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Andrzej Krzysztof Tarkowski abroad, in photos and correspondence

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ABSTRACT An informal account records the remaining traces of Tarkowski's research visits to the United Kingdom and France. The account has many authors and it should not be regarded as an exact history. The early 1960s began with the dramatic production of chimaeras at the University of Bangor and the long term exchange of information with Anne McLaren's Edinburgh laboratory. The techniques of parthenogenesis and nuclear transfer became the obsession of the 1970's and Tarkowski pursued the problem in Oxford (U.K.), in France, and with his group in Warsaw (Poland). A variant of this theme emerged during the 1980's and this was attempts to produce interspecies hybrids in Oxford and Warsaw. During the 1990's, the Warsaw laboratory became sufficiently well funded to make his trips unnecessary and his pupils became a Polish Diaspora of Embryologists.

KEY WORDS: *Tarkowski, travel, research*

NOTE from the Editor-in-Chief: Our original intention, in keeping with the tradition of the journal, was to dedicate this Special Issue to Prof. Tarkowski, in recognition of his important contributions to our understanding of the developmental biology of mammals. Other significant pioneers of the field, such as Barry Pierce, Ralf Brinster, Ann McLaren, etc., have been similarly honored in previous Special Issues of the *Int. J. Dev. Biol.* However, Prof. Tarkowski's extreme modesty and his persistent refusal to accept this honor, together with his preference to figure as Guest Editor of this Special Issue, which highlights important aspects of the history of experimental biology in his country, have successfully frustrated our initial proposal. Nevertheless, we decided to surprise Krzysztof Tarkowski with our own particular tribute by inviting Prof. Christopher Graham, his good friend for many years, to compose an article about the lesser-known aspects of this personality and the impact of his research outside Poland which has led to his international recognition. We hope that both you as reader and Prof. Tarkowski will enjoy reading this special, surprise article.

Juan Aréchaga (Editor-in-Chief)

Introduction

An internationally famous scientist has a problem. If he or she allows a volume to be dedicated to them then it can read like a premature obituary and make them depressed. On the other hand if they refuse to have a volume dedicated to them, as in this case, then they become a victim of their colleagues. Tarkowski has sought to neutralise the second outcome by his soberly thought out responses in interview. But the Editor of this journal decided that there was more to him than that. What follows is not an attempt to capture a person in amber and the Editor has requested that it remains secret from Tarkowski until publication thus committing the following sketches to the blurring memories of aged brains that are forbidden to check the facts or more likely fables with their subject. It is a collection of anecdotes, mainly about Tarkowski visits abroad when he was free from the responsibility of running a laboratory and it makes no attempt to capture the international contribution and influence of the now modern and well equipped Laboratory of Embryology of the University of Warsaw. His friends and acquaintances outside Poland saw him at his most relaxed, so as to speak «on holiday» if that is how the schedules in Figs. 5 and 7 can be described. He was always Krzysztof when abroad and never Andrzej or AKT, or Professor Tarkowski or more to the point in Warsaw, the Szef.

Whenever possible Tarkowski's written words are used and these passages are identified by "quotation marks". Remarks inserted into these quotations may clarify the extract and they are between square brackets [for example]. The reader is asked to bear in mind that Tarkowski's remarks are taken from personnel correspondence and record brief moments of irritation or success and they are not the considered views informed by the wisdom of hind sight that can be found elsewhere in this volume: they do capture the moment free from the gloss of history.

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Andrzej Tarkowski's primary professional passion is mammalian development and it is difficult to imagine him sitting in a comfortable chair not worrying about the outcome of an experiment. He has travelled widely and visited numerous laboratories, often for several months. Any semi-historical article relies on sources and this is the reason that there is bias towards periods in the United Kingdom and particularly times in Oxford when he interacted experimentally with the groups headed by Chris Graham, Richard Gardner and John Clarke and received substantial support from Henry Harris in the Sir Wm. Dunn School of Pathology. During these visits, the intensity of his post-mortem discussions about failed experiments was remarkable and persistent: nothing really worked. The explanation of this string of disasters was simple, namely that he was always trying to extend techniques beyond the existing possible and isolation from the day to day business of his laboratory gave him space to investigate and invent, and the results often lead on to long term research projects and papers back in Warsaw. These fragments record battles against the odds and can be taken as an antidote to the smooth, rational, and successful progress of science in the in Warsaw laboratory, as recorded elsewhere in this volume.

