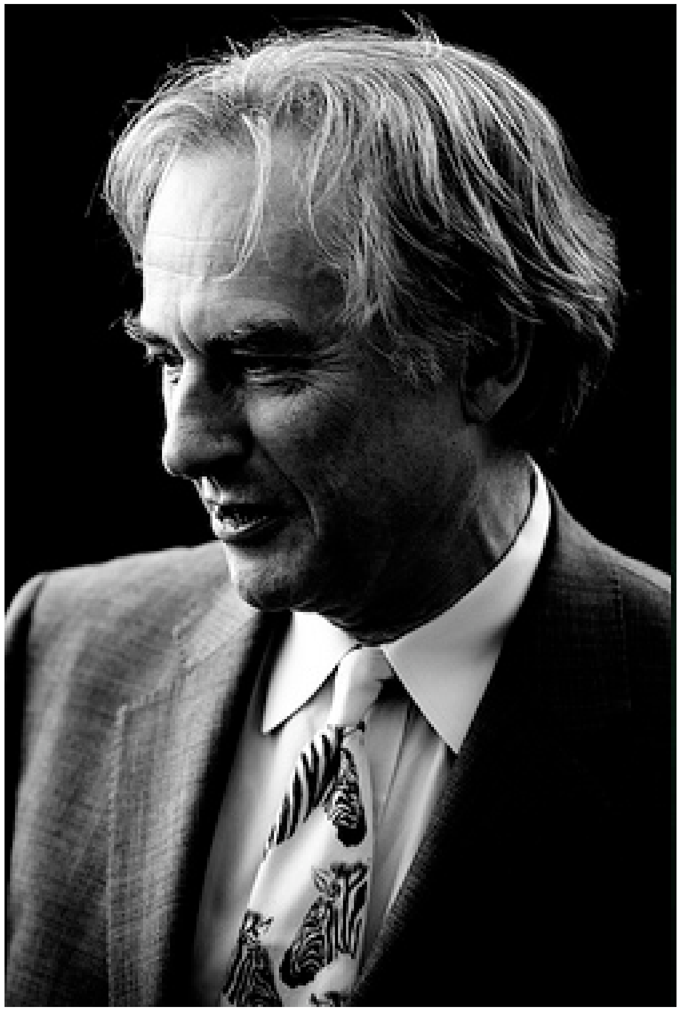
GROWING UP IN ETHOLOGY[1]

Richard Dawkins



Childhood and School

I should have been a child naturalist. I had every advantage: not only the perfect early environment of tropical Africa but what should have been the perfect genes to slot into it. For generations, sun-browned Dawkins legs have been striding in khaki shorts through the jungles of Empire. My Dawkins grandfather employed elephant lumberjacks in the teak forests of Burma. My father's maternal uncle, chief Conservator of Forests in Nepal, and his wife, author of a fearsome 'sporting'

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[1] Chapter 8 of L Drickamer & D Dewsbury (2009) *Leaders of Animal Behaviour – The Second Generation.* Cambridge University Press. A volume of invited autobiographical chapters by ethologists. Internet edition of this chapter slightly modified, for example to include hyperlinks to other web addresses Pictures are as in the published edition, plus one of Charles Simonyi.

work called *Tiger Lady,* had a son who wrote the definitive handbooks on the *Birds of Borneo* and *Birds of Burma*. Like my father and his two younger brothers, I was all but born with a pith helmet on my head.

My father himself read Botany at Oxford, then became an agricultural officer in Nyasaland (now Malawi). During the war he was called up to join the army in Kenya, where I was born in 1941 and spent the first two years of my life. In 1943 my father was posted back to Nyasaland, where we lived until I was eight, when my parents and younger sister and I returned to England to live on the Oxfordshire farm that the Dawkins family had owned since 1726.

It was through my father's middle brother that I met the young David Attenborough, already famous but not yet a household name. This uncle chose Sierra Leone for his enactment of the khaki-shorted family tradition, and David Attenborough was his guest on a filming expedition up country. When my uncle and aunt moved to England and I happened to be staying with them, David brought his young son Robert to visit, and he had us wading all day in shorts through ditches and ponds with fishing nets and jam jars on strings. I've forgotten what we were seeking – newts or tadpoles or dragonfly larvae, I expect – but the day itself was never to be forgotten. Even that experience with the world's most charismatic zoologist, however, wasn't enough to turn me into the boy naturalist that I should have been from the start.

Richard Dawkins aged about 7

My father's youngest brother was an innovative forest ecologist in Uganda. He later moved to Oxford, where he lectured in biological statistics – a teacher of genius with an unmatched ability to explain difficult ideas in simple language. It was for this that I later dedicated a book, *River Out of Eden,* to him. The worst he could say of a young man was "Never been in a youth hostel in his life"; a stricture, which, I am sorry to say, describes me to this day. My young self seemed to let down the traditions of the family.

I received every encouragement from my parents, both of whom knew all the wildflowers you might encounter on a Cornish cliff or an Alpine meadow, and my father amused my sister and me by throwing in the Latin names for good measure (children love the sound of words even if they don't know their meanings). Soon after arriving in England, I was mortified when my tall, handsome grandfather, by now retired from the Burma forests, pointed to a blue tit outside the window and asked me if I knew what it was. I didn't and miserably stammered, "Is it a chaffinch?" Grandfather was scandalized. In the Dawkins family, such ignorance was tantamount to not having heard of Shakespeare: "Good God, John" – I have never forgotten his words, nor my father's loyal exculpation – "Is that *possible*?" If Grandfather were alive today, I would explain that I learned late to love watching wild creatures: my original interest in biology came not from the woods and moors but from books.

For I became a secret reader. In the holidays from boarding school, I would sneak up to my bedroom with a book: a guilty truant from the fresh air and the virtuous outdoors. And when I started learning biology properly at school, it was still bookish pursuits that held me. I was drawn to questions that grown-ups would have called philosophical. What is the meaning of life? Why are we here? How did it all start? Biology comes closest to answering these deep questions, but that wasn't the reason I ended up in the biology stream at Oundle School. It was probably a bit of following-in-father's-footsteps, but also a genuinely inspirational young teacher. I.F.Thomas deliberately set out to teach in the tradition of Oundle's great headmaster, F.W.Sanderson (there was Arnold of Rugby and Roxburgh of Stowe . . . and there was Sanderson of Oundle). Sanderson died in 1922 so Ioan Thomas never met him, but he lived up to Sanderson's ideals, as I recounted in my inaugural Oundle Lecture in 2002, later reprinted in *A Devil's Chaplain* (2003):

> Some 35 years after Sanderson's death, I recall a lesson about *Hydra* . . . Mr. Thomas asked one of us "What animal eats Hydra?" The boy made a guess. Non-committally, Mr. Thomas turned to the next boy, asking him the same question. He went right round the entire class, with increasing excitement asking each one of us by name, "What animal eats Hydra? What animal eats Hydra*?*" And one by one we guessed. By the time he had reached the last boy, we were agog for the true answer. "Sir, sir, what animal *does* eat Hydra?" Mr. Thomas waited until there was a pin-dropping silence. Then he spoke, slowly and distinctly, pausing between each word.

> "I don't know. . ." *(Crescendo)* "I don't know. . ." *(Molto crescendo)* "And I don't think Mr. Coulson knows either." *(Fortissimo)* "Mr. Coulson! Mr. Coulson!"
>
> He flung open the door to the next classroom and dramatically interrupted his senior colleague's lesson, bringing him into our room. "Mr. Coulson, do you know what animal eats Hydra?" Whether some wink passed between them I cannot say, but Mr. Coulson played his part well: he didn't know. [Again] the fatherly shade of Sanderson chuckled in the corner, and none of us will have forgotten that lesson. What matters is not the facts but how you discover and think about them: education in the true sense, very different from today's assessment-mad exam culture.

With such a teacher, it isn't difficult to see why I chose biology. Unfortunately I didn't shine at that or any other subject. I spent too much of my time in Oundle's Music School, fooling around on the clarinet or saxophone, or indeed any other instrument that I might come upon unguarded. I wasn't good at music, but I had always been drawn to musical instruments and I had (still have) the ability to play, correctly and without practice, any tune almost as easily as one might whistle or hum it. This facile gift provided a constant temptation – and I readily succumbed – to dispense with reading music. The result was that, although I spent an inordinate amount of time with musical instruments, I didn't play them so much as tootle. Not time well spent. For whatever reason, my performance in science examinations at school was no better than average.

I won't say my time at Oundle was wasted, but I cannot claim to have made the best of it. My love of poetry probably came mostly from my parents, who gave me Yeats and Housman and Rupert Brooke, although my form master in my first year, Snappy Priestman, moved me with his readings from Shakespeare and Kipling. Oundle had the finest workshops of any school in the country and a unique tradition, dating back to Sanderson, of sending every boy into the workshops for a whole week in every term. All day, every day during the Week in Workshops, normal lessons were suspended; we donned brown overalls over our grey suits and – in theory at least – worked at becoming good with our hands. But only in theory. Part of the problem was that the workshops were *too* well equipped and we were too closely supervised – not by proper teachers but by workshop technicians with no idea of pedagogy at all. We did exactly what we were told, on advanced and expensive machines, and each of us ended up making something – a 'marking gauge' one term, a 'drill stand' the next – that looked exactly like what everybody else was making. I didn't even know what a marking gauge was. Like labourers on a factory production line, we learned how to follow instructions when operating a lathe or other large piece of advanced machinery. Maybe some of us learned ingenuity, inventiveness, improvisation, resourcefulness, design, but I certainly didn't, and there was no incentive to. It never occurred to me at the time, but Sanderson must have been spinning in his grave.

