Chapter 16
My Gunnar Olsson

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I first heard Gunnar Olsson speak at the Department of Geography, University College London (UCL), as a third year undergraduate in the spring of 1978. In my last year, I would sneak into Departmental seminars supposedly reserved for staff and post-graduates. I remember seeing Ray Pahl and Alan Baker present, both of whom I read and were excited about, but who in seminar mode were rather dull and dry. Then just before Finals, I saw that Olsson was speaking. I knew I should study for my exams, but Olsson already interested me. Earlier in the same year in a philosophy and methods seminar, a post-graduate student dressed up as a bus conductor declaimed to the class selected passages from a dog-eared copy of Olsson's (1975) *Birds in Egg*. He said the passages proved the end of causality. I wasn't so sure, but I wanted more, and which is what I got that spring day when Olsson came to UCL.

Olsson was in the process of making a slow farewell tour back to Sweden to take up a position at Nordplan, Stockholm. For the previous ten years he was in America teaching at the University of Michigan, Ann Arbor. Alan Gilbert introduced him at UCL. Olsson was tall and lean, with shaggy long blonde hair and a beard. He looked like a Viking. As soon as he spoke he instantly commanded the room. There was nothing dull and dry about him. With no notes, he paced up and down at the front of the stage like some caged animal, acting out his paper's argument. His dramatic portrayal of the "hermeneutic moment" was a *tour de force*. Not that I could always understand what he said. His deep voice, Swedish accent, and tendency to laugh at his own jokes before he finished telling them, meant I missed a good quarter of what he said. But then I didn't understand Marlon Brando either, and I thought he was great. The physical performance and the words of Olsson's I did catch were enough anyway. It wasn't an argument for the end of causality, but an argument for being clear about ambiguity. That was what all the pacing between one side of the room and the other was about, along with extravagant hand gestures, face grimaces and piercing staring eyes. He was performing the various tensions in life itself that produced ambivalence, and which lay for him at the very centre of the human condition. These were the tensions between thought and action, fixity and change, form and process, certainty and doubt, determinateness and chance, and even between the equal sign and the slash of the dialectic. As he says in the interview below, he gave the best years of his life to understanding these tensions. And you could tell on that day

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watching him. It was incandescently brilliant. Even the man dressed up as a bus conductor declaiming *Birds in Egg* dimmed in comparison.

Later that same year I left for graduate school in America. But the exposure to Olsson haunted me. My graduate supervisor at the University of Minnesota, Eric Sheppard, had on his bookshelf Michigan Geographical Publication, number 15, *Birds in Egg* (and which predictably had not been available at the University of London library). I immediately grabbed it, a giant door-stopper of a book, taking it in my first year and not returning it until five years later when I left for the University of British Columbia to take up my first academic job. I must have read it two or three times. I continuously went back to it to steal quotes and to check subtle philosophical points. I admired as much the writing in the book as its substantive content. There are not many brilliantly written books in Anglo-American human geography. Bill Bunge's (1971) *Fitzgerald* is one, and in its quiet elegance, Peter Haggett's (1965) *Locational analysis in human geography* is another. And Olsson's *Birds in Egg* is a third. The prose is shockingly good: funny, rich, learned, profound, purposeful. When I later spoke to Olsson about writing *Birds in Egg*, he said that precisely because he was not a native speaker of English he felt he could take chances, saying things that he would never dare say in Swedish. Like Joseph Conrad, another non-native writer of English language prose, Olsson produced in *Birds in egg* matchless writing, technically flawless, but not quite English. It was the "not quite English" that made it so compelling.

I interviewed Olsson because I was doing a project collecting oral histories of pioneers involved in geography's quantitative revolution. Of course, I had secretly wanted to speak to him for years. But I never had the temerity to ask. My project provided the perfect entrée, however. We agreed to meet at his Pittsburgh hotel room at the 2000 annual meeting of the Association of American Geographers.

