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Dephlogistication,* Imperial Display, Apes, Angels, and the Return of
Monsieur Émile Zola.

New developments in the origins of AIDS controversy, including some observations
about ways in which the scientific establishment may seek to limit open debate and
flow of information on "difficult" issues.

Edward Hooper

Abstract.

Since the mid-1950s, in laboratories around the world, oral polio vaccines (OPVs) have been routinely amplified in locally-prepared primate kidney cell cultures, before being diluted and fed by mouth to local populations. During the fifties, it was possible for a virologist to prepare polio vaccines in the primate cells of his or her choice: to do this contravened neither scientific rules nor recommendations of the day. Some OPV developers (such as Albert Sabin and collaborators) reported the fact that local preparation of the vaccine had occurred in the country of use, such as the U.S.S.R.; while others (such as Hilary Koprowski and collaborators) reported

* According to the Oxford English Dictionary, this rather impressive word means a "depriving of phlogiston", or a "relieving of inflammation". It is used in both contexts in the course of this paper, but overall I prefer to think of it in a slightly wider context – that of a "reduction in hot air levels".

Additional note: The year 2000 paperback editions of *The River* contain fully rewritten postscripts, and page references from page 827 onwards are therefore different from those in the 1999 hardback editions. All references in the current paper refer to the 2000 editions, which are henceforward referenced merely as "*River*, 2000". Because the figures in the new postscript were inserted on different pages in the US and UK paperback editions, references to passages in the postscript are sometimes one page earlier or later than specified.

only that their vaccines had been diluted, with no mention of local amplification. However, Dr Koprowski has since stated that his polio vaccines *were* amplified, both in Europe and elsewhere, and evidence from several sources indicates that amplification of his Type 1 OPV, CHAT, occurred in the Belgian Congo during the 1957-60 period. In particular, one witness has reported that an oral polio vaccine was being made in the virology department of the *Laboratoire Médical de Stanleyville* (LMS) in February 1958, and that he personally helped administer this locally-made CHAT vaccine by mouth to local African populations, including the inhabitants of a military camp.

Evidence acquired from numerous sources over a period of years reveals one other unique detail about the *Laboratoire Médical de Stanleyville*. From 1956 (at the latest), the major departments within that lab were routinely using tissue cultures prepared from chimpanzee cells (often with chimpanzee sera employed as growth medium). No other type of tissue culture is mentioned by these sources until a few tubes and bottles of baboon tissue culture, which were prepared experimentally in the middle of 1958. Until now, tissue culture and polio vaccine preparation at the LMS has been a subject surrounded by secrecy, for which documentation was either missing or misleading. What this new evidence strongly indicates is that the oral polio vaccines distributed by the LMS up to at least the middle of 1958 (save for 2,000 doses brought from the US in capsule form) were given a final passage in chimpanzee cells.

The fact that the closest known relative to HIV-1 Group M is the simian immunodeficiency virus of the common chimpanzee (SIVcpz), raises the question of whether this might be the way in which SIVcpz transferred to humans to give birth to HIV-1 Group M, and thus to the AIDS pandemic. The LMS chimp tissue cultures were primitive Maitland-type cultures, made without trypsin, and were thus prepared in a fashion which (a) would have failed to inactivate any SIVs that might have been present, and (b) would have facilitated recombination between individual SIV strains. This new evidence about the local preparation of CHAT vaccine in the Belgian Congo lends significant support to the so-called “OPV hypothesis” of AIDS origin.

Several different lines of evidence, presented here, offer further support to the hypothesis. Between 1956 and 1959, some 500 common chimpanzees (*Pan troglodytes*) and 80 pygmy chimpanzees or bonobos (*Pan paniscus*) were sacrificed at a chimpanzee research camp at Lindi, an isolated spot in the bush 15 kilometres from

Stanleyville. Although it has been claimed that much of this research involved testing the safety and immunogenicity of CHAT vaccine, virtually no significant details about the research have ever been published. Three African eye-witnesses say that the man who regularly extracted organs and blood from these chimpanzees and bonobos was the head of the Stanleyville virology department. In 1958, an outbreak of fatal *Klebsiella pneumoniae*, one of the opportunistic infections of both AIDS and simian AIDS, was reported at Stanleyville hospital. A similarly fatal *Klebsiella* outbreak had previously taken place at the isolated and quarantined chimp camp. It is proposed that the common denominator was a batch of CHAT vaccine prepared locally in chimp cells, and fed to local people.

Dr Koprowski's base for the polio vaccine research was the Wistar Institute in Philadelphia, of which he formally became director in 1957, although his links with that institute appear to go back several years before that. Between 1953 and 1957, a group of scientists at the Wistar, funded mainly by the U.S. Army Chemical Corps, worked on the mass production of viruses (notably polioviruses) in different substrates, such as human amnion cells, HeLa, and other cell lines – many of which, we now know, had been taken over by HeLa. The same and similar human cells and cell lines were in use in several labs in the Belgian Congo from 1954 onwards. According to a contemporary report, CHAT vaccine was being prepared in one of these Congolese labs (a place where *only* human cells were used for cultures) from around August 1958 onwards. On the basis of this and further evidence, it is proposed that some of the CHAT vaccine that was made in chimp cells in Stanleyville may later have been further passaged in human cells (including HeLa), with potentially even more serious implications for human health. This “HeLa addendum” is more tentatively proposed than the main OPV theory. It is not fundamental to that theory, but should be viewed as an adjunct, which (if substantiated) may help to explain the uniquely dreadful impact of this particular HIV strain (HIV-1 Group M) in human populations.

