facilitate integration and or expression of single copy genes.

It was one of those cold winter mornings in 1977 that I woke up early and I had to walk for 10 min in the snow to cross the Queen's Park that separated the University from the north-east of the city where my flat was located in order to come to the lab. Then it came to my mind that cancer genes should exist in tumour cells and it should be possible to eventually clone these genes using gene transfer techniques provided that a selection system could be devised.

Cancer was always fascinating to me and I thought this would be an important problem to solve. Over the next few weeks I pondered about the possible selection procedures. My experience in the lab with codominant markers like methotrexate, amanitin and ouabain resistance made me think that if the cancer genes behaved dominantly in cultured cells there must be a way to select for them. Work with DNA transforming viruses mainly SV40 and polyoma had shown that such assays indeed existed. I then set out to set up such systems.

Since cancer was thought to be a multistage process I thought I should set up at least two systems. One was measuring anchorage independence, a property of many tumour cells and a second involved immortalisation or rescue from senescence. The results of these studies were published in November 1977 (Spandidos, D.A. and Siminovitch, L. Transfer of anchorage independence by isolated metaphase chromosomes in hamster cells. Cell 12, 675-682, 1977) and January 1978 (Spandidos, D.A. and Siminovitch, L. Transfer of the marker for the morphologically transformed phenotype by isolated metaphase chromosomes in hamster cells. Nature 271, 259-261, 1978).

I worked much harder than any other postdoctoral fellow or graduate student in the Department. I was in the lab always by 8 o'clock sometimes 7 in the morning and finished at 9 in the evening. Most of the days I would return again at 10 pm for a few hours before going home to sleep. I would work at the week-ends as well. I never had coffee breaks and I ate sandwiches I brought from home at the bench in five minutes for lunch. I was staying about 10 minutes away from the lab in a flat with my wife who was expecting a child which was born in June 1977 (a daughter) and she was taking courses in the Graduate School of Law, University of Toronto, and was working nearly as hard as myself apart from taking care of me and the rest of the family. Although, because of the circumstances, she interupted her studies in June 1978, she eventually obtained a PhD in Law from the University of Glasgow. Today she is practising law in Athens and at the same time acts as Legal Adviser to the Greek Telecommunications Organisation. We also had a second child (son) born in 1981 in Glasgow.

In the lab I was more productive than most of the others although some other postdoctoral fellows like Pamela Stanley or Radley S Gupta were working much less but they were at least as productive. However, I felt that their work was very mundane involving lots of killing curves with cytotoxic drugs - the kind of work that had been done for years before I arrived and after I left Siminovitch's lab.

I believe in general I get along very well with my peers and people who work with me. Although I came across some difficult personalities I tried to avoid them as much as I could. I never had a clash with any of them although these things often happen in various labs. I participated often in