

Chapter 2

From There to Now, and the Origins of Some Ideas

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From There....

Looking back, it is clear to me now that I had a very privileged childhood. This was not for reasons of wealth or patrimony, but for two reasons of geographical accident. One of these was that I grew up in Tanganyika (the mainland part of modern day Tanzania). The other was that my father had been born in India. These two accidents of history meant that I grew up in an environment that was uniquely steeped in multi-culturalism in a way that would not have happened had neither of these particularities of birth happened.

We had gone to Tanganyika in 1951 from Australia when I was just three because my father, an electrical engineer, accepted a job with the electrical utility company; we left when I was twenty. Unlike most other parts of colonial Africa, Tanganyika was never a British colony, but a mandated territory under the United Nations. Originally a German colony, it had been taken from Germany at the end of the First World War, and Britain had been asked to look after it on the condition that it was not to be colonised (in contrast to neighbouring Kenya) and instead be guided to self-determination (independence). This meant that the European community was always very small – a few German families who had stayed on after 1918, a small British community mainly involved in running the administration in its various forms, or engaged in trading, teaching or missionary work; a scatter of Greeks, Italians, Poles

and Seychellois running garages, bakeries and hotels; and small contingents of Dutch and Swiss, mainly involved in sisal estates. And then, of course, there was a large Indian community who dominated the small businesses, trades and shopkeeping.

For many of us who grew up in the Empire, but especially in places like Tanganyika, there were not that many European, never mind British, children to play with, especially in the smaller up-country towns. As a result, much of our early socialisation was with the children of our servants¹, eating with them, playing with them, absorbing their language, customs and ways of doing things (Fig. 1). Children of this age do not understand the political or racial categories of the adult world, but treat all adults as authority figures and all children as peers. One just becomes part of the cultures – and I use the plural advisedly – within which one is embedded. As a result, I learned Kiswahili pretty much as I learned English, and learned it round the kitchen fire and not from books. I was so deeply immersed in the language that when, as a teenager, I discovered Swahili poetry – Swahili has a written literature dating back to the early eighteenth century that used Arabic poetic forms and Arabic script – I learned Arabic script in order to be able to write Swahili poetry in its traditional formal styles (Fig. 2).

Like English, Swahili is a creole, in this case an admixture of Arabic and the local Bantu language originally spoken on the East African coast. As a result, coastal (or proper) Swahili contains many loan words from Arabic, and Arabic phrases were used constantly in conversation. By the 1950s, this had been supplemented by a large

¹ Lest this be misunderstood, let me observe that servants were not the privilege of the rich in East Africa then or now, any more than they are or were in India (or, indeed, the UK a generation earlier, as even a cursory glance at national census records will demonstrate). Servants were part of the family, and in many cases much cherished in a relationship that was often one of reciprocated loyalty. Perhaps this was because they ‘lived in’, rather than turning up from an agency for an hour once a week to do a quick flit around one’s living room.

number of words from Hindi, albeit mostly in relation to the domestic world. Thus, in learning Swahili, I inadvertently picked up a casual smattering of Arabic and Hindi.

The ability to switch from one language and cultural mindset to another without even thinking about it is probably something that only those who are natively bilingual really appreciate (and they probably don't even think about it). The marvel, looking back on it, is the ease with which one can work in two completely different linguistic worlds that are only partially commensurate without getting confused. Swahili does not mark the genders, for example, yet not only did I not notice this until it was pointed out to me more than 40 years later, but I can safely say that neither I nor anyone else ever made a mistake about gender in a conversation (or, for that matter, failed to distinguish between the two genders in particular cases²). Similarly, Swahili has words that stand for several different concepts (it uses the same word for red and brown, for example), but Swahili speakers are no more likely to make a mistake than English speakers do when hearing the words *pair*, *pare* or *pear*. The fact is that languages are clunky, and all of them have imprecisions of this kind; yet, no one makes mistakes in conversation because what isn't obvious from the context is dealt with by well-rehearsed, simple workarounds. I continue to marvel that anyone ever believed the Sapir-Whorf hypothesis that language affects how we see the world. It doesn't: it merely effects the tools we have to describe that world and the circumlocutions that are sometimes necessary to express its complexities. I suspect the theory's genesis and survival has had everything to do with the fact that neither Sapir nor Whorf, nor the many who extolled their theory, were native speakers of the

² I once heard someone suggest in all seriousness at a seminar that the fact that Kiswahili does not mark gender must inevitably mean that Swahilis were not burdened by gender stereotypes.

languages they studied. It is perhaps a lesson about the perils of doing ethnographic work secondhand.