Undergraduate

My father and grandfather were keen for me to follow nine earlier members of the Dawkins family into Balliol College, Oxford. My parents went to see Mr. Thomas, who did his best to be cheerful. "Well, he might just scrape into Oxford, but Balliol is probably aiming too high." Nevertheless I applied to Balliol, and Mr. Thomas, despite – or more probably because of – his misgivings, had me round at his house in the evenings for extra coaching – for which he would have received no extra payment and no recognition from the school. He was just a great teacher, doing what Sanderson would have done. And he got me into Balliol.

I was in well into my second year at Oxford before my interest in the deep questions of existence, and biology's contribution to solving them, really found room to flourish. If I have made anything of my life, it was the Oxford tutorial system that first made me. Imagine the effect on an impressionable nineteen-year-old. Textbooks became a thing of the past, together with the whole notion that there existed a received answer to every question. I had the run of one of the world's great libraries. I was sent there each week with a list of readings from the original research literature, and required to write an essay, evaluating the evidence to make my own mind up about what might often be a controversial question. What a heady experience. My later panegyric was published in a variety of places, including David Palfreyman's (2001) collection, *The Oxford Tutorial: 'Thanks, you taught me how to think'*. By way of example, I mentioned my essay on the abstrusely detailed subject of the starfish water vascular system:

> I remember the bare facts about starfish hydraulics but it is not the facts that matter. What matters is the way in which we were encouraged to find them. We didn't just mug up a textbook, we went into the library and looked up books old and new; we followed trails of original research papers until we had made ourselves as near world-authorities on the topic at hand as it is possible to become in one week. The encouragement provided by the weekly tutorial meant that one didn't just read about starfish hydraulics, or whatever the topic was. For that one week I remember that I slept, ate and dreamed starfish hydraulics. Tube feet marched behind my eyelids, hydraulic pedicellariae quested and seawater pulsed through my dozing brain. Writing my essay was the catharsis, and the tutorial was the justification for the entire week. And then the next week there would be a new topic and a new feast of images to be conjured in the library. We were being educated . . .

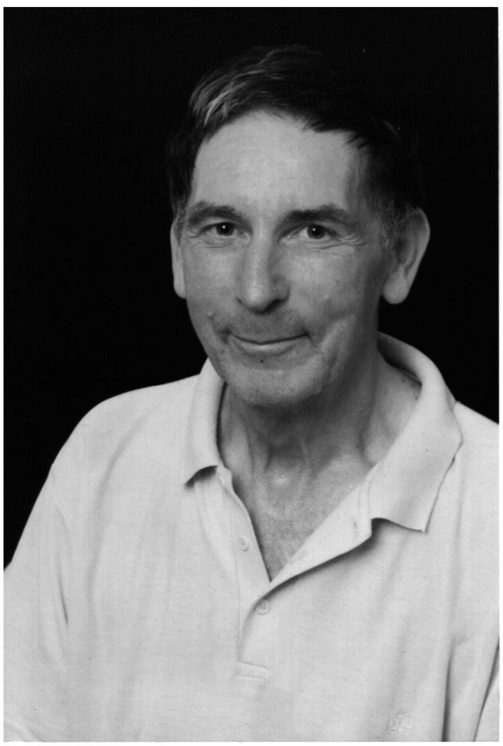
Niko Tinbergen entered my life as the lecturer on Molluscs. He announced no special affinity for that group save a fondness for oysters, but he played along with the department's tradition of handing out to each lecturer a phylum, more or less at random. From those lectures, I recall his swift blackboard drawings; his deep voice (surprisingly deep for a small man) accented but not obviously Dutch, and his kindly smile (avuncular as I thought it then, although he must have been much younger than I am now). In the following year he again lectured to us, this time on Animal Behaviour, and the avuncular smile broadened with enthusiasm for his own subject. In the heyday of Ravenglass, I was enchanted by his film on eggshell removal by blackheaded gulls. I especially liked his method of making graphs – laying out sticks on the sand for axes, with strategically placed eggshells for data points. How very Niko. How very un-Powerpoint.

Niko had by then, under the influence of Robert Hinde, Danny Lehrman and others, disowned much of *The Study of Instinct* (Tinbergen 1951). He was still loyal to *Social Behaviour in Animals* (Tinbergen 1953) even though, with the 'sociobiological' hindsight that came later, much of that book now seems nearly as disownable as *The Study of Instinct*. I wonder how much of our present theory will eventually be disowned by the hindsights of the future. I suspect not much, where the 'gene's eye view' of social behaviour is concerned, but I would say that, wouldn't I?

Niko's lectures, his writing and research, and his supervising of numerous graduate students in Oxford and Ravenglass, must have richly filled his time and he didn't do much tutoring. My mentor at Balliol, the incomparable Peter Brunet, somehow managed to persuade him to give me four tutorials in my penultimate term. Niko carried the principle of the Oxford tutorial to a quirky extreme. Where other tutors gave out a reading list that covered a topic, Niko would hand me nothing more than an unpublished doctoral thesis by one of his graduate students. I was to write an essay around the thesis, criticize it, go into the library to sleuth down its bibliography, and plan future research to carry it further. In effect, my undergraduate task was to play at being a doctoral examiner for a week, and then again the following week with a different thesis. (Later, when I myself started tutoring as a graduate student, I once or twice experimented with the Tinbergen formula of handing out a single thesis as an essay topic – but, unlike Niko, I dared to do it only with exceptionally gifted students.) I had just four tutorials with Niko and that was all it took. I threw out my plans to do biochemical research and applied instead to Niko: could I join the Animal Behaviour Research Group? I could. And it was the turning point in my career.

Graduate Student

Perhaps all scientists recall their graduate student years as an idyll. But surely some research environments are more idyllic than others, and I think there was something special about the Tinbergen group. Hans Kruuk (2003) has captured the atmosphere in his splendid biography, *Niko's Nature.* He and I arrived too late for the heroic 'hard core' period described by Desmond Morris, Aubrey Manning and others, but I think our time resembled it. We saw less of Niko himself, because his room was in the main Zoology Department while all the rest of us were housed in the annexe at 13 Bevington Road with Mike Cullen. And it was Mike who was by then the dominant influence upon the Tinbergen group.



Mike Cullen

My eulogy at his Memorial Service in Wadham College said as much, and I wanted to quote it at length here, for I believe Mike Cullen deserves a place of high honour in any history of ethology. Unfortunately, there wasn't enough space, but I have placed the complete text here.

The Friday evening seminars were the highlight of the week for the Tinbergen Group. They lasted two hours and frequently spilled over into the following week, but the time flashed by because, instead of the soporific formula of listening to a speaker's voice for an hour followed by questions at the end, our two hours were enlivened by argument throughout. Niko set the example by interrupting almost before the speaker could complete his first sentence. It wasn't as irritating as it sounds, because Niko's interventions aimed at clarification and it was usually necessary. Mike's questions were more formidable and more feared. He was the intellectual powerhouse of the seminar. Other penetrating contributors were Juan Delius and David McFarland, but the rest of us chipped in without inhibition too, almost from the first day we were there. Niko encouraged that. He insisted on absolute clarity about the question we were asking in our research. I recall how shocked I was on visiting our sister research group at Madingley in Cambridge, and one of the graduate students began to tell of his research with the words "What I do is . . ." I had to restrain myself from imitating Niko's voice: "*Ja ja,* but what is your *question?* Years later, I related this story when I gave a research seminar at Madingley. I refused to identify the culprit to a mock-scandalized Robert Hinde, and my lips are sealed to this day.

The question Niko gave me (he must have been writing his 1963 'Four Whys' paper for Lorenz's 60^{th} birthday at the time) concerned the ontogeny of behaviour, and my research method was the deprivation experiment. What is meant by the 'innate' and how does it mesh with learning in the development of the young animal? The theoretical stance I adopted was Lorenzian. Maybe Lehrman the developmentalist was right that behaviour itself could in principle not be innate, because you could never deprive a developing animal of everything (1953). But Lorenz's (1965) reply (which, with my interest in evolution I had arrived at myself before reading his book) was that you could deprive the young animal of the *specific environmental features to which the behaviour was adapted.* So you could demonstrate that the *adaptive fit* of the behaviour was innate, even if not the behaviour itself. At least in principle. How about in practice? That was what I set out to discover with my chicks (Dawkins 1968).

Newly hatched chicks peck at small objects such as spots of dirt on a wall, presumably a food-seeking response. Understandably they prefer solid to flat objects, and this carries over to photographs. But by what cues do they recognize a photographed object as solid? Humans use surface shading cues. Because the sun shines from above not below, upper surfaces tend to be lighter than lower surfaces, with a gradient between. Telescopic photographs of moon craters can look like hills depending on the direction from which the light falls. Predators use shading cues of solidity in hunting, which is why so many camouflaged animals employ countershading: the dorsal surface is pigmented darker than the ventral, thereby cancelling the expected solid appearance. The upside-down catfish *(Synodontis nigriventis)* is the exception that proves the rule (for once, the expression is spot on). This fish habitually swims upside down and, fascinatingly, it is reverse countershaded. The ventral surface has the dark pigmentation; the dorsal surface is light coloured like the ventral surface of a normal fish.