There are striking coincidences of place and time between the 1950s feedings of CHAT vaccine in Africa, and the first appearances in the world of HIV-1 Group M and pandemic AIDS. The earliest cases of Group M-related AIDS in Africa through 1980 come from the DRC, Rwanda and Burundi, these being the three African countries where CHAT was field-tested in the fifties, in 27 known campaigns. In fact, 68% of the earliest recognised cases come from the specific places where CHAT was

fed. Significant corroboration is afforded by the fact that the earliest evidence of Group M infection comes from the DRC and Burundi, with 76% of all recorded African Group M infections up to and including 1981 coming from the same towns and villages where CHAT is known to have been fed. These coincidences of place and time would be remarkable by themselves; but allied to the hitherto unrevealed information about local CHAT preparation in Stanleyville, they assume a real and ominous significance.

Several scientific arguments have been put forward to “disprove” the OPV/AIDS theory of origin. Most notable are: i) that phylogenetic dating analysis allows one to trace AIDS back to the 1930s (before the time of the African OPV trials); ii) that SIV would not survive the vaccine-making process; and iii) that the immediate viral ancestor to HIV-1 Group M comes from a “different chimpanzee”. The author reviews a dozen or more of these arguments. Closer analysis of the evidence reveals that many of these alleged disproofs are untenable, while the others are much weaker than has previously been claimed. To date, there is not one compelling argument against OPV/AIDS, although a *bona fide* sample of HIV-1 Group M from before the date of the OPV trials would constitute powerful evidence.

A great deal of effort has been devoted by both senior scientists and the major scientific journals to trying to persuade the scientific community and the general public that the OPV/AIDS theory has been “destroyed” by facts and evidence. In reality, the theory is shown to stand stronger than ever. An analysis is made of why most mainstream scientists find the OPV/AIDS theory so difficult to accept, and why there has been such a virulent response to the theory in certain quarters. The present author has been accused of inventing witnesses, misrepresenting evidence and lying. Evidence (both documentary and recorded) exists to refute each of these allegations, and the author invites those who have made the charges to review this evidence with him. The article ends with the author, assisted by Monsieur Émile Zola, making a series of charges of his own about the iatrogenic (physician-related) origins of AIDS, which the relevant protagonists are invited to refute.

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1. Introduction: “experts” and expertise.

Claims of expertise have been ringing down the ages since the snake first hissed into Eve’s ear, offering her a bite from the fruit of the Tree of Knowledge.

I have been assailed by claims of superior expertise since my book *The River*¹ (which proposed that an experimental oral polio vaccine, or OPV, might have sparked the AIDS pandemic) first appeared in the autumn of 1999. This is perhaps not altogether surprising, in that I am a writer, not a scientist, and there are very many scientists who surely should know more about this issue than I do.

To begin with, the most prominent such “expert” was an individual, Dr John P. Moore, who has worked for many years in immunology and AIDS vaccine research. Moore has a reputation for being academically able, and for turning a good phrase. However, he has also cultivated another reputation: that of an activist who makes robust and forthright contributions to scientific debates, specifically those concerning AIDS. He is less universally appreciated in this latter role. I have heard him referred to variously as a man with “his own agenda”, and as “an attack dog for the science establishment”.²

Whatever, Dr Moore began writing responses to my book. He wrote to newspapers, he wrote to his fellow-scientists, he wrote a review for *Nature*, and another for the Amazon web site. And he claimed that wonderful thing – expertise.

What he wrote, in effect, was: I am a scientist; Hooper is a journalist; I say the OPV theory is wrong; he says it's right; who are you going to believe? (The fact that I had not been a journalist since 1987 did not deter him; what was important here was that I should be pigeon-holed and dismissed.) And then, in each of these letters and reviews, he offered a little jokey metaphor. He likened the theory that an oral polio vaccine might have started the AIDS pandemic to the theory that the moon was made of cream cheese, that there was a monster at the bottom of Loch Ness, that there were little green men on Mars. And that was it. That was the basis of his argument.

There was only one problem. Dr John P. Moore might legitimately be considered an expert in AIDS vaccine development. But he had absolutely no expertise concerning how AIDS might have begun. He had never had anything published about it; had never (as far as can be discovered) spent any time researching it. His extensive knowledge and experience in other areas of AIDS research should have provided him with some decent perspective. But when his detailed arguments on the origins debate were revealed, such as they were, they were not impressive.

However, because he was a scientist, with letters after his name, and because he had worked in the field of AIDS for several years, he felt he had the right to claim that he was right. And many other scientists believed him.

In March 2000, the leading scientific proponent of the OPV theory, the universally respected evolutionary biologist Bill Hamilton, died tragically after a second expedition to the Congo to try to collect data that might throw more light on the origins of AIDS debate.

Almost as soon as Bill was interred, the experts emerged in droves. They let it be known that the theory would soon be dismissed by compelling scientific arguments. They effected a postponement of the Royal Society conference on “Origins of HIV and the AIDS epidemic”, in order that “more scientific data could be collected”, and during that postponement the balance of the meeting was adjusted a little here and there, so that the expertise of certain favoured colleagues could be presented in a more favourable light. And then, soon after that meeting, they claimed that they had looked at the OPV theory freely and fairly and that, through their expertise, had discovered some fatal flaws. Later, one of their number (Professor Robin Weiss, one of the organisers), even claimed to much publicity that the theory had been “destroyed”.

How did he know? He knew because he, too, was an expert.

Dr Weiss is a highly intelligent man. However, if you care to spend a little time looking at his arguments on the origins of AIDS from a purely scientific perspective, many of them are not quite as persuasive as they initially seem. Almost invariably they are dressed up nicely, but they sometimes contain more assertion than true science. In fact, on closer examination, some of these arguments seem little better than those propounded by his former student, the aforementioned Dr John P. Moore.

I am not an expert. However, I have studied different aspects of AIDS for sixteen years now, for the last twelve of which I have looked fairly seriously into its possible beginnings. During that time I have been lucky – I have had many tutorials from wise and knowledgeable scientists. I have educated myself as best I can by interviewing many of the protagonists, and by reading everything relevant I could get my hands on, sometimes several times. Those who have read *The River* with open minds – and they are, I believe, the majority of its readers – generally recognise that I am, at the least, an honest and painstaking researcher. I believe that I am well-steeped in the facts and arguments pertaining to this debate. But I do not call myself an expert.