In the cosmopolitan mixture of northern Tanganyika's small towns, we unavoidably engaged with people from many different African tribes, or *kabila* (literally "type"), and we absorbed in intimate detail their particular languages and cultural characteristics – Swahilis from the coast, Sambaa from the hills, Maasai from the edges of the Maasai steppe that extended over towards the foothills of Kilimanjaro, Zigua from the south with their (to a child) rather terrifying facial scarrings, Dorobo trackers working a long way from home in central Kenya, Somali traders from even further afield in their instantly identifiable sarongs. This early experience spawned a fascination for East African tribal cultures, and later – long before I discovered there was such a discipline as anthropology – I became an enthusiastic purchaser of the RAI's Ethnographic Survey of Africa series (edited by Daryll Forde), a dozen of whose little green volumes I still own.

At the same time, I was deeply immersed in local ways. I learned how to pound maize with the women, how to plant and look after the *mihogo* (cassava), how to climb coconut trees with just a loop of rope around bare feet, how to shred the meat on the ingenious coconut-grater seat used for this all over the Indian Ocean littoral, how to cook *posho* (the stiff porridge-like staple made from ground maize)...and how to kill the chickens we would eat for lunch. I was taught how to make bows and arrows the traditional way by our gardener, how to track, how to move silently through the bush while ever watchful for snakes, how to respect the bush and its wildlife and not take its dangers for granted. In retrospect, it was a rare and privileged upbringing, forged not in books and classrooms, but in the trivia of everyday life, mostly in the dark smoky rooms of people's homes, but also outside in the wild

bushland and forests that surrounded our houses. Not to put too fine a point on it, I did my participant observation the only way it should be done – in the cradle and by the fireside listening to the adults.

The second geographical accident that deeply influenced my upbringing was the fact that my father had been born in northern India and, like me, grew up with the servants' families. As a result, he spoke Hindi before he spoke English, and when very young would speak only Hindi even to his parents (much to their exasperation). Deeply immersed in the cultures of north India, he moved easily in East African Indian society. We visited Indian homes often, over long curry lunches while we children played in and out of the house. Under my father's tutelage, I grew up intimately familiar with the intricate differences between the various cultures from the sub-continent, Hindus and Moslems, Sikhs and Ismailis, Parsees and Goans, in many cases through later personal friendships. From here, I acquired a love of Indian classical music, and especially the amazing *qawwali* music of the sufi tradition of west Asian Islam – for me, second only to the skirl of massed bagpipes as music I have to stop everything for.

During the early 1960s, we lived just above the beaches where the lanteen-rigged trading dhows, or *sambuks*, from Somalia, the Persian Gulf and occasionally as far afield as Gujarat in northwest India were careened for minor repairs and keel cleaning before their return voyages home on the summer's southwest monsoon. Having himself been at sea as a young man, my father liked to pass time with the dhow captains in the cool of the late afternoon, and occasionally be taken out to sea by them. Through this, I came to have a first hand acquaintance with Arab culture and Islam, and its long historical association with East Africa – from five centuries or so of slaving and ivory trading through into the more prosaic twentieth century trade in

pungent dried fish, dates, carpets and kitchenware and their return cargoes of mangrove poles, copra and coir rope. The coastal Swahilis, and Kiswahili with its rich infusion of Arabic loan words and its use of Arabic script, were the product of these many centuries of pre-European exchange.

East African colonial society, just as was the case in colonial India, was far more leaky round its edges than the conventional urban myths, with their all the benefits of third hand knowledge, would now like us to believe. Just as the young Kipling acquired his deep sense of India and Indian culture by spending evenings gossiping round charcoal braziers with the bazaar traders, so we learned how to think and move and have our being in Swahili as well as in the many Indian cultures that permeated East African society. We were multi-cultural and international in our outlook.