Back to the chicks: I used grain-sized photographs of top-lit hemispheres mounted at beak height on the wall of the cage, and compared them with the same photographs inverted so that the light appeared to come from below. Chicks strongly preferred to peck at correctly oriented photographs over inverted ones. Apparently, then, chicks used the same surface shading cues of solidity as we do: they seem to 'know' that sunlight shines from above. Now for the deprivation experiments. Day-olds hatched in total darkness, who had never seen anything before, gave their first (sighted) pecks indiscriminately to inverted and correctly oriented photographs equally. Did this mean they normally *learn* the surface shading cues of solidity – learn, in effect, that the sun is overhead? Not necessarily. It could be that the naive day-olds, having never before seen so much as a chink of light, were too startled or dazzled to discriminate. So I did the definitive experiment. I reared and tested chicks in a special cage in which light came from *below.* They would be accustomed to light, and not startled or dazzled when they came to be tested. If learning is important, these chicks should if anything learn that solid objects are lighter on the underside, and hence prefer inverted photographs when tested. In fact, the chicks behaved like normal chicks. They overwhelmingly preferred the uninverted photograph, the one illuminated from above, the one that looked solid to human eyes. In Lorenzian terms, this showed that the adaptive information is innate: my chicks were telling me that they are born with the 'advance knowledge' that the sun shines from overhead. No doubt there are loop-holes in the logic, but I still think the experiment is a nice teaching aid: a neat demonstration of the *kind* of logic we employ when distinguishing the innateness of behaviour itself from the innateness of the *adaptive information* whereby behaviour fits its environment as a key fits a lock.

I think much of the research that came out of the Tinbergen group, including my own – maybe ethological research generally – could be called "For example, as it might be" research, rather than "This is the way it actually is" research. Watson and Crick's double helix was a discovery about the way things are. DNA is a double helix, and that's that. End of story, it will never be superseded. Sir Ronald Ross's discovery that mosquitoes carry malaria is another example of "This is the way it actually is" research. Our research wasn't like that. If I am right, the main use of such ethological experiments is to illustrate a textbook principle. Textbooks are probably correct that animals can be fooled by some restricted part of the natural stimulus situation into behaving in a way appropriate to the whole. Sticklebacks responding to red dummies are a nice *illustration* to get the textbook point across, but the true story of sticklebacks may not be so straightforward – did Tinbergen ever try a blue dummy? Similarly, my conclusion that chicks are born with the 'advance knowledge' that the sun shines from overhead may be too simple. But if there are some animals that are born with advance knowledge of some aspects of their future environment (it would be surprising if there were not), then my work illustrates the *kind* of experiment that can, in principle, be done to test it. I at least demonstrated that developmentalists were missing something if they claimed that you could never, *in principle,* do an experiment to test the innateness of behavior.

I don't know whether the camaraderie of 13 Bevington Road was exceptional, or whether all groups of graduate students nurture a similar *esprit de corps.* I suspect, at least, that being housed in a separate annexe rather than in a large university building improves the social dynamics. When the Animal Behaviour Research Group (and other outliers such as David Lack's Edward Grey Institute of Field Ornithology and Charles Elton's Bureau of Animal Populations) eventually moved into the present concrete monster on South Parks Road, I believe something was lost. But it may be that I was by then just older and more weighed down by responsibilities. Whatever the reason, I retain a loyal affection for 13 Bevington Road and my comrades of those times who foregathered at the Friday evening seminars, or in the lunch room, or over the bar billiards table in the Rose and Crown: Robert Mash, whose epidemic sense of humour I later recalled in my Foreword to his book How to Keep Dinosaurs; Dick Brown; Juan Delius whose deliriously eccentric brilliance entertainingly complemented Mike Cullen's; Juan's supernormally delightful wife Uta who gave me German lessons; Hans Kruuk, who later wrote Niko's biography; Ian Patterson; Bryan Nelson the gannet man, known to me in my first six months only from the enigmatic notice on his door, "Nelson is on the Bass Rock"; Cliff Henty; David McFarland, Niko's eventual successor who, although based in the psychology department, was a sort of honorary member of our group because his vivacious wife Jill was Juan's research assistant, and the couple had lunch in Bevington Road every day; Vivienne Benzie who introduced the sunny New Zealand girls Lyn McKechie and Ann Jamieson as yet other honorary members of the lunch group; Lou Gurr, also from New Zealand; Robin Liley; the jovial naturalist Michael Robinson, Michael Hansell, with whom I later shared a flat; Monica Impekoven; Marian Stamp, to whom I was later married for fourteen years; Heather McLannahan, Robert Martin, Ken Wilz; Michael Norton-Griffiths and Harvey Croze, who later formed a consulting partnership in Kenya; John Krebs, with whom I later wrote three papers; Michael and Barbara MacRoberts; Iain Douglas-Hamilton, unwilling exile from Africa while he wrote his thesis; Jamie Smith, with whom I wrote a paper on optimal foraging in tits; Tim Halliday, Lary Shaffer, Sean Neill and others whom I apologize for omitting.

Niko Tinbergen filming with Lary Shaffer

The Bevington Road population was tidal, emptying during the breeding seasons of the great northern seabird colonies, then filling up as the field workers returned to Oxford to write and think. Spring high tide was the fortnight of the annual ‘Block Practical’ when we were suddenly over-run by a swarm of undergraduates, learning how to do research under Mike Cullen’s guidance with help from several of us. Once again, the emphasis was on the clear formulation of discrete questions, and more especially on quantifying the answers. For example, one pair of undergraduates working with me noticed that baby chicks utter rhythmic, piercing cheep cries. These have been labelled ‘distress calls’, but could the students pin down the conditions under which they are uttered? The guiding didactic was not to tell the students what to do but to encourage them to suggest it for themselves: “How are you going to quantify cheep calls?” “Count them.” “OK, but how do you decide when a call is too quiet to count as a true cheep?” The students might at this point suggest some kind of decibel meter, but we didn’t have one so, instead, we might steer them towards the idea of inter-observer correlation: “If you and your research partner can’t see each other counting, do you end up with the same score?”

“OK, so we have our method of quantification. Now, what hypotheses are you testing? What provokes cheeping”? “They seem to be lonely.” “All right, but what is ‘lonely’? Don’t trust subjective language; look for something you can demonstrate experimentally. How might you manipulate loneliness experimentally?” “Pick up a chick and put it by itself.” “OK, but what are you going to compare it with?” The same chick while in a group of six.” “And how many chicks will you test, alone versus in a group of six?” “Ten chicks, for one minute in each condition.” “And will you always test each chick alone first and then in company?” “Oh, no, I guess we ought to control for order effects.” “Good, why don’t you get on with that experiment and I’ll come back in an hour and see how you are getting on. Don’t stop until you have done the full number of trials specified in advance by your experimental design.”

“OK, how did that go?” “A Matched Pairs Test, with each chick as its own Control, showed a statistically significant effect of loneliness.” “Well done. Now be more specific. How lonely is lonely? What if, instead of five companions, they have only one?” “Oh yes, that’s a good idea. And then, if one companion is enough to reduce the cheeping rate, why don’t we try a mirror, compared with the non-reflecting back side of the mirror?” (You see how Tinbergenian these experiments were). How about little balls of yellow cotton wool on stalks, instead of real companions? Do they have to be yellow? Does it help if you give them ‘eyes’?

How about dyeing real chicks different colours instead of yellow? How about no visual stimuli at all but a loudspeaker playing the noise of an invisible clutch of chicks?" The students raced off to do these experiments, which always had to be properly controlled for order effects and other confounding variables, and – one of the things Mike Cullen added to the original Tinbergen version of the block practical – they always had to be analysed with proper statistics. By the way, though they were surely not cruel, most of these experiments could not be done nowadays without a government licence, which would probably not be granted for student experiments. In other words, this kind of education in the quantitative research mentality is impossible in Britain today.

Bevington Road, and especially its satellite research stations in the gull colonies, ran a system of 'slaves' – young unpaid volunteers who wanted a brief taste of the Tinbergen experience before going to university. Among them were Fritz Vollrath (who later returned to Oxford to head a flourishing group working on spider behaviour, and remains a close friend), and (also from Germany) Jan Adam. Jan and I found an immediate affinity, and we worked together. He had remarkable workshop skills and, fortunately, these were the days before 'Health and Safety' existed to protect us from ourselves and sap our initiative. Jan and I had the freedom of the departmental workshops: lathes, milling machines, bandsaws and all. We (that is to say Jan, with me as willing apprentice) built an apparatus to automate the counting of chick pecks, using delicately hinged little windows and sensitive microswitches. Previously, when working on the surface shading illusion, I counted pecks by hand. Suddenly, I was in a position to collect huge quantities of data. And this opened the door to a completely different kind of research.

I early knew the name of Peter Medawar because he was an exact contemporary of my father in the biology stream at Marlborough College, and then at Oxford (Medawar in Zoology, my father in Botany). Medawar, as British biology's star intellectual, gave a visiting lecture at his old Oxford department, and I remember the excited buzz in the waiting audience (again, no bossy Health and Safety to prohibit standing-room-only). The lecture led me to read Medawar's essays, later anthologized in *Pluto's Republic* (1982) and it was from them that I learned about Karl Popper. I became intrigued by Popper's vision of science as a two-stage process: first the creative dreaming up of a hypothesis, followed by attempts to falsify predictions deduced from it. Just as Niko's stickleback experiments served to illustrate textbook principles, so I became fascinated by the idea of a textbook Popperian study: dream up a hypothesis that might or might not be true, deduce precise mathematical predictions from it, and then try to falsify those predictions in the lab. Jan's apparatus for counting massive numbers of pecks gave me the opportunity.