In fact, I have learnt to be wary of those claiming expertise. They aren't always what they seem to be. Especially if they claim that they have *proved* themselves right, when actually they have not. Especially when one of them (Professor Weiss) begins to get a little wild, and starts to imply that the other party (myself) is not only a non-expert, but that he is also deluded, or dishonest, or that he has invented witnesses. When people start to tell untruths like that, then the whole process becomes something else again. It becomes a piece of political theatre, a charade.

In this paper, I (the non-scientist, the non-expert) am proposing a controversial hypothesis, but one which I believe to be supported by good science. Many of the experts (like doctors Weiss and Moore) dispute this. And so what I have to do is not the easiest of tasks. I have to try to persuade my readers that the experts (well-intentioned though many of them may be) are not necessarily always right.

The truth is that Dr Weiss has grown used to taking on the role of the arbiter, the judge who weighs all arguments carefully and then determines which is right, or which is more likely to be right. But, like all men and women, he has his own favourites, his own prejudices, his own bias. And over the years he has grown used to wielding considerable power behind the scenes – to being both a scientist and a politician. It took me quite a while to realise it, but some of his arguments are based as much on political pragmatism (and on working for what I suspect he considers to be “the greater good”), as they are based on purely scientific reasoning. This is something to which I shall return later in this paper, but it is something which, I think, needs to be borne in mind from the start.

So I would invite all those who really care about the truth in this matter, who still believe that the origin of AIDS is an important issue (and one that can teach us much), to lay aside preconceptions about who has letters tagged on their name and who has not; about who has been published in *Nature* and who has not. And instead to listen again, as if for the first time, to the arguments that the different sides are able to put forward. And then decide – what makes sense? What can be supported? And what is based merely on an assertion of superior expertise?

2. Background: the origin of HIV-1 Group M: transfer via African cuisine, or modern medicine?

By 2002, the AIDS pandemic had caused the deaths of over 20 million people world-wide, and the infection of more than 60 million with the causative virus, HIV-1 Group M.³

In July 2002, UNAIDS issued a report which estimated that 68 million persons would die in the world's 45 most affected countries in the next twenty years. No estimate was advanced for the rest of the world, although it was noted that in many of these "lower-risk countries", HIV had now moved beyond specific risk groups, and was spreading at an accelerated rate in the general population.⁴

The syndrome now represents the gravest threat to human health in recorded history. But what started this dreadful outbreak of a disease which has swept across the planet, but which was unrecognised just 21 years ago?

a) The two main hypotheses.

Professor Bill Hamilton probably had the highest standards of probity of anyone I have ever known. He was (as is widely recognised) softly-spoken, modest and generous to a fault. But there was also another side to Bill – less familiar save to those who knew him well. He could be assertive (sometimes explosively so) when he felt that a wrong was being done, or that an untruth was being presented as a truth. I am confident, therefore, that Bill would not disapprove of the robust paper which follows.

Between 1993 and 1999, Bill and I worked together closely on the question of how AIDS might have started: I collected new information and evidence, while he provided comment, guidance and good counsel. We knew each other well.

In August 1999, after nine years of research and writing, *The River* was published, and near the start of the book I examined more than fifteen hypotheses about how the AIDS pandemic might have begun.

I proposed that the field could actually be narrowed down to two competing theories – one involving a "natural" zoonosis, and the other involving an iatrogenic (or physician-caused) event. Both theories sought to explain how humans had become infected with the simian immunodeficiency virus (SIV) of the common chimpanzee (*Pan troglodytes*), which for several years now has been widely recognised as the only really close ancestor to the major HIV-1 variant, Group M.⁵

I referred to the more widely accepted theory of origin as the "natural transfer" or "cut hunter" theory. As espoused by its leading proponents,⁶ this theory proposes that someone who hunted, skinned, ate or played with a chimp, became infected with chimpanzee SIV (SIVcpz) in or around the 1930s, somewhere in the west central African region of Cameroon, Equatorial Guinea, Gabon and Congo Brazzaville. It further proposes that the initial infectee, or someone infected by him/her, ended up not long afterwards in Leopoldville (now Kinshasa, the capital of the DRC, the Democratic Republic of Congo). There, the theory goes, transmission to other parties began, and the new human virus then began to spread up the river Congo to infect people in other parts of the DRC and, by the very end of the seventies, in other parts of Africa.⁷

The most plausible iatrogenic theory of origin, and the one which I came to favour, is called the oral polio vaccine (OPV) hypothesis. It proposes that the AIDS pandemic was sparked by the vaccination of approximately one million Africans with an experimental OPV called CHAT. The 27 known African trials⁸ of CHAT vaccine

occurred between the years of 1957 and 1960, in the central African countries now known as the DRC, Burundi and Rwanda. In the fifties, these lands comprised a colony (the Belgian Congo) and a trust territory (Ruanda-Urundi) administered by Belgium.

By the latter half of the 1950s, virtually all polio vaccines were prepared in a substrate of so-called “monkey kidney tissue culture”. Most of the major polio vaccine developers – such as Sabin, Salk, Cox, Lépine and Gear – identified the species they were using in one or more of their early publications. The first three used rhesus and cynomolgus macaques from Asia; Lépine mainly used Guinea baboons from West Africa, and Gear used vervets (a type of African green monkey) from South Africa. Alone of the major manufacturers, the developer of CHAT vaccine, Hilary Koprowski, never revealed the species of primate he had used to prepare CHAT in any publication of the fifties.⁹

For several reasons, I began to suspect that Koprowski and/or his collaborators might have used cells from the common chimpanzee to make some of the CHAT vaccine batches fed in Africa.