Growing up in East Africa had two other important formative influences beyond the purely cultural, though at the time the significance of these passed unnoticed. We lived in close proximity to nature, with monkeys and antelope as frequent visitors to our gardens (Fig. 1) and the wilds of Africa as my playground later on. I saw wildlife at first hand – by the roadside, while tracking elephant on foot through the savannah woodlands of the Maasai steppe, on walks in the forests, and while exploring the extensive bushland around our house. Perhaps fortunately, we lived at a time when wildlife was everywhere abundant, in a world that was remarkably safe with few of the risks that foreign travel now poses.

Last, but perhaps not least, there was a fortuitous early introduction to human evolution. The early 1960s witnessed the first spectacular new discoveries of hominin fossils in Olduvai Gorge and nearby Kenya. To all intents and purposes, modern theories of human evolution really date from this time when the first major East

African fossils rewrote the story of human evolution. Because all this was happening on our doorstep, it attracted a lot of interest and, though I was not to realise it at the time, it laid the foundations of a lifelong interest in human evolution. I gave my first lecture on the subject as a class exercise when I was 14.

So it was that my early life provided me with four key experiences that were formative of my later career. It seems obvious to me now, but being bilingual in a non-Indo-European language, being deeply familiar at first hand with Bantu and non-Bantu, Arab and Indian, not to mention different European, sub-cultures, attuned to the natural history of Africa and the rapidly developing story of human evolution all combined to guide my future intellectual interests. It was inevitable that whatever direction I happened to take in later life, it would always be influenced by this background.

....To Now

As it happened, my early academic interests were as far away from anthropology and natural history as it is possible to be. Having concentrated on history and English as my main secondary school subjects, I planned to study philosophy at university. It was the only subject I was passionate about. As luck would have it, I gained a place at Oxford. I say “as luck would have it” because, unlike all the other universities I had applied to (at least one of which conveniently rejected me), Oxford did not (and still does not) allow you to study philosophy on its own. I chose what I considered to be the least bad option of doing a joint degree in philosophy and psychology (with very little idea as to what psychology actually was). I have never ceased to marvel that had I gone to any other of the universities I had

applied to, I would have ended up doing a degree in pure philosophy, and I would never have had the career I did. In fact, I am convinced that I would have disappeared without trace. As I see it, psychology rescued me and, directly or indirectly, made possible everything that was to follow. It introduced me to the sciences, a discipline that I had done my best to avoid at school, taught me not to fear mathematics and opened up intellectual horizons that I didn't even know existed. It also introduced me to animal behaviour, because quite fortuitously the first year psychology course, gloriously misnamed "Comparative Psychology", was taught by the zoologist Niko Tinbergen, later to earn the Nobel Prize for his seminal work in developing the discipline of ethology. As I recall, there was precious little psychology, and a great deal of animal behaviour, in his lectures. Having been intimately familiar, albeit at arms' length, with wildlife, it sparked an interest that might otherwise never have happened.

Later, undergraduate summer expeditions to Ethiopia and Senegal introduced me to the newly developing field of wildlife research, and in particular primatology. It persuaded me to do a PhD in primate behaviour at the University of Bristol. Although based in the Psychology Department, the group I worked with consisted almost entirely of zoologists and I quickly acquired a zoologist's eye by osmosis. Two lengthy periods of PhD field work in the remote Simen Mountains of northern Ethiopia introduced me to fascinating new cultures, notably the Coptic Amhara and the once nomadic Moslem Galla, as well as to a host of unusual new mammals and birds. While the focus of this work was the social behaviour of the endemic gelada baboon, found only in the Ethiopian highlands, I was drawn into studying colobus monkeys, the Walia ibex and the klipspringer antelope simply because they were there. Later, during the 1980s, at Cambridge University's Department of Zoology, this

experience spawned two major field studies of ungulates – klipspringer in Kenya (1980-1982) and feral goats on the Isle of Rum off the west coast of Scotland (a project that ran on an on/off basis from 1980 until 2006). The many hours spent watching klipspringer (perhaps the most intensely monogamous of all the antelopes) were instrumental in my later interest in the evolution of monogamy.