The bulk of these visits to the UK cover a period when the Polish economy was weak and regulated by the Communist Party. In the Warsaw laboratory simple and effective methods had to be invented to bypass the lack of funds. Gas cylinders were not used for embryo culture because alveolar air blown into one of two holes in a sandwich box would do instead if the holes were then tightly sealed with plasticine, method mentioned at Ciba Symposium (Wolstenholme and O'Connor, 1965) and which he continued to use during his first visit to Oxford. The supply of mice was rate limiting and if there were no pseudopregnant recipients on a particular day then the embryos were transferred to the oviduct of immature non-cycling virgin females where they stayed until a suitable recipient was to hand for retransfer: in extreme cases the immature recipient could be left with a vasectomized male so that the experimental embryos could emerge from the first teenage pregnancy. Each pregnancy was precious and so vaginal swabs were taken at regular intervals to find out if the experimental material was becoming resorbed and if it was then the experiment was terminated in the hope that some of the embryos could still provide scientific information. These methods were unknown in Oxford at least, and he had much to contribute to the local practice of throw away if it does not work.

Fig. 1. "In Bangor, Professor Brambell was full of enthusiasm toward all my experiments ...." *from an interview with Tarkowski (Maleszewski, 2008). The ability of the Department to express their enthusiasm appeared limited. Professor Brambell is beside Tarkowski, smoking a pipe (Photo: Ian Wilson, 1961).*

By 1991 Poland had just had its first direct Presidential elections, a market economy was starting, the Republic of Poland had been founded, and for the first time during Tarkowski's long career it slowly became easier for him to buy chemical reagents and equipment and his laboratory started to become independent of money from the West. The written record dries to a trickle in about 1992, coinciding with the introduction of email and the consequent end of any history of scientific correspondence. Thus in 1991,

> "I have a computer in my room but I prefer to use my old typewriter. My young collaborators use computers even for writing letters composed of one sentence only."

However this recalcitrant palaeotechnic quickly gave in and his wise words were soon to be lost to the delete button of email communication.

Bangor, Edinburgh, chimeras and position. The 1960's

Historical sources of information are very scarce for the 1960's.

(1960-1961) The University of Bangor

The extent to which the department was able to express their enthusiasm for his work can be judged by Fig. 1. Brambell's typed report to the Agricultural Research Council on the work of their Unit of Embryology in Bangor contains 19 pages about transfer of immunoglobulins from mother to young, and in less than a page [edited]:

> "…some interesting work has been carried out on early mammalian embryology that should be mentioned. ……. This pioneering work of Tarkowki's has opened up a wide field for the study of genetical and immunological problems in chimaeric mice and the opportunity has been taken by Tarkowski after his return to Warsaw and especially by Mintz."

This chimaera work had depended on a previous advance: John Biggers and Anne McLaren had published the first successful 24 hour growth in culture and transplantation to a foster mother of preimplantation stages of mouse in 1958 and John Biggers (2001) wrote:

> "The first application in mice of the techniques of preimplantation embryo culture and transfer was published by Andrzej Tarkowski,…."

Ian Wilson was the only other person working on mice in Brambell's laboratory in Bangor, Wales, when Andrzej Tarkowski made chimeras and Wilson was to introduce the first lineage marking method into mouse embryology and go on to test the influence of position on cell contribution to the blastocyst. Wilson recalls an

utterly committed scientist managing to relax this commitment with a joint camping trip through Germany, Holland, and France, at that time a rare chance for a Polish scientist to see Europe and call on mammalian embryologists such as Seidel. He also visited Anne McLaren in Edinburgh and around this time and a good summary of the following 15 years of chimaera work from the Warsaw and Edinburgh laboratories can be found in the book "Mammalian Chimaeras", McLaren (1976).]