The first reason was that a short while before he developed CHAT, Hilary Koprowski had inaccurately reported the substrate in which he had prepared his previous Type 1 polio vaccine, SM N-90. In three articles published in 1956 and 1957, Koprowski reported that he had been using a tissue culture of chick embryo, when in fact he had been using one of monkey kidney.¹⁰ The reason for this misrepresentation is still not clear.

A second reason was that, at the time he developed CHAT, Koprowski – in collaboration with researchers from the Belgian Congo – had just opened a huge chimpanzee research station (*Mission Courtois Koprowski: Centre d'Experimentation*)¹¹ at a place called Lindi, about 15 kilometres outside Stanleyville (now Kisangani). There are virtually no details in the published literature about the polio work which was carried out at Lindi camp, and despite claims to the contrary,¹² that research was conducted under a veil of secrecy.¹³ However, before long I learnt that more than 400 chimpanzees had been present at the camp during the time of the polio research, in the first twenty months of its existence (June 1956 to February 1958). By the time it closed, in December 1959 or January 1960, more than 600 chimpanzees may have been utilised in the Lindi research.¹⁴ It is reported that 86 of these were pygmy chimpanzees (otherwise known as bonobos);¹⁵ the rest were common chimps.

But the most compelling reason involved the epidemiology of HIV and AIDS. Quite early in my research, I began recording the earliest cases of AIDS, both clinically plausible and serologically confirmed, which could be found in the medical literature, or else in documents such as doctoral theses. The following data have been updated to include those cases that have recently come to light.

Apart from one early cluster involving a Norwegian sailor and his family (all of whom turned out to have been infected with a minor variant of HIV-1, Group O, in the 1960s),¹⁶ the first plausible evidence of AIDS outside Africa (in the U.S., Europe

and Haiti) emerged in the year 1978.¹⁷ All these latter cases seemed likely to have been caused by the pandemic strain of HIV-1, Group M.

But within Africa, the epidemic began *at least five years earlier*. I was able to document fifteen clinically-defined African cases in which first symptoms appeared between 1973 and 1977, as well as one other case dating from 1962.¹⁸ (I shall return to the question of other possible early AIDS cases, from the sixties and fifties, later in this paper.) Interestingly, every one of the sixteen cases from 1962-1977 involved people from the DRC, Burundi and Rwanda, or else foreigners infected there.¹⁹

For the period 1978-1980, I documented 23 further African cases, mostly from the same three former Belgian colonies.²⁰ It was only in the latter two years that a few cases began emerging in adjoining countries, like Uganda, Tanzania and Zambia.²¹ About a quarter of these 39 clinically-defined AIDS cases were serologically confirmed.

It was in the middle of 1992, some time after I had carried out the greater part of the above research, that I first heard about the OPV/AIDS hypothesis, which was then being propounded by independent researchers such as Louis Pascal, Blaine Elswood and Tom Curtis.²² Some further research in Belgian archives revealed that many of the places where CHAT had been administered in Africa were, ominously, the very places where HIV-1(M) and AIDS had first appeared. **[Figure 1]** However, there were no such correlations in Europe, where more than eight million people had been fed with CHAT, in countries which had not witnessed any early cases of AIDS.

Some years later, I plotted out the vaccination sites, together with early African AIDS cases up to and including 1980, on a map of the old Belgian colonies, and discovered a remarkable fact. Thirty one of these cases could be linked to a specific city, town or village, and of these, 68% came from places where this experimental vaccine, CHAT, had been fed in the 1957-1960 period. **[Figure 2]** By contrast, 42% of the early cases came from Kinshasa, the city that features in both major theories of AIDS origin. (It was a CHAT vaccination site, but it is also viewed as a hub in the natural transfer theory.)

I then mapped the earliest serological evidence of HIV-1 Group M infection in Africa for the years up to and including 1981. Altogether there are sixteen sites in Africa for which we have evidence that HIV-1 Group M was present between 1959 and 1981,²³ and nine of these sixteen sites are places where CHAT vaccine was fed in 1957-1960. **[Figure 3]**

Through 1981, there are 70 instances of African Group M infection that can be linked to a city, town or village, of which 30% come from Kinshasa. However, there is a far stronger correlation, for 76% of these infected sera were obtained from places where CHAT vaccine was fed in the late fifties.

On the basis of such correlations, and of considerable further circumstantial evidence, I proposed in *The River* that batches of the vaccine fed in Africa *might have been prepared* in chimpanzee cells which contained SIV, and that the 27 CHAT vaccination campaigns conducted before independence in the DRC, Rwanda and

Burundi might have allowed chimpanzee SIV to infect one or several of the vaccinees.

b) The historical, scientific and political debates.

As I say, *The River* was published in August 1999, and by December of that year it was evident that the book had sparked a major controversy.

During the last two months of 1999, Bill Hamilton approached the Royal Society in London asking it to host a conference about the origins of HIV and AIDS, and the implications for modern medicine. (At around the same time, he was approached by the *Accademia dei Lincei*, and invited to explain his point of view at a similar conference.) Before he left for Kisangani in January 2000, the London conference had been approved, and two co-organisers appointed. What happened after his tragic death in March 2000 is a complicated story, and one that will be touched on later in this paper. Suffice it to say that after Bill's demise, the balance of the meeting became subtly but irrevocably tilted against the OPV theory.