In retrospect, I now realise that my early, long immersion in a multi-cultural environment, and especially the fact that I was, by all criteria, dual-cultural in Kiswahili and English, played a crucial role here. The ability to switch mindsets from one culture to another, to be genuinely part of a community and not an outsider peering in, naturally enabled me to do the same with the animals I was watching – not so much to become part of their community as such, but to be able to sit inside their heads and see the world as they saw it. Without thinking, I was naturally doing what the classical ethologists – Tinbergen, Lorenz and Frisch, who shared the Noble Prize for Medicine for this – insisted was essential to good research on animals: you need to just sit and watch until you can see the world from their point of view. Too much comparative work on animals, and probably humans, is currently being done by researchers who have never actually seen the species concerned in the wild, and the result has been some astonishingly naïve claims.

Between them, these early years in my academic career immersed me in two disciplines that had very different ways of thinking about the world – one very mechanistic (i.e. psychology), the other more functional (i.e. zoology) within a deep evolutionary framework. Tinbergen's *Four Why's* (that a biologist asking why something is the case might be asking any one for four very different, but complementary, questions – mechanism, function, development or history) came to symbolise for me the fact that most disagreements between disciplines come down to

the fact that they are usually asking different questions – in short, neither has bothered to do their participant observation properly so as to find out about each others' cultures. I learned how to slide effortlessly from one supposedly incompatible discipline to another, much, no doubt, as I slipped unthinkingly from Swahili to English and back. My exposure to philosophy played a seminal role in this, because the one important (perhaps only) lesson I learned from this august discipline was to ignore other people's disciplinary boundaries so as to draw the links between them and ask how each illuminates the other.

The 1980s witnessed a crisis both in academic jobs and in funding for large animal research. After a brief stint at the University of Stockholm's Institute of Zoology and several years as a freelance science journalist working for *New Scientist* and other magazines, I found myself in the Anthropology Department at University College London. The seven years I spent at UCL were seminal in the development of my thinking. Here, I became seriously immersed, for the first time, in biological anthropology and human evolution, and at the same time acquired a familiarity with social and cultural anthropology. The result was the four main ideas that I would consider my intellectual contribution: the time budget models of primate group size, the gossip hypothesis for language evolution, the formal version of the social brain hypothesis and Dunbar's Number. (I explore these in more detail in my other chapter in this volume, based on my 2015 Huxley Lecture; more extensive treatments can be found in the list of principal publications appended to the end of this chapter.) All of four of these ideas were developed within a 12-month period, and all were first published while I was at UCL. One might say that I have spent the subsequent three decades of my research career simply dotting the i's and crossing the t's of these UCL-derived ideas.

Fortuitously, the most important trigger for this by far was the lack of funding for field work on animals during the late 1980s. Unable to continue with the primate and ungulate research that had been my all-consuming interest between the late 1960s and mid-1980s, I decided the best thing to do was to switch to humans and ask the same kinds of questions as I had done on monkeys. Humans had the advantage of being there, on the street, and one didn't need money to study them. The result was a series of studies on mate choice strategies (largely based on analyses of newspaper personal advertisements), parental investment strategies (mainly based on analyses of the Krummhörn historical demography database in collaboration with Eckart Voland at Giessen University), vigilance behaviour and conversational behaviour (the latter two done in and around UCL and nearby public parks).

Of the four major outcomes that emerged from this period at UCL, all except the first were completely serendipitous. The time budget models had long been at the back of my mind, and grew directly out of the gelada fieldwork, especially the series of comparative analyses carried out with Japanese primatologists from the Japan Primate Centre who had also studied gelada. It had become obvious in the course of writing these collaborative papers that much of the variability in gelada behaviour could be explained by relatively simple ecological differences between the various habitats where we had, between us, studied this species. In many ways, the gelada were a lucky break, because their ecology is very simple, and so even with a very small number of study sites the relationships between behaviour, ecology, demography and habitat were glaringly obvious and could easily be quantified in equations. The time budget models that grew out of this aimed at explaining why group sizes for individual species varied across the habitats they occupied. In the years I had spent sitting watching gelada and various other monkeys in East and West

Africa, I had come to the conclusion that it was not nutrients as such that limited the size of groups a species could have in any given location (as conventional ecological theory would generally assume), but rather time – the time required to meet their various ecological and social requirements.