(1965) Inside-Outside Hypothesis

One explanation of the successful development of chimaeras was that the contributing cells were already committed to form the several cell types of the blastocyst and on aggregation of embryos they sorted out and reached their normal position. Mulnard, Mintz, and Tarkowski did not find clear evidence of sorting out when they met and fully discussed the issue at the Ciba Foundation meeting "Preimplantation Stages of Pregnancy" in London in April 1965 (Wolstenholme and O'Connor, 1965). From this volume:

> "In view of the fact that …….migration and selective sorting-out of cells do not occur ….I agree with Dr Mintz that the idea of early determination of blastomeres becomes difficult to maintain. Further evidence that this is not the correct explanation is provided by experiments [that]…… have shown that the ability to differentiate into trophoblastic cells is inherent in all blastomeres of the 4-cell and 8-cell egg."

The experiments were: Tarkowski AK, Wroblewska J. (1967) Development of blastomeres of mouse eggs isolated at the 4- and 8-cell stage. *J Embryol. Exptl. Morphol.* In this paper the theoretical idea of the influence of blastomere positions on their fate is elegantly derived from the experiments. This idea inspired a large number of papers from the UK about the influence of cell position, amongst other things, on the development of the cell types in the mouse blastocyst (first from the laboratory of Ian Wilson and then extensively from the laboratories of Martin Johnson in Cambridge and of Chris Graham in Oxford). Many years later in Theriogenology, the same paper was cited properly, but by different authors: Tarkowashi, Wroblewaka (Figs. 2,3).

It was these papers on chimaeras and cell isolation that excited the mammalian embryology community in the UK because they demonstrated that the preimplantation mouse embryos could be put together or taken apart and continue to develop into a breeding adult. Tarkowski had shown that the mouse embryo was robust, tractable, and could recover from manipulation.

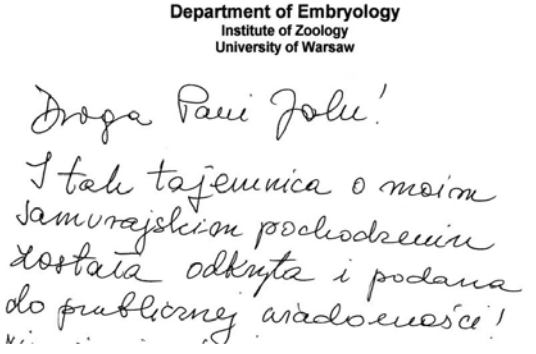
First Oxford Visit (1975)

Water, parthenogenesis, nuclear transfer

In the early visit there were three main preoccupations and the first of these was shared with every mouse embryology laboratory in the world, namely water quality for achieving viable development from zygote to blastocyst, the main limiting factor in mouse embryology (Arechaga 1998, Biggers, 1998). In 1968 Wes Whitten and John Biggers published a medium that could support development over the preimplantation period but it did not work universally and it was widely believed that Bar Harbor spring water at the Jackson Laboratory might be the secret. Oxford deionised and double or quadruple distilled with glass covered elements in a device made by a one man English company, the Pasteur laboratory installed a massive distillation unit and isolated accurate fractions to exclude lipid impurities, and chelating agents to neutralise heavy metals were added in Philadelphia. Ewa Mystkowska writes [edited]:

> "Purity of the water used [for] production of culture medium was my «idee fixe» too, as it was Krzysztof's one. He was dealing with construction of a machine (necessarily made of glass) for redistilation and my part was transportation of water not contaminated chemically from the rural well - if only it was possible. It isn't true that we were carrying water on bicycles, but the truth is that water was transported from the mountain stream in Tatra mountains, which are situated 400 km from Warsaw."

And so the reason that Tarkowski came to Oxford was water and


Department of Embryology
Institute of Zoology
University of Warsaw

Droga Pani Jolu!