When the London meeting took place in September 2000, the results of the testing of certain archival CHAT samples released by the Wistar Institute were announced: they were found to contain neither HIV, SIV nor chimpanzee DNA. In vain did I argue that the tests were relatively meaningless, because there was nothing to indicate that any of the samples which had been released, and tested, came from batches that had been prepared for use in Africa.²⁴

Most of the scientific and lay press overlooked this point, and concluded that CHAT vaccine had been vindicated. These conclusions were based to a large degree on a press conference which was staged immediately after the announcement of the Wistar test results, and on Professor Weiss's closing speech at the conference – and they were further reinforced seven months later, when *Nature* and *Science* published three brief formal reports of the CHAT vaccine testing, together with a short theoretical article about Group M phylogenetics.²⁵

“Disputed AIDS theory dies its final death”, ran the headline in *Science*.²⁶ “Polio vaccines exonerated”, was the title of Robin Weiss's commentary in *Nature*, which ended with a memorable sound-bite. “Some beautiful facts”, wrote Weiss, “have destroyed an ugly theory”.²⁷

I believe this was an irresponsible claim, for very few relevant facts (beautiful or otherwise) had been presented, and no theory had been destroyed.

There is a reason why I can write this so confidently. By a strange quirk of fate, just two weeks before Dr Weiss published his comments in *Nature*, I was visiting Kisangani, where (much to my surprise) I learned that, more than forty years earlier, CHAT vaccine had been prepared in the local medical laboratory – and almost certainly in a culture of chimpanzee cells.

By itself, this new evidence could be presented in the space of a few pages. However, so much disinformation and untruth has now been written in response to the OPV hypothesis, that the waters have become comprehensively muddied. Indeed, I believe

that a deliberate attempt has been made to obfuscate the issues, and that it is important that this obfuscation should be revealed for all to see. So, in order to answer the various claims (serious and spurious) which have been made, and to clarify some of the issues that have caused confusion, I have had to enlarge dramatically the scope of this paper. I have decided to present a great deal of new material, and I shall now be examining not just the scientific arguments, but several other aspects of the debate as well. I am grateful to the Lincei academy for giving me the opportunity to present such a detailed paper.

Unravelling the truth of what actually happened in the Belgian Congo nearly fifty years ago has not been an easy or straightforward process. Throughout the last decade, I have approached it through painstaking research, careful cross-checking and, above all, by a conscious attempt to avoid jumping to premature conclusions. (Doctors Plotkin and Koprowski claim that I have done exactly the opposite.)

Although I have looked at relevant materials from several different fields of science, it is not only science that has provided relevant information. Other important evidence has come from historical archives and testimonies, and, because the debate has become steadily more politicised in recent years, more again has come from what might be termed the political arena.

The remainder of this paper will therefore present the case for the OPV/AIDS hypothesis in three separate sections, relating respectively to the historical, scientific and political debates about how AIDS began.

3. The historical debate: was CHAT made in chimp cells?

This section will feature a large amount of newly-gathered historical evidence, much of which has only come to light since I delivered my address at Lincei in the autumn of 2001. This new evidence (much of it from Africa and North America) has allowed several key pieces of the historical jigsaw to slot into place, and has (I believe) moved the hypothesis that CHAT was made in chimp cells from the realms of the possible to those of the highly probable. The process has also revealed the first few pieces of a secondary jigsaw, one for which far fewer pieces have been located. It remains to be seen whether enough new pieces of this secondary jigsaw come to light to move the subsidiary hypothesis (that other cells may also have been involved with the preparation of the Congo vaccines) from the realms of the possible to those of the likely.

On my first visit to Kisangani, with Bill Hamilton in June 1999, I only managed to locate one significant witness to the events at Lindi camp, forty years earlier. However, on my second visit to Kisangani, in April 2001, I was present in the city for a longer period of time, and enjoyed more success in tracking down people who had participated in, or witnessed, the events that took place in the fifties.

[*A brief note on names.* In the revised postscript to *The River*, I referred to the 1999 witness from Lindi as “Antoine”, clearly indicating by the quotation marks that this was a pseudonym.²⁸ Dr Plotkin has since disingenuously reported that a search by African doctors in Kisangani failed to reveal an “Antoine” who fitted the bill, adding that this discovery “leaves open the question of where, how or whether the interview

was done”.²⁹ This false implication that I invented a witness does Plotkin no credit, although it is not atypical of the approach he has used in responding to *The River*.³⁰ The main reason why I avoided giving “Antoine’s” real name, was that I suspected that pressures might be brought to bear on him to modify his account – and indeed, this is exactly what has happened with other witnesses to the events recounted in *The River*; (for examples, see below). In an attempt to forestall this, I am continuing to refer to him, and to several of the other African witnesses, by pseudonyms or by descriptions only (e.g.: Osterrieth’s first assistant) in the text that follows. The identities of all these persons will be revealed at a later date.]

a) The Lindi camp nurse.

First I came across Joseph, the so-called “nurse” of Lindi camp, the man who had helped the Belgian doctors perform autopsies on the chimpanzees. (This came as a very pleasant surprise, because in 1999 I had been informed by “Antoine” that Joseph had died in 1964.)³¹ Joseph had been the key indigenous worker at Lindi camp, and was treated as boss by the other Africans who worked there. It was he who had sacrificed many of the chimps, opened them up, and had then carried out most of the initial gross dissections.³² He said that altogether, in the three and a half years of the camp’s existence, Lindi had housed over 600 “chimpanzees”, a term which includes both common chimps and bonobos; [see below].³³ He said that the chimpanzees were held in cages in two large hangars, and that all were eventually sacrificed, apart from the relatively small number which died of natural causes, and apart from the final 60, which were taken elsewhere when the camp closed.

Joseph said that he himself had probably sacrificed some 500 chimps. Sometimes he killed two or three in a single day. The work, he said, was secret: nobody outside the camp knew what was going on. There were some policemen based at the camp to make sure people stayed away.

For most of this time, he said, the doctor performing the autopsies had been Paul Osterrieth, the head of the new virus laboratory that had opened in Stanleyville in September/October 1957. During these autopsies, which were carried out on a table behind the cages in the second hangar, the major organs (including the kidneys) were taken away in metal containers – and were then, he thought, sent abroad, mostly to America. He said that the kidneys were always taken, as were large quantities of blood. Afterwards, he said, he had to pour poison on what remained of the corpses, so that local people would not eat them.