Medawar elsewhere made the point that scientific research doesn't develop in the same orderly sequence as the final polished 'story'. Real life is messier than that. In my own case it was so messy that I can't remember what gave me the idea for my 'Popperian' experiments. I remember only the finished story, which, as Medawar would have expected, gives an implausibly tidy impression.

The finished story is that I dreamed up a model of what might be going on inside a chick's head when it decides which of several alternative targets to peck at, did some algebra to deduce predictions from the model, then tested them in the lab. It was a "Drive/Threshold" model, with affinities to the classic Lorenz psychohydraulic model but more precise (Dawkins 1969a), somewhat along the lines of Bastock and Manning's threshold model of *Drosophila* courtship behaviour. It is good Popperism only because the predictions I deduced from it, by simple algebra, were mathematically precise. I mean, they didn't just predict that a measured quantity should be larger, say, than some other measured quantity. The prediction was that a measured quantity should be *exactly equal to* some function of other measured quantities. Several predictions, and more elaborate deductions from extended versions of the model (Dawkins 1969b), were upheld – less accurately, I suppose, than experimental physicists expect, but with an accuracy which, for ethologists, seems spectacularly good: we would normally expect points to be vaguely clustered on a graph, not lined up like soldiers on parade as mine were. I used data not just from my own experiments but also from Monica Impekoven's on blackheaded gulls (Dawkins and Impekoven 1969) and various studies in the literature including preferences for composers, among the members of four leading American symphony orchestras. An odd consequence of this research was that, because I had derived my very own 'formula' (using nothing more advanced than school algebra) I somehow acquired an undeserved (and unsought) reputation for mathematical expertise.

I gave a talk on this work at the 1965 International Ethological Conference in Zurich. For the talk, I built a physical model of my theory, incorporating a rubber tube filled with mercury that I jiggled up and down to represent fluctuating "drive". The rubber tube was attached to the bottom of a vertical glass tube, into which were let three electrical contacts at different depths, representing "thresholds". Mercury is an electrical conductor, so when the jiggling column hit any of these contacts (the "drive" exceeding the "threshold") a circuit was completed. Coloured lights flashed to indicate "pecks". The assumption of the model (from which I had derived my formula by algebra) was that pecks are distributed randomly between all colours whose threshold is exceeded at the time (obviously if mercury was in contact with any electrode, it was automatically in contact with all lower electrodes too), and I implemented this rule by means of a noisy system of electromechanical relays switching on coloured lights to represent pecks at different colours. The whole Heath-Robinson (American translation: Rube Goldberg) affair was calculated to bring the house down just as, at an earlier Ethological Conference in Oxford, a spoof hydraulic simulation devised by Desmond Morris and friends reputedly had. How I managed to transport it from Oxford to Zurich evades my memory, and indeed my comprehension. There's not a chance that today's airport security would allow anything remotely like it, bristling as it was with amateurishly soldered wires, relays, batteries and mercury.

Alas, just as I was about to go on the big stage (there were only plenary sessions at the IEC in those days) something went wrong and my contraption didn't work. I was in a sweat of panic and couldn't think straight, frantically tinkering on the floor outside the theatre, when I suddenly heard an amused Austrian accent barking out peremptory instructions at great speed behind me. The rapid-fire voice told me exactly what to do. As in a dream I obeyed, and it worked. I turned to look at my saviour, and beheld Wolfgang Schleidt, whom I hadn't previously met. Without any prior knowledge of what my infernal machine was supposed to do, this highly intelligent rising star of continental ethology had come upon my panic, instantly sized up the problem and dictated the solution to me. I have always been grateful to Dr Schleidt, whom I later was not surprised to learn had a reputation for technical ingenuity. I bore my strange device up into the theatre and at the end of my talk its spluttering coloured lights received something akin to a standing ovation. In the audience was George Barlow, rising star of American ethology, and he was sufficiently impressed by my talk to get me invited to the University of California at Berkeley for my first proper job, as a very junior Assistant Professor of Zoology.

Berkeley

I was torn between this flattering offer and another one from Schleidt himself at the University of Maryland, whither he had recently moved from Germany. Though tempted by Maryland, which, besides Schleidt, had other clever ethologists in the shape of Jack Hailman and Keith Nelson, the lure of California was strong. I was by then engaged to Marian Stamp, another Tinbergen graduate student, and we looked forward to an exciting new life by the Pacific, epitomised by the current hip anthem: "And if you're going to San Francisco, Be sure to wear some flowers in your hair . . ." Niko rightly decided that Marian needed no hands-on supervision.

She would simply get on with her experiments on search images at Berkeley instead of Oxford. That is exactly what she did, with great success, in the excellent scientific atmosphere provided by the Berkeley Zoology Department.

I taught the undergraduate course in Animal Behaviour jointly with George Barlow, basing my lectures largely on my experience from Oxford. During my last year there before I left for Berkeley, Niko had taken a sabbatical leave, and asked me to stand in for him and deliver the 1966 Animal Behaviour lectures to the undergraduates. He had offered me his lecture notes but, under the influence of Mike Cullen, I had become fascinated by the ideas of Bill Hamilton (whom I had not then met) on inclusive fitness, published two years before. When I came to write my lectures on social behaviour, I pushed the "gene's-eye-view" to centre stage. I was in some trepidation at my decision to depart so far from the Tinbergen canon, and also at the extravagant flourishes of my rhetoric: the body as mortal throw-away receptacle for the immortal genes, tripping like chamois from body to throwaway body down the generations. Seeking reassurance, I showed my lecture notes to Mike Cullen. He immediately took the allusion to Hamilton, and wrote "lovely stuff" in the margin. That was enough for me. I cast hesitation aside and went to town on the gene's-eye-view. I did the same thing in my Berkeley lectures, and I like to think that the undergraduates of Oxford and Berkeley in the late sixties were among the first to hear of the new ideas that were to become fashionable, in the seventies, as 'sociobiology' and 'selfish genery'.

Other close friends at Berkeley along with George Barlow were David Bentley the neuro-ethologist, Michael Land, now the world's leading authority on eyes throughout the animal kingdom, and Michael and Barbara MacRoberts, who later came to Oxford as spirited additions to the Bevington Road circle, as, later, did David Noakes, who was George Barlow's leading graduate student during my Berkeley years. George hosted a weekly ethology seminar for interested graduate students at his house in the Berkeley hills, and those evening meetings recaptured for Marian and me the wonderful atmosphere of Niko's Friday evenings at Oxford. My research at that time was a continuation of my chick pecking work. In the departmental workshops (again, no initiative-crushing "Health and Safety') I made a Skinner Box for chicks using a heat lamp for reward instead of food, in which I tested further predictions of my "threshold" model, looking at actual sequences of pecks rather than just total numbers of pecks per minute. This work was later published (Dawkins and Dawkins 1974).

Some time during our second year at Berkeley, Marian and I were visited by Niko and Lies Tinbergen. Niko wanted to persuade us to return to Oxford, where he had an attractive research post to offer me, and Marian could write up her doctoral research, which, as Niko could see, was going well at Berkeley. He returned to Oxford, leaving us to think about the offer. We decided to accept it, but meanwhile Niko had written of a new opportunity. Oxford had decided to initiate a University Lectureship in Animal Behaviour accompanied by a Fellowship at New College, and Niko wanted me to apply. This teaching job would not preclude the research opportunity he had earlier promised me. I agreed to apply for the Lectureship, and Oxford flew me over for the interview.

It was a magical time, with what seemed like all before me. Once again, music stamped the memory. This time it was Mendelssohn's Violin Concerto, which I listened to on the plane, spellbound by the Rocky Mountains below and by exciting prospects ahead. Oxford put on its very best performance, which is the May time blossoming of cherry and laburnum all along the Banbury Road. New College, too, played its golden fourteenth century part and I was happy, my exuberance not dimmed by the news that Colin Beer had put in an unexpected late application for the Lectureship, and Niko had excitedly switched his allegiance from me to Colin. If Niko had decided that Colin was a better bet, that was good enough for me. I would still have the research position and, as I told the interviewing committee, if Colin were there in Oxford too, so much the better. They indeed gave the job to Colin, and I indeed took up the research grant.

Back to Oxford

Marian and I left Berkeley with mixed emotions. Our time there, from 1967 to 1969, was rather politically active. Like most of our friends, we became heavily involved in the anti Vietnam War movement, and various other less reputable political issues such as the 'Peoples' Park, locally manufactured and later to be satirized by David Lodge (as the 'People's Garden' at 'Euphoria State College') in *Changing Places.* Berkeley remained an episode of magic in memory, a happy/sad dreamtime of lost youth, of clever and friendly colleagues, of clear, bright sunshine over the Golden Gate, of gentle people putting flowers in the rifle barrels of the California National Guard. We crated and dispatched our few belongings from our Berkeley apartment and drove right across the continent in our old Ford Falcon, thickly encrusted with anti-war slogans and Eugene McCarthy election stickers, to New York. We sold the Ford on the quayside, boarded the liner *France* for Southampton, and prepared to resume our life at Oxford with many of our old friends still there and Colin Beer newly arrived. In the event, Colin preferred to spend his time in New College and was scarcely seen in the Department, much to everyone's disappointment. He stayed only a year. Danny Lehrman had far-sightedly kept his position at Rutgers warm for him and, when it became clear that

Oxford could not find a position in Medieval French to match the professorship his wife held in America, Colin decided to return. Once again, the Lectureship in Animal Behaviour was advertised, once again the long-suffering New College agreed to associate a Fellowship with it, and once again Niko urged me to apply. This time, however, I really wanted it, and this time I got it.