Olsson, like David Harvey, had been a pivotal figure in geography's quantitative revolution. He had begun as a believer, but within a few years became one of its most trenchant critics. His early qualifications as a revolutionary were impeccable. As an undergraduate, as he says in the interview, he was captivated by the teachings of a young geography professor at the University of Uppsala who used "regression models," and which for Olsson were "beautiful," "open[ing] up a world." In 1961–62 as a graduate student he met and befriended David Harvey, and already beginning to gather ideas for writing what would be the philosophical bible of the quantitative revolution, *Explanation in geography* (Harvey 1969). During 1963–1964 he attended one of the temples of North American spatial science, Walter Isard's Regional Science Department at Penn. As a result of that visit, Olsson (1965) wrote the now classic interpretive review of one of the key theoretical and empirical contributions of geography's quantitative revolution, spatial interaction models. As a result of these impeccable qualifications, he was offered a job at the illustrious Department of Geography, University of Michigan. Among its faculty were John Nystuen and Waldo Tobler, two of the original "space cadets," the name given to the graduate students at the University of Washington who in the late 1950s spearheaded the quantitative revolution.

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Olsson's experiences and credentials were unimpeachable. He should have been a dye-in-the-wool quantifier. But he wasn't. From almost the beginning of his Michigan appointment the worm turned. In the courses he taught such as the one he talks about in the interview about "Thought and action," in the graduate students he supervised and who included Bonnie Barton, Stephen Gale, and Michael Watts, and in his Ann Arbor living room "Salon" (as John Hudson calls it), he struck out in a different direction. It was away from certainty, fixture, determinateness, and absolutes to the treacherous shoals of ambivalence, mutation, chance, and relationality. His guides were no longer that young mathematically inclined Uppsala professor, Walter Isard, or the former "space cadets," but philosophers and artists who had deliberately turned their cheek to feel the full gale of contradictions and uncertainties of being human, and who included Samuel Beckett, James Joyce, Søren Kierkegaard, Ludwig Wittgenstein, and the writers of the Bible and Greek epic poems.

Not that Olsson stopped being a geographer. Initially the new Olsson met the old Olsson with spatial interaction models getting the *Birds in Egg* treatment (and becoming *Eggs in Bird*; Olsson 1980). Later the geography took the form of a discussion of *Lines of Power* (Olsson 1991). The interview below, however, is confined to Olsson's early years and the period leading up to that moment in spring 1978 when Gunnar Olsson changed my life.

Interview with Gunnar Olsson

Pittsburgh, PA, 7 April, 2000

TB: I wondered if you could begin by talking about your early life.

GO: My very early life? As a child?

TB: If you think it is relevant.

GO: Most of what I have to say about that is in my contribution to *Geographical Voices*, the wonderful collection of autobiographical essays that Peter Gould and Woody Pitts put together.

I was born in 1935 and grew up a small town in a rather remote area of western Sweden. My mother was a midwife and my father owned a sawmill and a factory where he made doors and windows. They were a little older when they got married, my father 40 when I was born. From what I can judge I was a very wanted, loved child with no real problems. As a consequence I have never had any desire to reject my parents, and from what I can tell I had a very secure and safe childhood. I felt as if I could do whatever I wanted, and I was encouraged to do so.

When you are living in a small community of that type, everybody of course knows everybody else. Add to this my mother's job and it is clear that a large number of people were passing through our house, because in every family, rich and poor, there were babies being born. My father was very intelligent but he had only five years of school – every other day. He was clearly very bright and in his own fashion surprisingly well read. This was in no way an intellectual home, but it

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was very open and it provided a sense of security for which I am eternally grateful. This well-ordered but permitting atmosphere has no doubt had an impact on my later development, for if you risk everything when you go off, it is natural that you become cautious and on your guard. So, even though I am well aware that to others my work has sometimes seemed strange, perhaps even dangerous, this is not at all how it has appeared to me. The truth is, in fact, that I have yet to meet a person of whom I am genuinely afraid. It has taken me a long time to realize how unusual this feeling actually is.