I began to think in terms of the constraints that time imposed on animals' abilities to forage together in a group. The principle was simple: if we could define equations that defined the main time budget components (feeding, moving, and resting) in terms of environmental (specifically climatic) variables, then we would be able to predict the maximum size of group that a species could support in a given habitat because there was a very simple relationship between social group size and the time a species could afford to devote to social grooming. Whatever time they had left over after satisfying their ecological needs set a limit on the size of group they could have because, in effect, each individual added to the group required a very specific amount of additional social time to be devoted to it in order to ensure that it was bound into the group. The merit of this approach was that we could always define climatic conditions for any point on the map. Had the models been conceived in terms of vegetation or food availability, this would not have been possible: maps of vegetation on a fine enough scale are not available. But climate determines vegetation and, as the ultimate driver of things ecological, is much more general – and databases are available on a continental scale. Despite their conceptual and empirical simplicity, these models turned out to be at least as good as more conventional biogeographic models, and sometimes better, at predicting where species could live and where they couldn't. More importantly, in addition, they told us exactly why the species couldn't live where it didn't, and what sizes of groups it was limited to in those locations where it could live – something that no conventional ecological models can tell us.

The other three ideas all emerged together from what, at the time, I thought was a rather trivial research question. Somewhat irritated by continuing claims that social grooming in primates is solely hygienic in function, it occurred to me that we could settle the issue by testing directly between the two main competing explanations – that grooming is purely hygienic in function versus the claim that, while grooming no doubt began for hygienic reasons (and certainly still has that useful function), its main function had been co-opted to enable social bonding. I thought we could test between the two hypotheses by determining whether the average amount of time different species devoted to social grooming correlated best with group size (an index of social bonding demand) or with body size (an index of how much fur needed cleaning). Grooming turned out to correlate with group size, and not at all with body size. Perhaps because of my background in psychology, it then occurred to me that both of these should also correlate with brain size if the recently proposed Machiavellian Intelligence Hypothesis (the claim that primates have large brains because they live in unusually complex societies) was true. Cognition should underwrite, and ultimately limit, the number of social relationships animals can maintain, and cognition in turn is ultimately dependent on the size of brain you have.

Armed with this justification, I dug out data on brain sizes in primates and plugged them into my analyses. Slightly to my surprise, there was indeed a relationship between group size and brain size (or, to be more precise, neocortex size – the neocortex being the part of the brain that had evolved out of all proportion during the course of primate evolution). This finding effectively confirmed the social brain hypothesis (essentially an alternative, less politicised, version of the Machiavellian intelligence hypothesis, but one that focussed explicitly on social bonding rather than social exploitation). The paper reporting the brain size/group size

relationship was initially rejected by the journal I submitted it to, having been dismissed as idle speculation by both of the referees who reviewed it. For the first (but now by no means the last) time in my life, I was sufficiently scandalised to challenge the editor's decision. I was allowed to submit a revised version of the paper, and I am happy to report that it was subsequently published. With hindsight, I suppose it would have been quite embarrassing to have rejected what later turned out to be a foundational paper, so I like to think I did the journal a favour. It is the third most cited of all my papers, having been cited more than 1350 times in the quarter century since it was published.

Simple as this social brain result seemingly was, it raised a number of important questions. One obvious one was where humans might fit in. As far as I was concerned, this was simply a matter of slotting human brain size into the equation for monkeys and apes and seeing what group size it predicted. The predicted value was about 150, the value now known as Dunbar's Number. Actually, this was a lucky mistake, because I used a generic equation based on all primates combined; later, I realised that the social brain relationship actually consists of a number of socio-cognitive grades, and, since we are members of the ape family, I should have used only an equation for apes. As luck would have it, for obscure statistical reasons, the answer turns out to be the same either way – in this particular case. It could very easily have been otherwise.