I tak tajemnica o moim samurajskim pochodzeniu została odkryta i podana do publicznej wiadomości!

Fig. 2 (Left). "Dear Jola! And so has the secret of my samurai origins been discovered and announced to the public!"

Fig. 3 (Right). Japanese Prize 2002. *The prized possession of the sword that terminates discussion.*

Fig. 4. Krzysztof Tarkowski with Anne McLaren *at the International Society of Developmental Biology meeting in Basle, Switzerland (1981).*

Sendai virus (see below) and after all his early visits water, culture media, and Sendai virus were shipped to Warsaw.

The other two preoccupations were mouse parthenogenesis and nuclear transfer for cloning and both required effective egg activation. The publication dates of papers do not show that a range of activation methods had already been developed in Warsaw at the time of this visit. These methods included heat shock to ovulated eggs in the oviduct, fertilization followed by osmotic expulsion of the sperm nucleus through the sperm slit in the zona pellucida, and hypotonic shock.

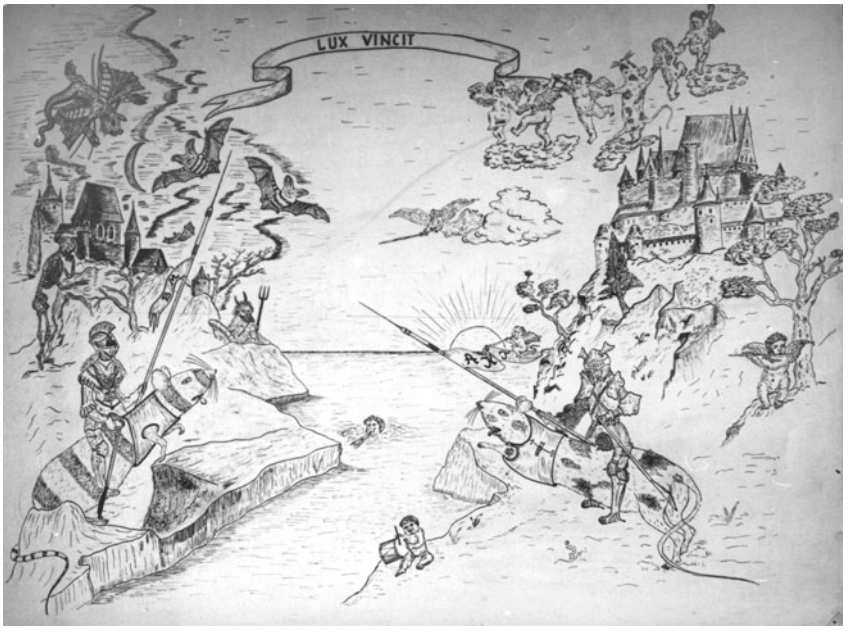
His Oxford work concentrated on the problems of enucleation and the reintroduction of nuclei into enucleated eggs. He had invented a novel enucleation method in Warsaw and this was further developed and it provided the essential step in the only two research papers that emerged, including one with Janet Rossant in Nature (1976). The zona pellucida was removed and the egg or blastomere placed in cold medium over an agar base. Turning the round object into a sausage shape by blowing it in and out of a finely drawn flame polished pipette produced something resembling a long thin balloon and this could be rapidly rolled under a glass needle to isolate one part from another in much the same manner as street vendors can twist long rubber balloons into toy dogs. The cell membranes were apparently annealed in the twist and the nucleate and enucleate parts could be cut apart. The reintroduction of a nucleus into the resulting enucleated cytoplasm was again not a problem because inactivated Sendai virus had been shown to fuse blastomeres and cells into zygotes and Henry Harris and colleagues in the Dunn School were the world experts on Sendai virus assisted cell fusion and 50 metres away. Part of his schedule of work is below (Fig. 6).

The experiments were performed free hand and the pile of books that supported his arm and elbow were believed to have supernatural power that would ensure a successful outcome to these demanding manipulations (Fig. 7).