As I remember it, I was punished only once, and on that occasion for doing something utterly stupid. My father did not smoke, but he kept a box of cigars for his business friends. Now, together with two of my friends, I stole several of them, not merely one but enough for my father to notice. He did and I got a (light) spanking. To this day I am uncertain whether he did it because I had stolen or because I had been careless enough to be caught.

I have subsequently acquired a number of friends in southern Europe – Italy and Spain in particular – and it has taken me a long time to realise how different cultures are using slightly different socialization techniques for making us predictable and obedient, the sense of shame reaching much deeper into them than into me; myself I rarely even notice if I happen to do or say anything unusual. And if I actually do notice, I almost certainly would not feel ashamed. But some of my friends actually do and that is a feeling which has taken me a long time to understand. It would be strange if this personally trait were not somehow related to the type of creativity that has formed my work.

TB: So a secure, comfortable home life.

GO: Yes, and in the creation of that home life my father played a great role. Growing up on a small farm, and after a tough period first in the States and then in Canada, he returned to Sweden in the mid-1920s eventually earning enough to make him comfortable. As a sign of his determination I was only three months old when he bought a tract of forest land that he registered not under his own name, but under mine. It is still in my possession and even though it is far from enough to live on, it has sometimes made a difference. Most likely it has played a role in my decision never to apply for research money not, of course, because I would not have enjoyed the cash, but because it never crossed my mind to ask for the tacit permission. Why would professors be given tenure, if it were not because they were judged competent enough to decide by themselves what is worthwhile research? In the traditional Swedish system it was in fact only professors and judges that the constitution forbade the King to fire, although that of course sometimes happened anyway. The idea was nevertheless clear: even the sovereign needs servants who are sufficiently independent to obey by the law (juridical or scientific) rather than the dictates of the ruler. Money and freedom is indeed a multifaceted relation and in my mind the price for the Faustian pact is regularly too high to pay. Once that has been said, though, I admit that in my type of non-empirical research there is less need for money than for peace and quiet.

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I know David Harvey quite well and when last January he came to Uppsala to receive an honorary degree, we talked about these issues quite a lot. I think he and I are the only one geographers of any repute who have never asked for funding. In turn this means that even though I have worked very closely with a number of students, none of them has had to rely on me for their bread and butter. I realize that this view of the students' independence may be outdated, but I feel so strongly about it that I will not give it up. Mentor yes; slave-owner no. But in today's Sweden, all doctoral students are formally appointed as state employees. I feel very sorry for them, for how can they under these circumstances ever grow into independent thinkers.

TB: What were you like as a student?

GO: First, of course, I went to the primary school, and that was in the little village where grew up. For secondary school I had to take the regular bus, 40 kilometres there and 40 kilometres back, every day for four years. A formative experience. Then I switched to the gymnasium for another three years. Too far away to travel daily, so my parents rented me a room from an old lady, a friend of the family. This meant that from the age of seventeen I have had to look after myself.

TB: How old were you at the gymnasium?

GO: When I began I was seventeen and when I graduated I was turning twenty.

TB: Was it there that you wanted to be a geographer?

GO: I never wanted to be a geographer. Of course, like other children I liked the atlases, imagining the cold of the North Pole and the heat at the equator. But I was generally speaking good at school, the first years in a new system average, at the end always at the top.

Geography was always one of my best subjects, but I was actually more interested in history, literature and political science. And in the gymnasium we had a seemingly bad geography teacher – bad in the sense that he could not keep order in the class. As it turned out, though, he was a dedicated and quite competent researcher, who was talking about ideas, von Thünen included. Very beautiful and very simple.

When I later came to the university, where you had to take courses in a combination of disciplines to qualify as a teacher, I chose history and geography. Since I was much more interested in history I decided to start with geography, simply to get it out of the way. The studies were essentially free with very few lectures and hardly any seminars. You signed up for an exam once a semester and how you acquired whatever knowledge you acquired was really none's business but your own.

TB: Which university did you go to?

GO: The University of Uppsala, which was were most students from my province went.