The life of a Tutorial Fellow of an Oxford College is in many ways a charmed one. I got a teaching room in a mediaeval building surrounded by famous gardens, a book allowance, housing allowance, research allowance, and free meals (not free wine, contrary to envious rumours) in the stimulating and entertaining company of leading scholars of every subject except my own. The stimulating scholars of my own subject were to be found in the Zoology Department – where I spent the majority of my time. The glory days of 13 Bevington Road came to an end and the Animal Behaviour group moved to the battleship-like horror on South Parks Road, then informally known as HMS Pringle after the ambitious Linacre Professor who persuaded the university to build it (I have mixed feelings about my part in getting it recently named the Tinbergen Building, for it is widely reputed as the ugliest building in Oxford). The ironically named "Laughing John" Pringle was further immortalised in the mock-German past participle *"abgepringelt"* which became the Department's in-word for any kind of ruthless modernization.

In the same spirit of modernity, my research grant paid for a PDP-8 computer, my pride and joy and a valued resource for everybody in 13 Bevington Road. I was already acquainted with large mainframe computers, both at Oxford during my doctorate and at Berkeley, a fact that may have contributed to my undeserved reputation for mathematical sophistication. Jobs were submitted to the mainframes on miles of punched paper tape (British computers) or barrowloads of punched cards (American computers) and came back the following day, so there was a premium on not making trivial mistakes in programming. Unlike today, we did our own programming, and results came back not on screens but clumsily printed on paper. Before I left for Berkeley, a major feat of *Abgepringelheit* had been the luring to Oxford of David Phillips's molecular biophysicists from London. They brought with them an Elliott 803 computer, and its keeper Dr Tony North kindly allowed me to use it at night. This was when I became fully aware of the addictive lure of computer programming. I really did literally – and frequently – spend all night in the warm, glowing computer room, entangled in a spaghetti of punched paper tape, which must have resembled my insomnia-tousled hair. The Elliott had the charming habit of beeping an acoustic rendering of its inner processing. You could listen to the progress of your computation through a small loudspeaker which hummed and hooted a rhythmic serenade, doubtless meaningful to Dr North's expert ear but merely companionable to my nocturnal solitude.

When I returned to Oxford from Berkeley, the singing Elliott had gone the way of all silicon, but my 'own' PDP-8 was more than a substitute, and programming became even more of an addiction than before. Previously I had used only high-level compiler languages but, in order to use the PDP-8 as a research tool, I had to master its 12-bit machine code, and I threw myself into this with zest. My first machine-code project was the 'Dawkins Organ', a system for recording behaviour using a keyboard. Engineers had developed rival systems, using sophisticated and expensive hardware. I wanted to replace almost all the hardware with software. Following a brilliant suggestion by my Oxford colleague Roger Abbott, I succeeded, and over the next few years Dawkins Organs were used by numerous members of the Oxford Animal Behaviour Group, and even – for I published a paper on the design (Dawkins 1971), and supplied the software free of charge – by some ethologists elsewhere in the world, for example Canada.

Marian soon obtained her doctorate, and we started to collaborate on research. We had always been good colleagues, thinking aloud to each other at mealtimes and on walks in the country. Our doctoral theses had both, in their different ways, approached the question of what it means to talk of decision-making in animal behaviour. We now planned a study that would – yet again – serve to illustrate a textbook point. The point was that decisions occur between fixed action patterns (FAP) not within them. Once a FAP had begun it would continue to completion, after which there would be a new decision on what to do next. That was the textbook ideal, but could we demonstrate it statistically? We decided to film behaviour (it happened to be drinking in chickens, an elegant *glissando* of a movement) and analyse it frame-by-frame. Any frame that was highly predictable from preceding frames would be deemed part of a fixed action pattern. Moments of high unpredictability would be our 'decision points' (Dawkins and Dawkins, 1973 & 1974). We were, in effect, testing the hypothesis that frames were bimodally distributed: either very predictable or very unpredictable but not intermediate. Using an information theory metric, we plotted a graph of predictability, measured in bits of information, against time. The research could almost be described as philosophical: helping scientists to clarify what they mean (in this case by 'decision') rather than actually finding something out about animals. Marian and I followed this with a similar, but more elaborate piece of work on grooming behaviour in flies (Dawkins & Dawkins, 1976). We used a Dawkins Organ to record the behaviour, and it then occurred to us to *listen* to the tape to see if any pattern emerged. It did. We could *hear* the rhythms and melodies of the flies' Fixed

Action Patterns as well as analyse them statistically! The tunes reminded me of the Elliott computer, companion of my youthfully mis-spent nights.

In 1974, I became the British Editor of *Animal Behaviour.* I took over the job from David McFarland who, in turn, had succeeded Pat Bateson. I can't say I enjoyed it but it was made enormously more bearable by the cheerfully efficient Jill McFarland, my assistant. Jill had been doing the job with David, so the office simply went on running as a well-oiled machine. I probably didn't make much difference as Editor, although I occasionally tried to improve the clarity of the writing. My particular bugbear was the formulaic scientific paper with its standard headings: Introduction, Methods, Results, Discussion. The rubric's limitations were especially glaring when – as was common – the author had done a series of experiments, each one prompting the next. I tried to persuade authors that the proper sequence for such a paper was: Question 1; Methods 1; Results 1; Discussion 1 leading to Question 2; Methods 2; Results 2; Discussion 2 leading to Question 3 . . . and so on. You'd be amazed how many people arranged their paper in the following way: Introduction; Methods 1, Methods 2, Methods 3, Methods 4 . . . ; Results 1, Results 2, Results 3, Results 4 . . . ; Discussion. Could anything be more obviously calculated to confuse and bore? But I don't think I had any lasting impact on the standard format of the scientific paper, and I was glad to pass the Editorship into Peter Slater's more capable hands in 1978.

In 1974, the year after he shared a Nobel Prize with Konrad Lorenz and Karl von Frisch, Niko retired and the coveted position of Reader in Animal Behaviour was advertised. There was a stiff competition in which I played no part (recounted by Hans Kruuk in his biography of the Maestro) and eventually David McFarland emerged as the new Reader. David had an idiosyncratic approach to the study of behaviour, highly original but difficult for non-mathematical ethologists to understand. He surrounded himself with bright people, including Richard Sibly and Robin McCleery (who tragically died in 2008) and the evening seminars continued stimulating and interesting, as in Niko's era. The work that Marian and I were doing on the statistics of decision-making fitted into the mathematical atmosphere rather well. And I became increasingly addicted to programming computers.

The Dawkins Organ was only one of several programming projects to which I devoted more time than was good for me or my career. While still at Bevington Road, I had devised my own language, BEVPAL, to speed up machine code programming for the PDP-8 (indeed the Organ program was itself partly written in BEVPAL). I now wrote a program to translate any program from one language to another. Not a very useful exercise, but it taught me a lot about the theory of syntax and enabled me to read Chomsky with far more comprehension than I otherwise could have mustered. Again using syntactic principles, I wrote a program to simulate the song of any cricket. Punningly called Stridul-8, this PDP-8 program was planned for an ambitious series of experiments on a laboratory population of crickets of the Pacific Island species *Teleogryllus oceanicus,* which my graduate student Ted Burk maintained from stocks supplied by my old Berkeley friend David Bentley. David had persuaded me that crickets were the animals that everybody should be working on. Although Ted went on to complete a good doctorate on other aspects of cricket behaviour, my project on behavioural responsiveness of crickets to computer-generated song was never completed, and my song-simulation software was scarcely used in earnest, though it worked well and was versatile enough for anybody to synthesize the song of any cricket in the world.

Examining a cricket in Oxford in the 1970s

One of my most ambitious programming projects was inspired by daily exposure to the ethos of the McFarland research group. The talk in the coffee room and the evening seminars was of control theory models of animal behaviour: boxes and arrows and feedback loops. The natural way to simulate a control model is with an analog computer. You set up a model by patching together a circuit of modules with names like integrator, subtractor, adder, multiplier, comparator. Then you switch on the analog computer and see what happens. We didn't possess an analog computer, but I was taken with the idea that a digital computer can be programmed to do anything that any other machine can do, so I set out to program the PDP-8 to behave like an analog computer. Writing my GenSim program probably wasn't a good use of my time, and, as with the language translation program and the cricket song simulator, the software was scarcely ever used, but the exercise taught me a lot about integral calculus, and it equipped me to keep abreast of Robin McCleery, Richard Sibly, Alasdair Houston, Ivor Lloyd and the other control theorists of the McFarland group who dominated my world at the time.

For me, the decision-making work, which I had begun with my thesis and continued in the joint papers with Marian, came to a climax – and closure – in

1975. On their 25th anniversary, our sister group at Madingley decided to hold a birthday conference, to which both David McFarland and I were invited as the Oxford contingent. I threw myself into producing something completely new, and spent a year, with Marian's constant advice and encouragement, working on a paper on Hierarchical Organization (Dawkins 1976a). I won't try to summarise it here, but it is of all my papers perhaps the one to which I devoted the most hard thought and concentrated effort. A red-letter year for me and the end of an era (that's what I meant by 'closure'), 1976 was also the year in which my first book, *The Selfish Gene* was published (Dawkins 1976b).