The obvious question was whether or not this value had reality as a natural group size in humans. It seemed to me at the time that the only appropriate place to look was small scale societies, since, as a species, we had spent almost all of our history in these kinds of societies, and our psychology would have been designed by evolution to be optimal for these societies. But what was the appropriate grouping to

look at? I was not even sure there were any data on group sizes in small scale societies, never mind what type of group was the right one. My solution was a reverse engineering one: is *any* kind of human group at all whose size corresponds to this value.

After several weeks locked in the ethnographic section of the UCL library, I had a list of small scale societies for which ethnographers had provided census data on grouping levels of different kinds. A simple statistical analysis revealed that only one of the four types of grouping found in these societies was even close to the predicted value of 150. This was the hunter-gatherer grouping level known as the community (in the South African Bushman literature) or clan (in the Australian Aboriginal literature). I have spent much of the quarter century since then demonstrating that this figure reappears in many different examples of human social organisation as well as in personal social networks, online as well as offline.

The important lesson here was that I had ignored a piece of statistical advice – that one should never extrapolate beyond the range of the data you have. This is frowned on because you don't know what shape the distribution of your data might have in the regions of the graph where you lack data. Had I listened to this advice, I would never have made the projection to modern humans, and Dunbar's Number would never have been discovered. I hesitate to say it, but the moral of this story very clearly is: ignore most of what statisticians advise. That said, I might observe that, because of the very sound grounding in statistical theory that my psychology degree provided, I like to think that when I ignore statisticians' well meaning advice, I do it not from ignorance but from a position of informed consent: I do understand what the statistics are doing and what their limitations are, and that element of immersion in the discipline is important.

The time budgets models, and the grooming equation in particular, raised a further important issue. If primates used grooming to bond their social groups, and no primate society ever devotes more than 20% of its total day to social grooming, then how on earth do humans bond their social groups or communities, given that the primate grooming equation predicts that we would need to devote around 45% of our day to social grooming if we are to bond our communities of 150 the same way that primates do. It occurred to me that language might fill the gap, because language allows us to use our social time more efficiently than monkeys and apes can do when grooming. Grooming is a strictly one-on-one activity, and indeed still is with us even in the adapted versions of it that we commonly use in the form of stroking and patting. By using a vocal channel, we might be able to cut through this particular glass ceiling by “grooming” with several individuals simultaneously. By ‘grooming’ with more individuals at the same time than monkeys and apes can, we would be able to increase the size of social group we could bond.

Initially, I had conceived this simply in terms of time spent with someone. By talking with a group of people, I declare my commitment to all the relationships involved: I would rather be standing here passing the time of day with all of you than talking to that lot over there – in effect, a statement of how much I value my relationship with you. The content of the conversation is irrelevant. A casual remark by Dick Byrne at a conference in Belgium when I was explaining this to him gave rise to the gossip theory: it was he that suggested that it is what we talked about that might be important. It prompted me to initiate a series of studies of natural conversations to see what we actually did talk about, and how many people we did it with. This revealed that conversation groups have a very limited size (rarely more than four people) and conversations are mostly concerned with social topics. I have had many

people doubt that conversation groups are so limited in size, asserting confidently that they regularly hold conversations with many more than four people at the same time. Interestingly, everyone I know who has made this claim is the kind of person who is very fond of pontificating: their conversations are more like lectures at which the rest of us have to maintain a respectful silence. In our extensive sampling of natural conversations, we have yet to find a truly interactive conversation that involved more than four people.

Subsequently, I have come to modify the gossip hypothesis in two important respects. One derived from the finding that primate social bonding involves the endorphin system, and it is this that is triggered by social grooming. In fact, mammals have a special nerve system, the afferent c-tactile system, that runs from the hairy skin directly to the brain that seems to be specifically designed to underpin this: these nerves are unmyelinated, slow, one-way (there is no return motor loop) and respond to only one specific stimulus (slow stroking). They trigger the release of endorphins in the brain and create the sense of warmth, comfort and contentedness associated with close friendship.

This is all very interesting, but it creates a problem: if social bonding is associated with activation of the endorphin system, then how does language – or at least conversation – trigger the endorphin system? Something seems to be missing. Over the next two decades, I gradually came to realise that, during the course of human evolution, we had adopted and adapted a variety of social behaviours that turn out to do exactly this. These behaviours include laughter, singing, dancing, storytelling and the rituals of religion. In all likelihood, they were acquired one by one as our changing ecology made it necessary to break through a series of glass ceilings on the size of our social groups.