TB: That was about 1955?

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GO: No, it was in 1957, two years after my graduation from the gymnasium. These two years in between turned out to be very important and I would like to say something about them.

At that time every twenty-year old male Swede had to do military service, a duty no one could avoid. A few weeks after graduating from the gymnasium I therefore found myself in the barracks, a place I quickly realized was not for me. Since everyone knew that students of medicine, dentistry and veterinary medicine (the army still had horses) could do the military training as part of their regular studies, the strategy gave itself. I wanted so very much to get out, but to become a dentist – I couldn't imagine anything duller. And sticking an operating knife into a human body was really not an alternative either. Veterinary medicine! An application was submitted and ten days later I was free.

Come fall I went to Stockholm to begin my new career. The second day there I biked to the school where I was met by a most enthusiastic assistant, who took me to an operating room where they were performing a caesarean on a horse. I knew from my mother how dangerous that operation was, but I had never really imagined that it could be done on a horse hanging in hooks from the ceiling and by vets with rubber boots on their feet. More blood than I had ever seen. And such was the end of my life as a veterinary.

In retrospect I often think of that summer of 1955 as being exceptionally important. The reason is that this was one of those rare moments in my life when I have done something on purpose, when I have manipulated myself, when I purposely have tried to reach a well-defined goal. Not letting life just happen, but consciously intervening. In the process I learned a lot about myself, a lesson which I now understand has had a profound impact on my attitude to politics, *the* prime example of intentional and institutionalised action.

Once this had happened, all universities had already started and it was too late to do anything about it. In addition I knew that no matter how much I disliked it, I would eventually have to complete the eighteen months of military training. To pass the time, I got myself a job as a teacher in a nearby high-school, as it turned out a life-changing experience. Why? Because in the graduating class was Birgitta, that cute little girl to whom I am still married. Of course there was a minor scandal, but what do you do when you are twenty-one.

TB: Then you went to Uppsala.

GO: Yes, and I started reading geography because I wanted to get it out of the way. But then the fantastic happened that one of our teachers was a young guy who had just finished an unusual thesis on migration, a study in which he had experimented with a set of regression models, essentially a social gravity model. To me this was extraordinarily fascinating, partly because the formulations were so beautiful, mostly because I didn't really understand what was going on. The challenge was enormous and I was convinced that here lay the frontier of the social sciences, indeed of knowledge in general. The point was to translate observed reality into the language of precise and non-ambiguous language, for only in that way could knowledge be accumulated. Already at that stage there was a streak of

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minimalism, the feeling that empirical data can be condensed into a small set of parameters. A new world was opened up, a world which in a sense I have never left; whenever I don't understand what is going on, I automatically reformulate the problem into a regression model with dependent and independent variables.

TB: Did you have much training in mathematics?

GO: Yes and no. In the gymnasium you had to concentrate either on language or on mathematics, and I chose the latter. However, I was never especially good at it and even though I think I could understand what was going on, I would never have been able to do mathematics creatively. Certainly nothing compared to what I have done in ordinary language.

TB: Did you think about these things philosophically at all?

GO: Quite early actually, because in the very beginning of my graduate work, probably in 1961, I took a seminar on causal models from Herman Wold, the professor of statistics and a leading econometrician. This course had a lasting impact partly because it was technically quite sophisticated and partly because it was firmly grounded in philosophy. To be minimalistic, the entire course was about the coordinates-net in which we capture the world, especially about the naming of the axes; not merely the singling out of dependent and independent variables, but the fixing of the fix-point (the origo) on which everything is hung. I think this is a key to understanding almost everything I have subsequently come to do. Of course I did not know that at the time, but Wold's seminar – its content at the very edge of my comprehension – was no doubt more formative than any specifically geographic study I ever did.

TB: Naming the axis and then realising that by choosing the fixed point you determine the world you see.