Selfish Genery

Once again, the real story of *The Selfish Gene* was less tidy. It wasn't really a new departure after 'closure'. I had actually written the first chapter two years earlier. The winter of 1973 was a time of industrial unrest in Britain under Edward 'Grocer' Heath, and there was a period known as the three day week when electricity was available only intermittently. This made my normal research impossible and I turned to something that needed only sunlight (or, at most, candles). I would write a book, expounding the "gene's eye view" that had dominated my lectures at Oxford and Berkeley in the 1960s. I was moved to do so by a spate of popular books of the time, which promoted a kind of group selection. The authors of these books, for example Konrad Lorenz and Robert Ardrey, never properly realised they were promoting group selection. They seem to have so misunderstood Darwinian natural selection as to think that it actually *was* a theory of group selection! My main motive in writing *The Selfish Gene* was to counteract this by expounding neo-Darwinism from first principles, and that meant – so I maintained – the "gene's eye view".

The three day week came to an end. I went back to the laboratory and put my unfinished chapter away in a drawer until 1975 when I took a sabbatical leave, dusted off the chapter again, and finished the whole book rather swiftly. By then new theoretical ideas, by Robert Trivers and John Maynard Smith were available to supplement those of Bill Hamilton and George Williams, which had inspired the first chapter. On Desmond Morris's advice, I showed some chapters to Tom Maschler, doyen of London publishers, in his spacious, book lined room at Jonathan Cape. He liked the book but not the title: 'selfish' is a 'down word'; I should call the book *The Immortal Gene.* Perhaps he was right. He might have published the book but meanwhile Roger Elliott, the professor of Theoretical Physics and a colleague at New College, had introduced me to Michael Rodgers of Oxford University Press and from then on there was never any question about who would publish it. Michael simply had to have the book, and he would not rest until he got it: "I haven't been able to sleep since I read it. *I must have that book."*

There is something exhilarating in having your first book published, especially when you have a publisher like Michael Rodgers. In joking reference to ecological theory, he has described himself as a 'K-selected publisher', and that is exactly right, with the addition of the adjective 'obstinate'. If your publisher is K-selected and obstinate, and if he really likes your book, there are no lengths to which he will not go on its behalf. One anecdote. The International Ethological Conference in 1977 was at Bielefeld. I was invited to give a plenary talk, and I used it to introduce the idea of the extended phenotype as an outgrowth from *The Selfish Gene*. The conference bookshop had a few copies of that book, and they instantly sold out. The bookseller frantically telephoned Oxford University Press to try to get emergency reinforcements and she received a brush-off from one of the suits who infest such organizations: "Whom do we have the honour of addressing? Well, you must understand, we have procedures to go through, you might get the books in three weeks if you are lucky." Three weeks would have been much too late: the conference would be long over. The German bookseller appealed to me for help, and I telephoned Michael Rodgers. My memory still hears the slam of his fist on the desk: "Good! You've come to the right man!" I don't know how he did it but, the very next day, a large box of books arrived in Bielefeld. I had indeed gone to the right man. Not just on that occasion, but in the first place. If you have a book to sell, go to a K-selected publisher. An obstinate one.

The Selfish Gene was well received by the critics, almost the only major exception being Richard Lewontin, a man of fabled intelligence who completely missed the point and thought it was an advocacy of panglossianism and genetic determinism! It was an especial joy for me to receive a rave review from my intellectual hero Bill Hamilton, lately migrated from London where he felt unappreciated, to Michigan, where he was deservedly appreciated by the flourishing group of Darwinian social theorists under Richard Alexander. I met Bill in a London underground train, shortly before his departure for Michigan. He told me he was reading *The Selfish Gene* and far preferred it to *Sociobiology*, which, of course, delighted me. His review in *Science,* when it eventually appeared, was all that I could have hoped for. Not just complimentary but deeply and idiosyncratically *thoughtful.* Bill characteristically ended by quoting two poems, one by Wordsworth (the famous lines on the statue of Newton in Trinity College, Cambridge) and one by Housman (I don't know whether Bill consciously identified with the melancholy protagonist of *A Shropshire Lad,* but it would have been in character):

From far, from eve and morning
And yon twelve-winded sky,
The stuff of life to knit me
Blew hither: here am I
 . . . . .
Speak now, and I will answer;
How shall I help you, say;
Ere to the wind's twelve quarters
I take my endless way.

What "stuff" and what "I" was Housman referring to, Bill wondered: memes or genes. Bill later became my Oxford colleague and dear friend. His premature loss remains hard to bear. I organized his secular funeral in New College chapel, and spoke a eulogy, which is reproduced as Chapter 4.1 of *A Devil's Chaplain.*

After *The Selfish Gene* and the Bielefeld conference, my research interests took a new turn with the arrival in Oxford of Jane Brockmann as a post-doc. Her PhD under Jack Hailman at Wisconsin was a field study of digger wasps, *Sphex ichneumoneus,* American solitary wasps similar to those made famous by Tinbergen and Baerends in Holland. Jane's study was a classic of field observation. She colour-marked all the individual females digging and provisioning nests in two study areas. She possessed precisely timed records of exactly when each marked individual started digging a burrow, left it, returned with prey, left again, entered another wasp's burrow, fought with another individual, sealed up a burrow, started another burrow, etc. She had used her data for a completely different purpose in her thesis. But it was Alan Grafen at Oxford who clearly saw the econometric possibilities opened up by such carefully *timed* individual data as Jane had gathered.

In mentioning Alan, I need to digress for a moment. I have taught, and learned from, some good students in my time, both undergraduate and postgraduate. But I am sure they would not mind my singling out Alan Grafen, Mark Ridley and Yan Wong as ex-students from whom I have learned *hugely* more than I ever gave them as a teacher. Alan was, at the time of Jane Brockmann's year in Oxford, doing a master's degree in mathematical economics after his BA in experimental psychology (during which I had tutored him). He was greatly valued as a kind of honorary member of the Animal Behaviour Research Group, in advance of joining it formally to do his D.Phil (Oxford-speak for PhD) with me. Alan's formidable intellect devoured Jane's data like a swarm of locusts. Alan and Jane showed me how a really good collaboration can be one of the great pleasures science has to offer. What Alan taught Jane and me during that collaboration (Brockmann, Grafen and Dawkins, 1979) was the importance of *time* as an economic variable in animal behaviour. I gladly pay tribute to the earlier mentoring I received from Niko Tinbergen and Mike Cullen. But what I later learned from the young Alan Grafen during our wasp work was, I think, at least as influential on me. Alan wasn't so directly involved in the two further papers that Jane and I wrote on digger wasps (Brockmann & Dawkins 1979, Dawkins & Brockmann 1980), but his extraordinary biological and economic intuition continued to guide us. The second of those two papers, by the way, was an intriguing demonstration that digger wasps behaved as if committing the 'Concorde Fallacy', a term that I had introduced in *The Selfish Gene* (and in Dawkins & Carlisle 1976).

The learning process carried on after Jane took up her faculty position at the University of Florida at Gainesville. I went there for a sabbatical term, taking Alan, who was by now my graduate student, with me. Jane and I ran a graduate seminar and it was almost embarrassing how, in the nicest possible way, Alan seemed naturally to take command. He played the same role as he had during our Oxford collaboration, but now he was playing it for the benefit of a whole class of students, most of them older than he was. I would summarise his gift as a quite extraordinary economic and biological intuition. Mathematics comes into it, but Alan is like R A Fisher in that, although he can do the mathematics explicitly (and does so in his published work) it is his biological intuition that lifts him over mathematicians who think they are qualified to take over biology. As Marian put it in the Preface to one of her excellent books, Alan has "the annoying habit of always being right." He is now my colleague at Oxford.

My sabbatical term at Gainesville was my opportunity – enriched by numerous discussions with Alan and Jane – to break the back of my second book, *The Extended Phenotype*. It is the only one of my ten books aimed primarily at a scholarly audience of scientific colleagues, but I like to think that all of them can be read with profit by professionals as well as amateurs. I finished it when I returned to England, and it was published in 1982. Of all my books, I think it is my most original, although the novel part doesn't really start until Chapter 11, 'The Genetical Evolution of Animal Artefacts' where I introduce the idea of the extended phenotype itself. There isn't space to expound it here, of course. I reprised the main argument in the second edition of *The Selfish Gene*. Earlier chapters consist of replies to critics of *The Selfish Gene* and various other exercises in clarification and cleaning up. The book benefited from a month I spent in Panama. The Smithsonian Tropical Research Institute had the good habit of inviting biologists to 'interact with' the resident field researchers on Barro Colorado Island and my stint overlapped, happily, with John Maynard Smith's. To spend time with John anywhere would be a privilege. To do so in a tropical jungle, in the company of locally expert naturalists, was a bonus. How I miss him, but of course biologists all over the world are saying that.