The catch was that nothing much developed when the cells were taken through the whole enucleation-renucleation protocol. Certainly nothing about nuclear transfer was fit for publication. There is a photograph dating from this visit (Alexandre, 2001).

(1975-1981)

The written Oxford record covers a period when all the work was based in Warsaw and the preoccupations of the first period had become persistent obsessions. These extracts from Tarkowski letters to Oxford were usually written when an experiment had crashed or the electricity supply had failed, or the male mice had lost their libido in the summer heat of an animal house without cooling.



Water and Sendai Virus

(1979)

Following a week end break in the power supply to the freezers:

> "could you possibly bring one small bottle of the frozen stuff [already inactivated virus?]"
> "We are half dead after 4 weeks of continuous dry weather. In the lab temperatures are above

Fig. 5. Coat colour patterns of chimaeras, as interpreted by the laboratories of Beatrice Mintz in Philadelphia and Andrzej Tarkowski in Warsaw, attributed to Jacek Modlinski, School of Jan Matejko (1838-1893). *The discussion was about the coat colour patterns of chimaeric mice. Some believed that striped patterns (left) represented the migration of melanocytes from 17 founding populations on each side of the dorsal mid-line. Others (right) could not see these patterns. Lewis Wolpert published a theoretical paper showing that the patterns could have been produced by chance.*

30º and we could probably do all our culture work without incubators."

"All [Oxford] media work and we try to find out what is wrong with our medium-probably it is water." …

"Freezer disaster, please send more virus".

(1983)

[Please bring to a meeting in Paris]

"1. A bottle of freshly prepared Whitten's medium so that our lab could do some egg culture again-one small bottle would suffice to start and complete 4 or 5 projects that would otherwise be delayed for who knows for how long. 2. English penicillin and streptomycin. 3. Viable lyophilised Sendai virus."

Parthenogenesis and nuclear transfer

(1977)

"Your news about XX's experiments makes me absolutely mad-this experiment was carried out in a slightly different way (cytochalasins B at 2nd cleavage) by Modlinski, who, however, was only able to obtain blastocysts."

(1979)

While planning a brief visit to Oxford:

"We could do short and ingenious experiment (if you still believe in short and successful experiments), or just discuss scientific matters of mutual interests, or do nothing at all (I shouldn't say preferably)….please let me know [about the visit], preferably in an official and serious form so that I could start to make arrangements …" [i.e. provide a formal invitation so that his University and the Government would give him permission to leave the country].

(1980)

"XX works so fast that he hardly leaves anything (in the field of nuclear transfer) for other people. No chance to compete."

"XX etc are far ahead of us and I think that we should play with differentiated cells rather than embryonic from successive stages as he does. By the way, it is now quite clear for me why we didn't observe any reactivation of the follicle cell nuclei in early two-cell embryos - once the nucleus is reformed (and all the nuclear proteins are taken in by it) even an "ideal" nucleus such as the second polar body nucleus will remain unchanged until the next cell cycle. We made a mistake - we should have fused follicle cells with zygotes just prior to the first cleavage division."

(1980-1982) Second Oxford Visit

(1980)

"This time I would really like to do micromanipulation of nuclei, using a micromanipulator rather than a pile of books."

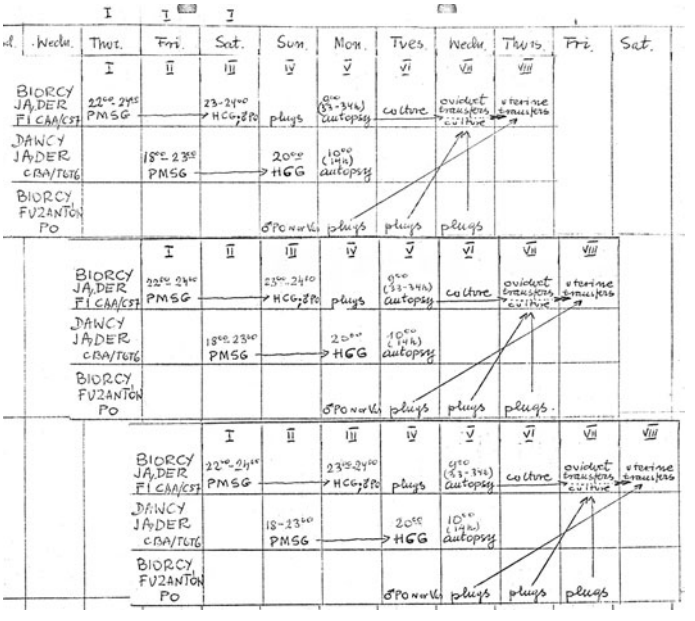
(1981)