GO: Yes. Yet – and perhaps even more importantly – the very point of the causal models is not merely to describe the world but to provide a means for changing it. It follows that lodged in the axes of the regression models is the key problem of thought and action, that is the relation between description and prediction. In Swedish geography of the 1960s this was indeed a central issue, for in the social engineering of that era the social gravity model was perhaps the most important tool for rebuilding society, by extension for making the country a better place to live. That said it should also be said that I remain very critical of the geographers who were active in that work, Torsten Hägerstrand and Sven Godlund most prominent among them. Once again it is a question of ethics, for I will certainly demand of those who deliberately set out to change the world that they should have a clear view of the consequences of their actions. In this case they did not. To this day I remain uncertain whether they acted as they did because they were tempted by the rewards of power or because they were methodologically naïve. Perhaps it was just the moods of the time. The tone of the voice gives me away, just as it always does when I witness how integrity is being sacrificed on the altar of political correctness.

TB: I think that was the first article I read of yours – spatial engineering.

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GO: How do I know, except to say that these issues have been with me for a very long time. And when in 1977 (after the eleven years at the University of Michigan) I accepted the job at Nordplan, my hope was to examine the relations of thought and action in greater detail.

TB: Were you aware of this that early – in 1961 and 1962.

GO: Probably not in 1961, but most definitely in 1965.

TB: Is Hägerstrand at Uppsala now?

GO: No. Hägerstrand has always been at Lund. Sven Godlund did his doctorate there as well, even though he later was appointed to the chair at Gothenburg. This means that both of them were members of the Lund school, which was decisively influenced by Edgar Kant, who after the war came to Sweden as a refugee from Estonia, where he had done a fair bit of Christaller-type work.

TB: Who were influential for you at Uppsala?

GO: In some ways the question is wrongly posed, because throughout my development hardly anyone has been strong enough to change me. Yet I have already mentioned Herman Wold and Esse Lövgren, the young man with the regressions. These two made an impact, almost certainly because they were so challenging. Gerd Enequist – formally my professor at Uppsala – was a historical geographer, who didn't have anything intellectually to do with my work, but who was curious and open enough to give me all the support I needed and never had to ask for. Like my parents she trusted me to do whatever I pleased and for that I remain incredibly grateful. None of them ever interfered, perhaps because they somehow knew that telling me what to do would never have worked. And that is despite my very Swedish non-aggression complex, an evasiveness which I have learned can be irritating.

TB: Even at this early stage, there is almost an ambivalence you have towards statistics, or mathematics. That seems to me still true.

GO: It is the aesthetics rather than the formalism itself that I find so appealing. And in my particular brand of aesthetics, minimalism plays a pivotal role. Here, again, I have been much influenced by the gravity model in which everything worthwhile is condensed into the points at which the line crosses the axes; the slope of the line, the angle. Since I am blessed with a rather poor memory I have to condense the world into geometric patterns which I can see with my eyes and grasp with my hands.

TB: You said you were thinking of going into history?

GO: I did do history. But when I did I had already been sucked into the geography department, where I was more attracted by the parties and the social atmosphere than by the intellectual questions. To tell the truth it is only recently that I have become seriously interested in geography.

TB: Was there much on the mathematical/statistical side?

GO: Nothing at all. Except in 1961–62 Julian Wolpert visited the Uppsala department and we were spending a lot of time together. As it turns out he too attended Wold's seminar and we were also taking the same course in computer programming. Before that – in 1960–61 – David Harvey came to Uppsala too. It

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was Gerd Enequist who decided that he and I should share an office, an experience that turned out to have a tremendous impact on both of us.

TB: What were the circumstances of you meeting?

GO: We were exactly the same age but because of the different educational systems he was just finishing his doctorate at Cambridge and I was beginning my graduate studies at Uppsala. He came there with a Leverhulme scholarship, ostensibly because of the Swedish data banks, more likely because he just wanted to get out of England. There was no particular reason why he should come to Uppsala, rather than to Lund or Stockholm, but luckily he did.