Many of the significant details of Joseph’s testimony were supported by other witnesses – including “Antoine”, who also added some other important details. During a further interview given in 2001, “Antoine” once again mentioned that there had been a group cage for the younger chimps. And he reiterated that organs and blood had been taken from the chimps while they were anaesthetised but before they were killed. The one documentary record that exists of the process of sacrificing chimps at Lindi to obtain kidneys also mentions that the kidneys were removed using “aseptic precautions”,³⁴ clearly to minimise the possibility of bacterial contamination.

These observations by Joseph and “Antoine” are crucial, because contemporary references confirm the importance of these procedures (the bleeding of animals, and

the taking of organs from anaesthetised animals, before sacrifice) when organs such as kidneys were being removed specifically for tissue culture work. One example features in a 1955 article from the von Magnuses, who carefully describe how they prepared the Danish polio vaccine (an IPV) that was used in 1955. “The kidneys”, they wrote, “are removed aseptically from monkeys exsanguinated under sodium pentobarbitol anesthesia.”³⁵

b) The assistants at the Stanleyville virology laboratory.

Perhaps the most important part of the new *Laboratoire Médical de Stanleyville* building which opened in Stanleyville in September 1957 was the virology department (or, as it was commonly known, the virology lab, or virus lab). Over the next few days, I spoke with two of the men who had worked in this virus lab, under Dr Paul Osterrieth, between 1958 and 1960.

The first of these assistants³⁶ had been speaking about his former boss for several minutes when he quite casually volunteered: “and he was also making the polio vaccines in the laboratory”. Hiding my astonishment, I asked how this had been done. To begin with, the assistant responded by talking about Osterrieth’s visits to Lindi camp to take blood from the chimps, and his spinning the blood down into serum when he got back to the lab. I asked him again how Osterrieth had made the polio vaccine, and he replied: “I was just sterilising the materials in the lab. What he was doing with that blood to make vaccine I don’t know.” He went on to explain how he used regularly to accompany Osterrieth to Lindi on Saturdays, when the two of them would take blood from the chimps, using different syringes for each animal. He knew nothing about autopsies or the taking of organs, saying that it was the camp workers at Lindi who should be asked about those subjects.

In another answer given later in the interview, the assistant once again linked the making of chimp serum to the making of polio vaccine. He was unable to provide further details, but the very fact that the two events were associated in his mind seemed significant.

When I asked for more details about the occasions when the vaccine was made, he said that what happened first was that an order for more vaccine would come from the “provincial director”. After that, he said, Osterrieth would start preparing the vaccine, often staying behind in his lab in the evenings, after the other workers had gone home. (This was Osterrieth’s own lab, he added, the one which was kept sterile, and which nobody else was allowed to enter. This was also where Osterrieth used to take the bloods from the chimpanzees to spin them down into sera.) Osterrieth was not making the vaccine all the time, he said. It was only after the orders came from the provincial government that he would do so. The taking of serum, by contrast, happened on a regular basis, which suggests that although the vaccine-making may have required serum, this was probably not the only reason that serum was taken.

Later, when Osterrieth had finished preparing the vaccine, the assistant would help transfer it into smaller bottles, which sounded like phials. There was a “machine” to help them do this, he said. Sometimes he (the assistant) also helped administer the vaccine that Osterrieth had made – and this was always done by mouth. Apparently

the vaccinations continued up to 1960, and he added that the other doctors in the lab all knew that Osterrieth was making the polio vaccine.

One particular vaccination the assistant recalled was that at Lukusa, the military camp of Stanleyville, on the south side of the river, where he helped feed the vaccine. This was significant because, unusually, the vaccination at this site was documented in a letter in the Belgian government archives. Over 3,000 men, women and children were vaccinated here on February 27, 1958 – this being the eighth anniversary of the date when Hilary Koprowski, the developer of CHAT, became the first scientist to feed an OPV to a non-immune subject.³⁷

The assistant had started working at the laboratory on February 12, 1958, and he emphasised that Osterrieth had already been making the polio vaccine before he arrived there. Osterrieth had been away on leave from July 1957 until about February 4th, 1958, so this indicates that he must have been preparing vaccine from around the time of his return to the lab, a week or so earlier.

The assistant also said that Dr Osterrieth used to send reports about the vaccinations overseas, and he recalled that Osterrieth made two further trips abroad before he finally left in 1960.³⁸

Shortly after my return from Africa, the proceedings of the Royal Society meeting were published,³⁹ and they included a contribution by Dr Osterrieth, entitled “Vaccine could not have been prepared in Stanleyville”.⁴⁰ In this article, and in statements quoted in articles by Stanley Plotkin,⁴¹ Dr Osterrieth made a series of definitive statements, some of which were clearly responses to different parts of *The River*.⁴²

He stated that after receiving training in tissue culture techniques in the USA between October 1957 and January 1958, he returned to Stanleyville in February 1958 with the aim of setting up a cell culture laboratory. He said that as far as he recalled, it took several months before he succeeded in producing cultures from baboon kidneys and from HeLa cells. He added that he tried to make tissue culture from the kidneys of other small monkeys, but failed, and that trypsin was uniformly used to disperse the cells. He stated that when he was not present in his laboratory, the room was locked, for fear of contaminating the tissue culture, and that nobody else had access to the virology lab. He stated that autopsies of chimps were never done inside the main medical laboratories, and went on: “I have no knowledge of polio vaccine being diluted or distributed into smaller flasks at the Stanleyville laboratories, and in any case it was never done in my laboratory.” He also stated: “There is no possibility that chimpanzee cells could have contaminated the vaccine that was produced elsewhere.”