I never worked in the same department as John, but I revered him as a mentor in the same class as Niko, Mike Cullen and Bill Hamilton at Oxford. I first got to know him at the BBC in London, where Peter Jones was producing a documentary version of *The Selfish Gene* for Horizon (Horizon documentaries were frequently rebranded with an American accent as 'Nova'). I was too nervous to accept Peter's invitation to present the show, which was a good thing as John was better than I could ever have been. The documentary undoubtedly helped sales of the book. Some eight years later, I was approached by another Horizon producer/director, Jeremy Taylor, who was planning a documentary on the 'prisoner's dilemma', with special reference to Robert Axelrod's computer tournament (1984, reprinted 1990). Jeremy wanted me to present the show, and this time I had the courage to say yes. It was released under the title 'Nice Guys Finish First'. As a result of this title, I became briefly regarded as a champion of niceness instead of selfishness, which was a welcome, if temporary change. I was wooed by three great corporations, each eager to demonstrate their niceness. The Chairman of Marks and Spencer invited me to lunch in the boardroom so he could explain to me how nice his company was to its employees (a fact that I have no reason to doubt). A woman from the public relations department of Mars Bars gave me lunch in order to persuade me that her company's motivation was not to make money but to distribute sweetness to consumers. And a man from IBM flew me to Brussels to preside over a day of prisoner's dilemma-style gaming for middle executives undergoing a refresher course. The idea was to instill in them a spirit of amicable cooperation, but unfortunately it backfired. The suited executives were divided into three teams: the Reds, Blues and Greens. They played for notional money, not real, but shortly before the game ended (and, of course precisely because it was about to end), the Reds suddenly betrayed a whole day of cooperative trust by reneging on the Blues. The bitterness was so palpable and personal, they all had to have counselling for fear they would be unable to work together in the future. As for the idea of *The Selfish Gene* being an advocacy of either selfishness or niceness, both were absurd, and good examples of the inflated importance of titles. The 'selfishness' we are talking about is of *genes.* From selfish genes, either altruism or selfishness at the individual organism level might flow, depending on the economic conditions that obtained. That was the whole point!

As with *The Selfish Gene* a decade earlier, British sales of *The Blind Watchmaker* received a boost from a BBC Horizon documentary that shared its name. Like ‘Nice Guys Finish First’ it was directed by Jeremy Taylor and presented by me. A new bout of programming addiction fed into the book, my ‘computer biomorphs’ simulation of evolution by artificial selection, the word ‘biomorphs’ being borrowed from Desmond Morris’s paintings. I described in my book the surreal exhilaration that the emergent biomorphs induced in me: -

> When I wrote the program, I never thought that it would evolve anything more than a variety of tree-like shapes. I had hoped for weeping willows, cedars of Lebanon, Lombardy poplars, seaweeds, perhaps deer antlers. Nothing in my biologist’s intuition, nothing in my 20 years’ experience of programming computers, and nothing in my wildest dreams prepared me for what actually emerged on the screen. I can’t remember exactly when in the sequence it first began to dawn on me that an evolved resemblance to something like an insect was possible. With a wild surmise, I began to breed, generation after generation, from whichever child looked most like an insect. My incredulity grew in parallel with the evolving resemblance. . . I still cannot conceal from you my feeling of exultation as I first watched these exquisite creatures emerging before my eyes. I distinctly heard the triumphal opening chords of *Also sprach Zarathustra* (the ‘2001 theme’) in my mind. I couldn’t eat, and that night ‘my’ insects swarmed behind my eyelids as I tried to sleep.

My next writing project was the second edition of The Selfish Gene (1989) commissioned by O.U.P. and published in 1989. Right from the start, the publishers agreed that the original text should remain unchanged, warts and all. Revisions would take the form of extensive endnotes, some of them quite long. And there would be two completely new chapters, ‘Nice Guys Finish First’ (the title came from my BBC television documentary, and the subject matter mostly from Robert Axelrod) and ‘The Long Reach of the Gene’ (a potted version of the last four chapters of *The Extended Phenotype).* In writing all three new portions, I was hugely helped by Helena Cronin while, in return, I offered modest help to her with her own beautiful book, The Ant and the Peacock.

I have always enjoyed collaborating, and wish I had done more of it. My three joint papers with John Krebs showed me how immensely valuable joint thinking can be – like a kind of mutual tutorial. In 1978, we wrote a chapter on ‘Animal signals: information or manipulation (Dawkins and Krebs, 1978). This paper, and its sequel (Krebs and Dawkins, 1984) were influential in redirecting the attention of students of animal communication, in two ways. Instead of treating communication as a mainly cooperative enterprise, we stressed deception (mainly under John’s influence) and manipulation (this was my contribution). It is often not realised that deception and manipulation are not the same thing, although both are important. In 1979, John Maynard Smith and Robin Holliday convened a meeting of the Royal Society on the Evolution of Adaptation by Natural Selection, and they invited John and me to present a joint paper. The subject we chose was evolutionary arms races (Dawkins and Krebs, 1979), and I think it was at least as valuable a contribution as our joint papers on animal signals. The contribution to that symposium that I mostly remember was the paper on comparative studies of adaptation by Tim Clutton-Brock and Paul Harvey. It pulled the rug from under Gould and Lewontin's 'critique of the adaptationist programme' (1979). Characteristically, however, Gould blithely went ahead and ignored Clutton-Brock and Harvey when he delivered his 'Spandrels' paper later in the same day. Even the ludicrously over-rated written version of that paper didn't deign to mention Clutton-Brock and Harvey (1979).

My life became increasingly driven by commissions rather than my own initiatives. I was invited to give the 1991 Royal Institution Christmas Lectures for Children, five one-hour lectures for a 'juvenile auditory' (the words of Michael Faraday, founder of the lectures) and since 1966 televised in their entirety. After much justified hesitation, I agreed. Not the least humbling aspect was the list of my predecessors, which began with Faraday himself and ran through just about every household name in British science ever since. The Christmas Lectures are apt to take over a scientist's life in a way no other commitment easily does. I won't say the lecturer's every footstep and gesture is choreographed for the benefit of the television audience, but there is some truth in the exaggeration.

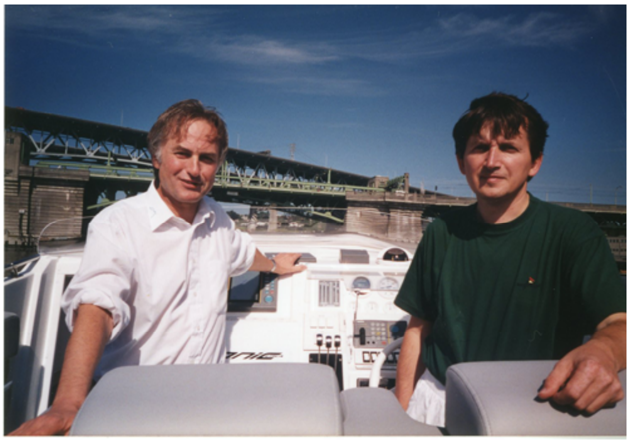
My series of five lectures was headed 'Growing Up in the Universe'. 'Growing up' had three meanings, on different timescales: first, on a timescale of decades, the growing up of an individual's understanding of the world – especially appropriate for a series of lectures for children; second, on the historical timescale of centuries, the growth of humanity's understanding of the universe; and third, on the geological timescale of millions of years, growing up in the sense of evolution. At one time I thought of writing a book with the same title, and it even mysteriously found its way onto an Amazon list. However, as it turned out, the theme was too big for one book and it spread into two: *Climbing Mount Improbable,* published in 1996 and *Unweaving the Rainbow* in 1998, whose publication was delayed by a shorter book, *River Out of Eden,* commissioned by my literary agent John Brockman and the publisher Anthony Cheetham. *Unweaving the Rainbow* owed more than just its subtitle (*Science, Delusion and the Appetite for Wonder)* to my Richard Dimbleby Lecture, commissioned by the BBC and televised in 1996. *Climbing Mount Improbable* incorporated a colour version of the biomorphs program (mostly written during a gloriously productive fortnight in Los Angeles as the guest of Alan Kay and his pioneering group within the Apple company). It also included 'arthromorphs', perhaps the most biologically interesting program I have written, in which I collaborated with Ted Kaehler, one of Alan Kay's most brilliant colleagues. I think *Climbing Mount Improbable* is a candidate for my favourite of all my books, and it is surely the most under-rated. The arthromorphs are the centrepiece of a chapter called 'Kaleidoscopic Embryos' which, together with 'The Museum of All Shells', I think captures something of the flavour of what later became known as 'evo-devo'.

River Out of Eden and *Climbing Mount Improbable* were both illustrated by Lalla Ward, who had recently become my wife. Though an accomplished artist, Lalla's main profession was acting. She is best known as Dr Who's companion, but was distinguished by her role as Ophelia to Derek Jacobi in the BBC television production of Hamlet. Her beautiful speaking voice came into its own again in 1996 when I was touring the USA, promoting *Climbing Mount Improbable*. I got laryngitis and I lost my voice . . . in San Francisco. The show must go on, and Lalla stepped into the breach, doing elegant readings from the book, after which I croaked out answers to questions from the audience. We continued like that for the rest of the tour and her readings were such a success that, even after my voice came back, we maintained the formula as a double act when promoting all my later books, and also when recording audio books. Audiences agree that the two-voice pattern makes for easier listening. I think, too, that I have learned from Lalla how to read aloud, and it stood me in good stead recently in my difficult task of recording an audio version of *The Origin of Species.* In that undertaking, I made no attempt to act the part of Darwin but instead worked hard on the phrasing and stressing, to make his prose as understandable as I possibly could.