So one day there was a knock on the door, Gerd with David in tow. Once she had left the room, it took the boys two minutes to realise that they knew equally little, two more to become friends for life. This is also how it has stayed, and that is despite the seemingly different paths we have subsequently followed.

TB: What was this conversation that you had with him?

GO: Once again the question is wrongly put. For the truth is that we were just fooling around, drinking and having a good time, talking about everything and nothing, naïvely unaware. Throughout the sixties we used to meet at least once a year, normally for several weeks at a time. All told we spent a lot of time together, increasingly talking about location theory and spatial modelling, almost always with a philosophical bent to it. Almost from the beginning we kept returning to the form-process problem. And when we met next time, we were surprised to discover that we had written virtually the same papers without ever having corresponded in between. Of course there were intellectual parallels. But more importantly it was a very close and personal relation.

TB: What were the kinds of philosophical literatures you were drawing upon and reading that were influential?

GO: It came gradually, more from sociology than from philosophy per se. As before, the focus was on issues of causation, occasionally on the relations between induction and deduction – reasoning as a travel story, if you will.

My first thesis – the equivalent of the American PhD – consisted of several articles, but the major one was a migration study in which I played with the axes of a set of regression models. To be very precise: is human interaction a function of distance or is social distance a function of interaction? There were, of course, many other problems as well, including the fact that the gravity model is a deterministic formulation in which the outcome is more dependent on the normalizing means than on the deviant variations. How could I acknowledge in the model the fact that even though you and I are alike, we are not identical? The answer to that basically existential question lay in probability theory and therefore I spent a lot of energy on trying to translate the deterministic formulations into stochastic form. In hindsight these issues are closely related to the problem of geographic inference, the question of what a description of spatial form can reveal about the social processes that have generated it. In my mind it is this problem of cartographic reason that eventually blew the discipline apart, a torpedo hitting the

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ship of Geography below the water-line. In its frantic search for saving -isms, the discipline at large seems happily unaware of what is at stake.

Why is this so? Because since its Greek beginnings Geography has been founded in description, description presented as a map. But what does the map tell you about the processes through which the phenomena represented by the points, lines and planes actually came about? How *are* spatial patterns generated? It was this methodological question that a small group of us got hooked on. And suddenly it was crystal clear: the same spatial form can be generated through drastically different processes. In turn this implies that from a perfect description of a spatial form you cannot say anything conclusive about how it has been generated. A conclusion that goes completely counter to centuries of geography!

Who were we? A gang of five often gathering on the living-room floor of the Olsson house at 2128 Geddes Avenue in Ann Arbor: David Harvey, Reg Golledge, John Hudson, Les King and myself. Fireworks exploding, lightening bolts out of control.

TB: It was pivotal for you

GO: Yes, and not only for me. In fact I am convinced that the inference problem played a role in David's conversion to Marxism just as it did in Hudson's drift towards historical geography, in King's decision to become a university administrator, in Reg's growth as a behavioural geographer. Splitting without splitting, a kind of revolution that the majority of today's geographers know very little about. Yet they are just as steeped by its consequences as we once were ourselves.

TB: You were taking about the same problem with Harvey in 60.

GO: Yes, and in many ways this problem is still with me. And I think in a sense it is for him too. It is so challenging because like all interesting problems it refuses to sit still. Once you approach it, it turns another face.

TB: Has that been the central problem in your intellectual life.

GO: In some ways. If I were to pick only one, I would say the geographic inference problem. But that is in itself an issue of translation, hence at bottom an issue of exploring the prison-house of language.

TB: To backtrack – when did you graduate as an undergraduate?

