscopic mechanism of the halteres of Diptera. *Phil. Trans. R. Soc. Lond. B* **233**,347 -384.

[3] Maynard Smith, J. The importance of the nervous system in the evolution of animal flight. *Evolution* **6**, 127-129, 1952.

[4] Turing, A.M. The chemical basis of morphogenesis, *Phil. Trans. R. Soc. London B.* **237**, 37-72, 1952.

[5] W. Ross Ashby. *An Introduction to Cybernetics*, Chapman & Hall, London, 1956.

[6] C.H. Waddibngton (Ed). *Towards a Theoretical Biology, vol. 1: Prolegomena*, Edinburgh University Press, 1968. [Several volumes in this series were produced]

[7] Said referee, Tony Maynard Smith, recalls that his umpiring may have been less than perfect as hand calculation was too slow and boring for a teenager to put up with!

[8] An article on the match between these chess machines appeared in the popular science magazine New Scientist: J. Maynard Smith and D. Michie, Machines that play games, *New Scientist* **No. 260:** 367-9, 9th November, 1961. The article records the result of the match thus: "Move 29: draw agreed. NOTE: Combatants exhausted; in any case, neither machine is programmed to play a sensible end game."

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