Charles Simonyi Professor

By the early 1990s I had become fretful at my long service as University Lecturer in Animal Behaviour and Tutorial Fellow of New College. Much as I had loved the tutorial system as an undergraduate, I couldn't help feeling that as a tutor, with the best will in the world, I was becoming a little jaded. Promotion to the distinctively Oxford rank of Reader was a gratifying honour, but the workload remained the same. Approaching my last decade before retiring, I felt that I needed a complete change in order to give of my best. At the same time, the professional fundraisers at Oxford University's Development Office had the idea of using my reputation as an author to raise the money for a new Professorship in the Public Understanding of Science. They brief Oxford's New York office, which set about exploring various possibilitites until, through the mediation of Nathan Myhrvold, then at Microsoft, they found the perfect benefactor.

Charles Simonyi, originally from Hungary, was one of Microsoft's most brilliant software designers (he has now left to found his own company, Intentional Software). He has a deeply informed interest in science, and a dedicated commitment to its promotion. A tentative approach from Oxford's New York office revealed that he admired my books, and prompted an invitation for Lalla and me to visit Seattle. He invited about 30 people to dine with us from the high-tech electronics, software and biotech industries, including Bill Gates, Nathan Myhrvold and other luminaries. Charles presented every dinner guest with a copy of *River Out of Eden,* which had just been published. When, at the end of the dinner, he called on me to speak, I became alarmingly aware that this was my audition (as Lalla, with her acting background, put it) for a part that I very much wanted to play. Even more alarming was the question and answer session afterwards. A lifetime among university academics had hardened me to intelligent and searching questions, but the high-tech whiz kids of Charles's West coast circle conferred new meaning on both 'intelligent' and 'searching'. I felt lucky to come through in one piece. The next day Charles piloted us in his helicopter for a breathtaking ride towards the Canadian border, jostling the skyscrapers of Seattle on the homeward journey. Back on *terra firma*, a ten-minute meeting between him and Michael Cunningham of Oxford's New York office apparently clinched the deal. There were details to be worked out, but from our homebound plane window the dreamlike vision of Mount Rainier rearing up through the clouds seemed to promise an exciting new career: Charles Simonyi Professor of the Public Understanding of Science.



With Charles Simonyi

Oxford has a good rule that no individual should be *promoted* as a result of a benefaction. Therefore, although Charles had endowed a full Professorship, I gratefully accepted the job initially at the lower level of Reader, which was the same level as my existing job, and I even took a cut in salary. A year later, after undergoing Oxford's customarily rigorous process of peer-reviewed vetting for promotion, I was raised to the rank of Professor. Charles was generous enough to endow the post in perpetuity, so I am only the first of what I hope will be many Simonyi Professors at the University of Oxford. Charles's manifesto, which he wrote for the guidance of future Oxfords seems to me a model of generous far-sightedness, and I have reproduced it here. One of the first things I did, on assuming the Chair, was to make a modest endowment to New College to finance an annual lecture in Charles's honour. Lecturers have included two Nobel Prizewinners, the President of the Royal Society and many other distinguished scientists.

After *Unweaving the Rainbow,* my next writing project was the largest and most demanding of my career: *The Ancestor's Tale.* Once again it was a commission, this time by Anthony Cheetham of Orion Books. Anthony and his wife Georgina had been friends since he published *River Out of Eden* in 1995. Lalla and I were staying with them in their country house at Paxford in the Cotswold Hills, on the very day that little book first hit Number One in the British bestseller list, so it was a house of good omen. Some time in 1998, in the same house, Anthony urged me to write a big book, a *magnum opus* on the evolutionary history of the whole of life. To say that I had cold feet would be to overstate their temperature. I understood well what a lot of work it would be. At the same time, something told me that I needed a major challenge. Having given up the daily round, the common task of a tutorial fellow for the comparative luxury of the Simonyi Professorship, I felt that I owed it to Oxford, and to Dr Simonyi, not to relax but to push myself to a new limit. I think I quoted Yeats, "The fascination of what's difficult . . ." On a subsequent weekend, in an effort to help me resolve my shilly-shallying, Lalla drove over to Paxford alone, to discuss the matter with Anthony further. When she returned full of enthusiasm, I took the plunge and signed the contract. It would take five years to complete and, as it seemed, nearly killed me. Looking back on those five years, I now do not understand how the project was ever finished. Several times I approached the brink of giving up and handing back the (frighteningly generous) advance. I think the only things that kept me going were Lalla's unswerving moral support, and the talented and hard work of my research assistant Yan Wong.

Yan had been my student at New College, one of the best undergraduates I ever had. I suppose he was my grandstudent too, since Alan Grafen supervised his doctoral thesis. Before he had finished his thesis, I hired Yan to work with me on *The Ancestor's Tale.* I can't imagine anybody more suitable. He had the breadth of biological knowledge, the diligence and dedication, the computer expertise, and, not least, the literate wit and good humour to see me through the desponds. Somehow we finished the book, and it has to be the achievement in my life that was the hardest to complete. It is a comprehensive history of life written backwards, in the form of a Chaucerian pilgrimage from the present to the evolutionary past.

Despite Yan's help, I became so embarrassed at the slow progress I was making that in 2002 I offered Anthony Cheetham, and the American publisher Houghton Mifflin, another book, as a sop. This was a collection of essays, mostly previously published but gathered together into some kind of coherent order. It was published in 2003 as *A Devil's Chaplain.* Apart from the title essay, which I wrote specially for it, and some connecting notes mustering the essays into themed sections, no new writing was required, so it could all be assembled quite fast, with the knowledgeable assistance of my Editor, Latha Menon. It distracted me for a while from the big book, and I think it really did enable me to return to that book, refreshed for the task of completing it by 2005.

I suppose that in an autobiographical chapter I should say something about honours. In 2001 I was overjoyed to be elected a Fellow of the Royal Society, the highest accolade my country can bestow on a scientist. As a scientist interested in communicating the love of science, I am also gratified that my first two Honorary Degrees were Doctorates of Literature (rather than Science as all the others have been). The first of these was from St Andrews, which is Britain's third oldest university after Oxford and Cambridge, and one of its most distinguished. It was a great pleasure to me when my daughter Juliet enrolled there to begin her training as a doctor. I have also been fortunate during my career to win some valued prizes, including the 1994 Nakayama Prize for Achievement in Human Science, the 1997 International Cosmos Prize, the 2001 Kistler Prize, and the 2005 Shakespeare Prize for Contributions to British Culture. On one of the expeditions to Japan to accept a prize, Lalla and I were accompanied to the ceremony by the British Ambassador and his wife, Sir John and Lady Boyd, who had become dear friends since we first met them on a previous visit when I reprised the Christmas Lectures in Japan. After the prize giving there was a group photograph, for which we all had to sit in a very neat row. A neat-suited young woman employed by the photographer bustled along the chairs, adjusting our knees to perfect symmetry and our shoes to flawless alignment. When she reached Julia Boyd and Lalla, well-bred politeness struggled but failed to restrain their giggles. The photographer's assistant literally reached inside their skirts to straighten their tights. Gospel truth.

My next book after *The Ancestor's Tale* was *The God Delusion* (2006). It is not so detached from my scientific career as some might think, but I shall say little about it here, nevertheless. It sold more than a million copies in English even before the American paperback was published, and it is being published in more than 30 languages. Its success has prompted me to start my own charitable foundation for the promotion of Reason and Science – actually two sister foundations, one in Britain and one in the USA. Most recently, my latest book

(2008) is *The Oxford Book of Modern Science Writing,* an anthology again assembled with the help of Latha Menon.

That brings me to the present. I have entered my retirement year, but I can't detect in myself any desire to wind down. Quite the contrary. My two charitable foundations, Liz Cornwell's OUT campaign and the rest of the consciousness-raising exercises associated with my website run from California by the incomparable Josh Timonen, seem to have rejuvenated me and filled me with enthusiasm for the future. In March 2007, Liz, Josh and the website regulars secretly conspired to put together a 66th birthday tribute for me (two thirds of a century) to which more than 3000 people from around the world contributed. The previous year, Helena Cronin organized a celebration at the London School of Economics for the 30th anniversary of *The Selfish Gene.* The overflow audience heard speeches by the philosopher Daniel Dennett, the novelist Ian McEwan, and the biologists John Krebs and Matt Ridley. And, again at about the same time, Oxford University Press published a *Festschrift* volume, edited by Alan Grafen and Mark Ridley under the title *Richard Dawkins: How a scientist changed the way we think*, with all new essays specially written for the book by 26 authors: the biologists Andrew Read, John Krebs, Michael Hansell, Marian Stamp Dawkins, David Haig, Alan Grafen, Patrick Bateson, David Barash and Matt Ridley, the historians Helena Cronin, Ullica Segerstråle and Marek Kohn, the physicist David Deutsch, the philosophers Daniel Dennett, A.C.Grayling, Michael Ruse and Kim Sterelny, the psychologists Martin Daly and Margot Wilson, the psychiatrist Randolph Nesse, the publisher and editor Michael Shermer, the memeticist Robert Aunger, the linguist Steven Pinker, the computer scientist Seth Bullock, the novelist Philip Pullman and the theologian Bishop Richard Harries. To be presented with a dedicated essay by any one of these distinguished individuals would have been an honour in itself. To receive 26 essays all bound together, and edited by my two most outstanding pupils . . . well, what can I say? My cup runneth over.

But now, move on, there's work to be done!

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