GO: In 1960. And immediately after graduation I was awarded a fellowship to begin work on the licentiate theses that I defended in 1965. The most formative of those years was 1963-64 which I spent on an ACLS Fellowship at the Regional Science Department in Philadelphia. In that adventure Julian Wolpert played a role, not only because he was at the University of Pennsylvania at the time, but also because he had helped me put together the application that got me there. A completely fantastic year in the company of Walter Isard, Julian, and Michael Dacey, with whom I shared an office. Topping it off were two NSF summer institutes, the first in Regional Science at Berkeley, the other in Spatial Statistics at Northwestern. For my own generation of quantitative geographers that summer of '63 was crucial, much the same people attending both events, Leslie King foremost among them. David was not at Berkeley but he did come to Evanston, where we

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all struggled with Michael Dacey’s point models, the stochastic formulations that later led to the inference problem. It is fascinating to see how the person who was so determined to transform the discipline into a hard science, through those very efforts provided the means for tearing it apart. Very strange. And with its own streak of tragedy.

TB: Other people I’ve spoken to have talked of an almost religious fervour in that period. Was that something you believed in? Were you an outsider?

GO: No, I was not at all an outsider. Of course there was an engagement, an obsession. Yet it was clearly more important to have a good time than to change the world. Lots of laughter, wine and food. Life as it is lived, fully and without compartmentalisation. That’s why we are still meeting whenever we get a chance, that is where the foundation was laid.

TB: Did you feel that you discovered the truth, or was that just irrelevant?

GO: Of course we knew that we were on to something exciting. But we were also young. Much of what happened actually happened in our house, everyone who counted coming through for the parties, the seminars and just the fun. Without my wife as willing hostess it would all have been very different.

TB: When did you get your first PhD?

GO: In 1965. And without stopping I set my sights on getting the second doctorate, receiving a number of job offers at the same time. One of them came from the University of Michigan, another from Akin Mabogunje in Ibadan, who wanted be to join him in Nigeria. And I said yes, I will do that. But it took such a long time with the bureaucracy, and I was getting all these other offers, that in the end I decided to go to Michigan instead. The times were really unbelievable: in Ann Arbor I had never set foot, there was no job interview, I had met none of my colleagues to be. And even more remarkable, before our trunks had arrived I was receiving several new job offers, one from Toronto, where I knew that Les Curry was teaching. Since his work was important to me, I went to the Michigan chairman – I think it was Mel Marcus – asking for advice. “Take it easy,” he said. And three weeks later I was promoted to Associate Professor with tenure. I had only the vaguest idea of what it all meant, the dictionary definition of the word “tenure” of little help. And given the reactions, I was careful not to ask.

It was fantastic, especially as the quick promotion strengthened my feeling of independence. In the meantime I kept working on my Swedish second doctorate, which I received in the spring of 1968, David one of the guests at the party that nothing could beat.

TB: What was that second doctorate about?

GO: I used some parts from the first thesis, but the second was called *Distance, Human Interaction and Stochastic Processes: Essays in Geographic Model Building.* Formally it was only 5 pages long, but in reality it was a weaving together of six separate articles. I was stunned to learn that the committee (Professor Wold one of its members) had given it the highest possible mark, a most rare distinction. Once again a shot of independence.

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TB: You had that monograph on spatial interaction published by the RSA – was it in 65?

GO: Yes, a combined monograph and bibliography that I wrote during the year at Penn. It got a good reception, but eventually it made me embarrassed. Although it was really only a student paper, it got reprinted and reprinted until in the end I finally said no.

TB: How would you characterise your relationship with mainstream work up until these pivotal meetings in 1968 in your living room? Were you a true believer or had you always had a few doubts?

GO: It's difficult to say, for even though I have always been deeply engaged in what I am doing, I would never call myself an obsessional believer. If there ever were any true believers, they were not in my generation, but among those who were formed towards the end of the fifties. Brian Berry, for instance; I think he might have wanted to change the world. David too, of course, but with him everything is different. In my own case I just did what I did, no cult of anything except the urge of doing it on my own.

TB: Did you feel that geography up until the quantitative movement was fundamentally wrong – that this place-based, regionalist descriptive approach was something that needed to be overthrown?

GO: Yes, although it wasn't wrong, merely not interesting. No theory, hence no beauty. We just made fun of it, cracking jokes as we went along. I'm sure many do the same with me now.

TB: I did interview Peter Gould before he died. He demonised particularly Hartshorne – as an evil person, not just that he was wrongheaded intellectually.

