social activities in the lab, ie being invited or inviting people for dinner. Every Friday noon all the people in the Dept of Medical Genetics had lunch together in the Departmental library tasting food from different countries and discussing our work. Siminovitch, who was chairman of the Department, never participated in this or any other social activity in his Department.

I spent less time at the bench in the last couple of months in Toronto although I was in the lab the same hours as before. The demand of writing papers and being invited to give lectures had grown at that time occupying 20-30% of my time in the lab.

Typical of my committed attitude towards my research was the fact that when I had an operation to have a large stone removed from my kidney in December 1977 I amazed everyone by returning immediately to the lab. Moreover, when I came out of the hospital it was a week before Christmas, a time when the lab was nearly dead. I remember working even for several hours on Christmas Day.

Nancy Stokoe worked with me for only a few months. However, soon I found out that, not only was she incapable of following the rhythm of my work, she was inhibitory. I had to spend at least one hour every morning writing down and explaining everything to her but by the end of the day nothing had been done or it was a disaster. So I gave up by December 1977. Without saying anything about the problems to Siminovitch I told both her and him that since I was going to have an operation in the next few weeks it would be better for her to work for somebody else in the lab.

It was in March 1978 when Siminovitch came out of his office one afternoon and told me that Richard Axel of Columbia University in New York had 'phoned and told him that he had obtained similar results to mine using total cellular DNA. That is, he could transfer biochemical markers like the thymidine kinase gene using the calcium phosphate technique and that he was about to submit a paper to "Cell" where we had already submitted ours and he was refereeing it. He said that it would be nice to delay a bit, to give some time to submit his own, and cite each other since he claimed that he had arrived at the same conclusion independently. Soon we heard from "Cell" (the letter is dated March 14th, 1978) that our paper had been accepted and it was characterised by the editor Benjamin Lewin as a "tour de force". It was going to appear in June but it never did. I never learned why. The paper by Axel which is considered a classic was published in "Cell" in July 1978 (Wigler, M., Pellicer, A., Silverstein, S. and Axel, R. Biochemical transfer of single-copy eucaryotic genes using total cellular DNA as donor. Cell 14, 725-731, 1978).

It is also of interest that in 1980 the same group published a paper (Wigler, M., Perudo, M., Kurtz, D., Dana, S., Pellicer, A., Axel, R. and Silverstein, S. Transformation of mammalian cells with an amplifiable dominant-acting gene. Proc. Natl. Acad. Sci. USA 77, 3567-3570, 1980) where they used donor DNA from the same methotrexate resistant cell line which they obtained from Siminovitch's lab on which I had done all my experiments withouting citing even my first paper published in 1977 (Spandidos, C.A. and Siminovitch, L. Transfer of codominant markers by isolated metaphase chromosomes in Chinese hamster ovary cells. Proc. Natl. Acad. Sci. USA 74, 3480-3484, 1977).

In early March 1978 I had been invited to attend an international