From Tarkowski application for a 3 month EMBO Fellowship:

"Nuclear transfer in the mouse. The aim of the proposed investigation is to examine the capability of nuclei from fully differentiated cells from adult mice (and possibly from teratocarcinoma cells) to undergo reactivation in the egg cytoplasm and to promote embryonic development."

(1982)

On return to Warsaw and processing the whole mounts, the nuclear transfer experiments with a micromanipulator in Oxford turned out to be the transfer of cells with an intact cell membrane into egg cytoplasm and these cells within a cell did not show any signs of activation. Tarkowski went back to Sendai assisted cell



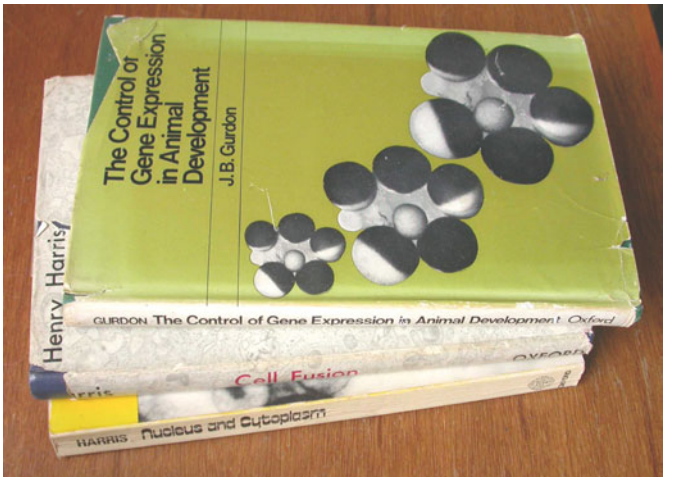


Fig. 6 (Left). Tarkowski's protocol for experiments in Oxford. *The exact purpose of the work is uncertain but a chromosome marker was used (CBAT6T6) and it is likely that all these superovulations were for a nuclear transfer experiment (1975).*

Fig. 7 (Right). Believed to be the pile of books that supported Tarkowski's elbow during free hand enucleation and nuclear transfer by cell fusion (1975).

fusion and more:

> "At the moment we carry out large scale experiments aimed at fusing the second polar body with one-cell zygotes which are just about to undergo the first cleavage division. This technique works well but despite various modifications of hypotonic treatment fusion does not exceed 20% at the best." [The zygote swells in hypotonic media, pressing the zygote against the polar body and maximising the chance of fusion.] ....I am going to fuse cells with unfertilized and newly activated eggs using PEG.... the best PEG turned out to be Fluka 1000 which we will be able to order through WHO later in the year."

[A reminder that it continued to be difficult to obtain funds for chemicals.]

(1984-1985) Third Oxford Visit

(1984) Lead up to a visit to Oxford

> "We are steadily approaching my long lasting goal of nuclear transfer....We have succeeded in adapting the techniques of cell fusion in electric field to eggs and blastomeres. We started with the 2-cell eggs as a model system and we get high fusion rate and good survival to blastocyst stage. Of course fusion of half blastomeres and producing tertraploid blastocysts is not interesting per se and we used them only to work out the optimal parameters..... We know that the technique works for eggs + karyoplasts, eggs + blastomeres, eggs + thymocytes, and surprisingly for the producing blastocysts with two inner masses. The technique may have tremendous potential. Do you think that you could get it before I arrive a pulse generator ....?I really hope that this time we will be the first to publish the technique."
>
> "Our electrofusion technique works beautifully ....nearly 100% ...it is a beautiful toy."
>
> "Modlinski has just come back from Paris where he finally got a very good batch of virus and was very satisfied with fusion of eggs with cells injected under the zona pellucida."