The second modification was the eventual realisation (and it took the better part of two decades to materialise) that what was important about language and conversation was that it allowed us to tell stories. Language allows us to tell fictional and factual tales of how the world is, how it might have been in the past or might be in the future, in a way that creates a sense of community. It allows us to create and, more importantly, pass on to others a worldview – a kind of verbal totem pole standing at the centre of the community. It is what allows us to create and maintain very large communities indeed. And so culture was born. That realisation has spawned a whole new research programme in collaboration with classicists and English literature scholars in an attempt to understand some of the psychological processes that underpin our enjoyment of drama and storytelling.

The quarter century that has passed since discovering these four classic findings has largely been concerned with providing the evidence to support them, while welding them into a single, coherent theory of primate and human social evolution. Much of this work has been done by graduate students, postdocs and research fellows working with me, through funding provided by the British Academy, the Leverhulme Trust, the Engineering and Physical Sciences Research Council (EPSRC), various European Union research grants and the European Research Council. Much of the credit for working out the details of these ideas lies with the individuals concerned; my part has been that of the storyteller weaving these disparate strands into a single, coherent tale.

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Looking back on all this, it becomes obvious to me how the various threads that were first sewn in East Africa a lifetime ago have gradually intertwined and come together. They provided a framework of interests that has, over and over again, made

me ask the same set of questions: why do these animals/people do what they do (or think what they think), and what makes it possible for them to do that? And how did they get from then (in some remote evolutionary pre-existence) to now? The road I have travelled in doing this has been exciting, challenging and often frustrating. It has required patient observation, deep thought, and the need to learn new and challenging skills – how to observe animals, how to design experiments, neuroimaging technology, genetics, statistical methods, computer programming and various branches of mathematics including probability theory, geometry and graph theory. With a philosopher's blissful disdain for the problems that lack of knowledge and intellectual naivety might create, I simply did what needed to be done to follow the long and winding road from one question to the next. I think it paid off. But one thing I can definitely say: it has been a lot of fun.

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Fig. 1

Above With Kidogo (“The Little One”), the youngest son of our cook, under the watchful eye of his big sister, Sali.

Below Early primatological experience.
Tanga, June 1950.

هَآپْ تَانْغْ

نِگْمَشِيكَ بِيْبِ يَانْغْ	هَآپْ مَكْنُونْ
نِلْكُوتَ هَآپْ تَانْغْ	نِگْمَفَوَاتَ سِهِيْمُ زُوتْ
أُوسِ بِيْغِ مِيْمِ سَانَ	نِگْمُوبِيْ مَوِيْ يَانْغْ
أُجُو نِكْبِيْنْدَ يَانْغْ	نِكْبِيْنْدَ سَانَ سَانَ
أَكْتَآيِ مِيْمِ سَانَ	نِگْمُوبِيْ تَوِيْنْدَ زِيْتْ
أُولِ بِيْبِ مَوُوبِ يَانْغْ	أَكْمَاچَ مِيْنِ هَآپْ
أَتُوْنِ جَانْجَ يَاكْ	ج سِكْلِيْزَ هِيْفِ بِيْبِ
سَوُوتَ هِيْسِ يَانْغْ	زَوَادِ هَيْنِ مَوْجَ

Fig. 2

My Swahili poem *Hapo Tanga* ["There in Tanga"] (1970)

In the C17th, Arabic script was adapted for use in Swahili (mainly for letters and the local forms of Arabic-style poetry or *utenzi*). Swahili has a number of consonants that do not occur in Arabic, and writers usually adapted Arabic letters (or occasionally even invented new ones) to fill the gaps; more confusingly, they sometimes just used an existing Arabic letter to represent several different sounds in Swahili. The script used here is that recommended by Omar YA & Frankl PJL (1997: An historical review of the Arabic rendering of Swahili, together with proposals for the development of a Swahili writing system in Arabic script. *Journal of the Royal Asiatic Society*, Series 3, 7: 55-71) as implemented in the software *Andika* (www.andika.com). The original poem was written in an older script.