G: Peter was religious, of course. A true believer. But at the same time, he was actually very open-minded. But it is certainly true that he believed not only in what he was doing, but in the idea of changing the world. Very much the same mould that you can now see in European and American politics, where the world leaders are presenting themselves as moral leaders. They are dangerous, because they are religious enough to go to war. Peter had some of that.

TB: 1968 – Had you started writing *Birds in Egg* at that time?

GO: No. That book grew out of the gravity model. As you will recall, my question had been whether what the gravity model says is correct or whether its various pronouncements are consequences of the particular reasoning mode in which they have been phrased. Was the conclusion a result of me not properly understanding the stochastic processes, of my own misunderstanding of the relations between theories and observations. So I was asking, once again, whether the revolutionary conclusion about geographic inference is correct or whether we had reasoned in a way that is good enough. Had we just done a sloppy job? Those kinds of questions obviously lead to the philosophy of science and, in my particular case, to logic, especially many-valued logics and fuzzy set theory. These were the connections that Stephen Gale and I were pursuing as early as 1970. To my knowledge these papers were the first about logic and fuzzy sets ever to appear

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in geography. Stephen, by the way, was the first in a long row of graduate students who individually and collectively continue to mean so much to me.

Of course we didn't really understand what we were doing. What we did know, however, was that unless the structure of the phenomena we were studying and the structure of the language in which we were trying to capture them were not the same, then we would be faced with an insurmountable translation problem. Easy to see what is at stake, exceptionally difficult to know what to do about it. Form and process in another guise, the parallels to the physics of the 1920s too obvious to ignore. In my own case, and with my Swedish background, these issues were directly connected first with the practice of social engineering and then with Aristotle's question about the sea-battle tomorrow.

Birds in Egg took six or seven years to write. It drew quite heavily on the lecture notes I developed for a course entitled "Thought-and-Action", an educational experiment in which the students earned credits not in geography but in the college at large. This in turn meant that I attracted some really excellent students, many of them senior undergraduates who had completed all requirements except the residency. Hence they were free to elect whatever courses they liked. And they did.

TB: In that book you draw on people like Hegel, like Wittgenstein. Did you only start reading them from 1968 onwards or had that been part of your earlier intellectual training?

GO: I had read some of it in the gymnasium, but it became important with the political turmoil between 1968 and 1975, a period which I often refer to as some of the best years of my life. Those were the days and nights of the equal sign, the key theme of *Birds in Egg*. The outcome of those investigations was, of course, that even though we cannot do without the copula, there are several alternative ways of defining it, each mode expressible through drastically different languages; dialectics one of them, conventional logic another. So overwhelming was the experience that when the book was finally out, it set off a tremendous, perhaps life-threatening, creativity crisis. A most difficult time.

TB: For you personally?

GO: Yes. For what can you conceivably write about that is more important than the equal sign? You can of course remain on the same level of abstraction, but if you want to dig deeper, where do you go.

TB: Did it create a crisis between you and other people with whom you had been friends?

GO: No, it didn't. At least not that I know of. Although I am well aware that others sometimes see me as a controversial figure, that is most definitely not how think of myself.

TB: Did you start moving out of that circle that you were a part of? Did people like Les King and Reg Golledge move away from you?

GO: No, not at all. On the contrary, we got closer. This was also the period when Reg encountered severe difficulties in his first marriage and that circumstance

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brought us even closer than before. Wine and lobster, white and red, light and dark. But at bottom always the connection between what you think and what you do.

TB: But it was such a different perspective. You seemed to be fundamentally questioning their very project by raising this issue of the equal sign or the meaning of "is." Did you agree to disagree?

GO: Yes. They thought it was interesting and I didn't feel in any sense excluded. Perhaps because our relations had always been less intellectual than human.

TB: Do you see the work you've been doing most recently – your experiments with language and that kind of thing – you see that coming up against the same fundamental problem of form and process?

GO: Yes, of course.

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