He ended his statement to Stanley Plotkin with the following passage: “[A]t no time did I ever attempt to make cell culture from chimpanzee tissues. In addition, I wish to state categorically that no poliovaccine was ever produced or could have been produced in Stanleyville, since the facilities were totally inadequate for a production or control of poliovaccine.” (It is unclear whether the italics were contributed by Osterrieth or Plotkin.)

How are we to resolve the basic differences between these two accounts from the director of the Stanleyville virology laboratory and his assistant? Should we be more

impressed by Osterrieth's account, because it comes from a Western scientist, rather than an African lab technician?⁴³ Or should we compare the two accounts, and also, where possible, compare them with accounts from other sources? I propose to adopt the latter approach, which clearly seems fairer.

Firstly, it should be noted that there may be some confusion about terminology. According to his assistant, there was a sterile room within the virology lab which was always kept locked, and it was here that Dr Osterrieth did most of his work – like centrifuging the chimp blood to produce sera. So, when Paul Osterrieth writes about “my laboratory”, he may be referring either to this locked sterile room, or to the virology lab as a whole. It seems very probable that the sterile room is the same place as the lab which, according to Osterrieth, was kept locked to avoid contamination of the tissue culture.

Dr Osterrieth denies ever having handled polio vaccine in his laboratory. However, the assistant not only states that Osterrieth himself regularly handled polio vaccine in the lab, but he recalls his boss preparing it in the sterile room on several occasions, whenever an order came down from the provincial government.

Osterrieth says that the lab facilities were totally inadequate for the production and control of polio vaccine, but there is substantial evidence to counter this claim. For instance, a Leopoldville newspaper article from August 1958 specifies that Koprowski's polio vaccine had been both “prepared” and “controlled” in the Belgian Congo, and that the control, at least, had been carried out at the lab in Stanleyville.⁴⁴ Apart from illustrating the inaccuracy of Osterrieth's claim about controlling the vaccine, this also highlights the fact that it is not *production* that is at issue here, for the initial production step for CHAT had already been carried out in the U.S. The key issue is whether batches of CHAT could have been *prepared* locally – and it is becoming clear that this was quite a straightforward process, which could be achieved simply by placing a little of the existing vaccine into a new tissue culture.

The fact that the vaccines made by Osterrieth were fed by mouth is vitally important, because the only vaccine administered orally in the late fifties was OPV, and the only OPVs reported in the Congo at that time were the Koprowski vaccines, CHAT and Fox.⁴⁵

A document in the Belgian government archives reveals that CHAT was fed to 3,102 individuals at Stanleyville military camp on February 27th, 1958. Koprowski's type 3 vaccine, Fox, was fed to 3,131 people at the same camp on May 27th, 1958.⁴⁶ We don't know for sure whether the assistant helped with the February vaccination, or the May vaccination, or both. The important detail, however, is his evidence that Osterrieth was already making the vaccine in early February 1958, which confirms that *CHAT vaccine*, at least, was prepared and fed locally.

The account of new vaccine orders coming periodically from the “provincial director” corresponds nicely with a previous account provided by Dr Ninane, who talked of requests for vaccine coming in sporadically from local doctors – requests which clearly would have been channelled through the provincial medical directorate in Stanleyville. It also ties in with the annual reports of the Stanleyville medical laboratory. These reveal that during 1958 responsibility for “the control, storage and

distribution” of all vaccines passed from the medical laboratory to the Hygiene Department, and that its director, Dr E. Peeters, advised the *Medecin Provincial* on such matters. This transfer clearly happened after the responses to polio epidemics in *Province Oriental* in January and February 1958, and after the Ruzizi Valley campaign in February to April, in both of which the doctors from the medical laboratory played the leading role. The Ruzizi trial actually took place outside *Province Oriental*, but Courtois helped organise the programme, while Ninane took charge of several of the feedings.⁴⁷

The testimony of the first assistant in the virology lab contains a number of precise details, which are all the more impressive because they come from someone who is not himself a specialist. Clearly he did not dream up these details, nearly all of which are corroborated by other sources. Examples include the fact that vaccine orders came in from the “provincial director”, that Osterrieth sent reports about the vaccine overseas, that vaccine was transferred into smaller bottles, that it was fed by mouth, and that it was administered at the local military camp. Throughout the course of two interviews the assistant spoke quietly, and yet confidently. I do not believe his testimony can be “explained away” as the product of a faulty (or over-imaginative) memory. Furthermore, there is no reason for his statements to have been fabricated.

By contrast, many of Dr Osterrieth’s various statements would appear to be qualified in one way or another, or else are capable of different interpretations – such as the references to “my laboratory”, or to “production and control of poliovaccine”. The main exception would seem to be the categorical and italicised denial that Dr Plotkin has reprinted.

At this point, let us turn to Lindi. I now have the detailed testimony of three African witnesses who spent time at Lindi camp, who say that Dr Osterrieth was a regular visitor to the camp, and that it was he who did most of the autopsies, and who carried out most of the extractions of organs and blood from the Lindi chimpanzees. By contrast, Dr Osterrieth has minimised the importance of his own role at Lindi camp. He has never acknowledged that the research involved the sacrifice of chimpanzees. Furthermore, he has never vouchsafed how the organs such as kidneys were obtained (for instance for Dr Deinhardt’s hepatitis work). In short, Dr Osterrieth has consistently avoided answering specific questions about the work conducted at Lindi.

There are further question marks, also. One of Plotkin’s articles quotes Osterrieth as stating that: “I never tried to dilute the polio vaccine that was received”. The article then reveals that Dr Ninane had apparently made the same statement, word for word, just six days earlier: “I never tried to dilute the polio vaccine that was received”.⁴⁸

On one level, this raises questions about the ways in which these statements were obtained. But it also begs a question – that of whether Osterrieth (and/or Ninane) perhaps diluted a *different* variety of polio vaccine – not the one “that was received” from abroad, but one which had been prepared locally by further passage in tissue culture. (As discussed more fully below, this would essentially conform to the normal process which took place in labs that received polio vaccine from overseas in the fifties.)