Tarkowski worked in Oxford between Oct 1984 & April 1985 but once again nuclear transfer did not lead to long term development. In 1985, he and his colleagues did however manage to publish this technique first.

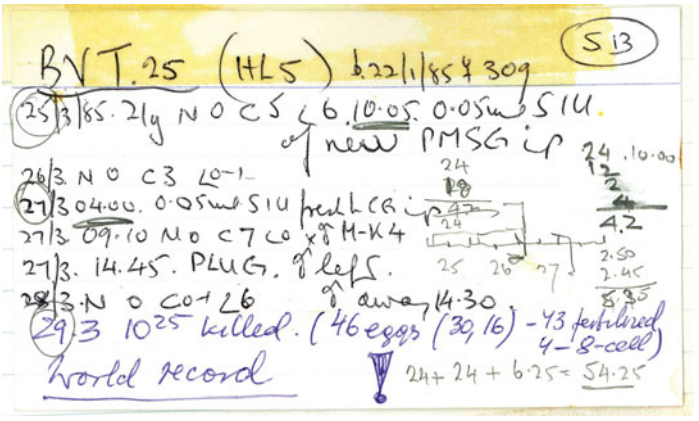


Fig. 8. Cage card of bank vole superovulation routine in John Clarke's experimental note book (1985). *Note the timing of injections.*

(1986)

> "In the lab a disaster. A mouse pox was introduced with animals bought from the breeders and we have to kill the whole colony."

Interspecies hybrids

In 2007, the UK Academy of Medical Sciences report about Inter-Species Embryos concluded that "research involving interspecies embryos (human embryos containing animal material) should be permitted under regulation, to develop tools for understanding human development and to further our knowledge of nuclear transfer techniques and human embryonic stem (ES) cells". More than 20 years earlier Tarkowski had begun a long term investigation of interspecies hybrids that did not involve humans.

(1984-1985)

Tarkowski had quickly seen that cell fusion techniques could be used for making animal hybrids across species barriers. The following account is an attempt to track the progress of a project as it bounces between the Warsaw & Oxford laboratories. During the 1984-1985 visit, he and John Clarke set up a heroic experiment to unite mice and laboratory bred Bank voles (*Clethrionomys glareolus*) at the level of the blastomeres. They took it in turns to get up at 3 am in the morning to give injections of super-ovulating hormones to the Bank voles. Luckily written evidence does exist that these relatively mature scientists (over 100 years between them at the time) were able to maintain this demanding protocol although one of them slipped by an hour when the world record for Bank vole superovulation was achieved (Fig. 8).

The mean for 22 successfully induced females was 15.3±2.5 eggs and 86% of the oocytes had been fertilised. When interspecific fusion between blastomeres was achieved the cytoplasm of the "tetraploids" filled with small vacuoles during the next 24 hours and development was blighted. John Clarke writes [edited]:

> "he was (is) unique in my University experience in the passion with which he worked, expressing himself very forcefully when a procedure failed and displaying infectious pleasure when something went well."

(1988)

Magdelana Zernicka-Goetz writes [edited]:

> "I was given the task of continuing this study. Prof. Tarkowski (as we referred to him) handed me all his tidy lab books from his Oxford days so I would develop a feel for what to do to make the project a success....The Bank voles were wild and caught in Bialowieza every other week and brought to Warsaw..... I caught these wild beasts for superovulation by placing my [leather] gloved hand into their cage, where each finger provided the host for a set of jaws. The hand had then to be rapidly removed bringing with it five or so of the retaliating organisms. Such was the charismatic power of the Professor Tarkowski over his students that they would do such things for him.... However, after several months of being bitten I managed to persuade Professor Tarkowski

that we should perhaps consider an alternative rodent on which to try the hybrid experiment."

Despite "a more positive and active attitude to life" a productive union between mice and rats was not achieved.

(1991)

After this, Zernicka-Goetz joined the Oxford laboratory for a year with the following introduction from Tarkowski.

"I don't want to persuade neither you nor her to undertake the "hybrid embryos" project, the more so that she is a bit fed up with it [the double negative does not conceal Tarkowski's intentions]. However, I would like to explain the background of this project with some of the results so far obtained. We have produced hybrid embryos by blastomere fusion (the experiment I did in Oxford using bank vole (much more convenient than the rat) and mouse), cross fertilization and transfer of pronuclei or metaphase II spindles (the latter experiment done by another girl in the lab). All these experiments have shown that there is a very early incompatibility between nuclei and cytoplasm which causes rapid arrest of development either in the same cycle or during the next cycle……I do not hope to produce hybrid animals [any longer]……It is very puzzling for me that while haploid embryos can develop into blastocysts the presence of an alien haploid set of chromosomes creates a lethal situation. When development proceeds without a problem we learn very little (or nothing). In this particular case some very basic processes are affected." [edited]

[In Oxford Zernicka-Goetz worked on a different project. Later, the Syrian hamster was also to prove no better as an illicit partner of the mouse (Tarkowski *et al.,* this volume).]

(1991)

"I will then go to Paris (Maro's lab) for 2 1/2 month this autumn. I am very curious whether I am still able to do experimental work – at home I have too much other work to do experiments. Pity because at last we have bought very nice equipment."

Jacek Kubiak writes [edited]:

"Once in Paris, however, he found himself in a hospital that had been reserved for prostitutes in the 19th century. Despite his health problems, the stay in hospital was however an opportunity to read all books by Isaac Bashevis Singer from the library of his Polish collaborators in Paris. Quitting his hospital bed he emerged into Maro's lab and collected fragments of mouse cytoplasm from 1- and 2-cell embryos (cytoplasts) and these samples eventually took part in a study of autonomous activation of histone H1 kinase (Ciemerych et al., 1998)."

On photography

See a sample of Tarkowski's photography in Fig. 4 of the interview with A.K. Tarkowski (Maleszewski and Tarkowski, 2008, pp. 163-169 in this issue).

(1986)

"May I ask you once again (hopefully for the last time but I will not promise) to send this [colour] film for developing and printing to one of the English companies. What I do not like about them now [xx company] is that they-as many other laboratories-have switched from matt to glossy prints. If you by any chance know of any laboratory that still makes lustre prints …. Orchids in Singapore."

(1987)

"I am sorry that I trouble you at all with my new hobby but I am getting worried …. the colour prints have been stolen in the post."… "From this you may gather that my only obsession is photography. It is not quite so and I try at least to do a little bit of research."

"Please send negatives of colour films…."

(2004)

"After my exhibition in Gorlitz, I received a letter from a German lady saying that my photographs had changed her life by their insight. Nobody has ever said that to me about my science!"

Szef's advice to scientists

(1979)

"I hope that you enjoyed your talk (this is much more important than enjoying the talk by other people)".

(1980)

Humour in Lectures

"For my nuclear-cytoplasmic lecture in Crete I wrote a short play. When writing it I considered it very funny but when I showed the text to some people on the course the response was meagre. Perhaps my sense of humour differs from that of other people, or only the author finds the text funny."

And from the stage directions

"The ashamed curtain quickly goes down".

(1982)

Never look back

"When lecturing to Warsaw students, I was suddenly struck dumb for what appeared to me to be about 5 minutes. It flashed across my mind that the experiment that I was talking about had been performed by me over 20 years ago and I suffered a severe depression that probably lasted no more than 1/100th of the perceived time."

(2005)

Reacting to editing

"Despite the efforts of friends and former friends, I succeeded in saving a few precious fragments of this paper. The best parts were lost to the scientific community beyond retrieval."

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