Two other interviews need to be mentioned. The first was with another of Osterrieth's lab assistants, who joined the lab in December 1958. Like the first assistant, this second man recalled that Dr Osterrieth had made vaccine, but said he could not recall which one it was. I wanted to avoid any possibility of prompting him, and so changed the subject for a while. Earlier, he had mentioned that he had recently had some news from Dr Osterrieth, so I asked him about this. He explained that a few months before, he had received a letter from Dr Osterrieth, the first communication for some 40 years. Why had his former boss written, I asked? Apparently Osterrieth had wanted to know which of his former African assistants at Lindi camp, and at the Stanleyville virology laboratory, were alive – and which were dead.

Later, I asked the second assistant if he had helped with the polio vaccinations that were taking place at around this time, and he became perceptibly nervous. Eventually, he answered: “yes, but I don't know well. I have forgotten. But I worked, I worked”. Later, he added that he had helped vaccinate children in Kisangani town during 1959, but could add no further information. 1959 was when the whole population of Stanleyville, some 15,000 people, was vaccinated with CHAT.

A further interview involved a technician who had worked under Ghislain Courtois in the microbiology department at the old *Laboratoire Medical de Stanleyville* from April 1956 onwards. The key revelation came when the assistant revealed that they had indeed been making tissue culture in the microbiology lab, and that he himself had prepared culture media..

When asked from which animals these tissue cultures had been made, he answered “*surtout des chimpanzés*”, or “mainly chimpanzees”. He gave this answer confidently and immediately. Later, he said that they were also making tissue cultures in Dr Ninane's histopathology department.

When asked how they were making these cultures, he said that it would be better to ask Joseph (the Lindi camp nurse), because they were doing autopsies of the chimpanzees at Lindi camp. Later, he added that they would take the lungs, the heart and other organs from the chimps, and put them into flasks, and then work with them, but he wasn't sure of the precise details. I decided not to press him with further questions on this issue because it seemed clear that he had given what he had to give – and I did not wish real memories (which these clearly were) to get confused by well-intentioned “attempts to remember”.

Courtois' assistant said that Dr Osterrieth had worked for a time in the microbiology lab, and had then left to become head of the virology lab (where, as we know, he took over after his leave, in February 1958). He said he did not know what they were doing in the virology lab, although he stated that the work on the poliovirus had involved chimpanzee blood and sera.

The information supplied by Courtois' assistant is highly significant, in that it is the first unqualified confirmation from someone working in the Stanleyville medical laboratory that (a) tissue cultures were being made locally in that lab, and (b) that they were being prepared from chimpanzee organs.

The fact that he did not know the precise details of tissue culture manufacture is frustrating, but not entirely surprising, because (as is discussed elsewhere), the Belgians did not routinely hand on knowledge, or train their African workers in different lab techniques. Indeed, some of the Africans I spoke with felt that, at least in some instances, they were deliberately left in the dark about what was going on.

However, we do have one brief, but extremely telling sentence from his old boss, Dr Courtois, which goes a long way towards confirming his account. At a conference about the use of primates in the laboratory which was held in Lyon, France, in December 1967, two French doctors delivered a speech entitled “Monkey cell cultures in virology”, in which they stated that some twenty primate species had been studied for the purpose of making tissue culture, adding that, in addition: “some laboratories may have used or regularly use species without disclosing the fact in publications”.

In the discussion that followed, someone commented: “Some laboratories, instead of killing monkeys themselves, obtain refrigerated kidneys from another laboratory. Is this satisfactory or wasteful?” Ghislain Courtois responded: “More than 10 years ago we sent kidneys from the Congo to Europe and they were quite satisfactory.”⁴⁹

More than ten years ago (note the careful phrasing) means before December 1957, and the response by Courtois’ assistant, that the tissue culture in Courtois’ lab was made mainly from chimps, strongly suggests that Courtois was dispatching *chimpanzee* kidneys for tissue culture work. So by 1957 (at the latest) Ghislain Courtois was apparently sending chimpanzee kidneys to Europe (which most likely means to Belgium). It seems likely that the making of chimp cultures in the microbiology lab which he headed would have been going on at the same time.

The new, concrete, custom-built Stanleyville medical labs opened officially in September 1957, but Lindi camp opened more than a year before that, in June 1956. So a further possibility that needs to be considered is that the making of chimp cultures may have started even earlier, back in the old redbrick medical laboratory in 1956. (Indeed, it may be that Courtois’ “more than ten years ago” comment should be read less conservatively – as “*at least eleven years ago*”, which would indeed put the date for his handling chimp kidneys back to at least 1956.)

At this point, we have to confront the problem of which versions of events are right: those of the two Belgian doctors, Ninane and Osterrieth, or those of five different African witnesses.⁵⁰

The testimonies of the African witnesses sometimes conflicted on minor details, but they corresponded convincingly on the major points. By contrast, Osterrieth and Ninane contradicted each other on what was, in effect, the key point. Each of them, at different times, claimed that he was the only one in the Stanleyville medical laboratory who had ever tried to make tissue culture.⁵¹

What is even more telling, however, is the fact that Dr Osterrieth’s own accounts given at different occasions over a period of seven years have featured gaps, anomalies and internal contradictions. Indeed, on the question of whether chimp kidneys were ever sent abroad (and if so, to where), Dr Osterrieth has now given three fundamentally different versions of events.⁵²

Taking all this into account (and having listened again to the original tape recordings of my interviews with him in 1993 and 1994), I now believe that Dr Osterrieth's memories cannot be relied upon, at least with regard to the key issues such as making tissue culture and vaccine, and the Lindi research. Neither, I believe, can the memories of Dr Ninane be relied upon on these key issues.

c) Making tissue culture in Stanleyville.

So, let us assume for a moment that Dr Osterrieth's first assistant is correct in his confident account, and that oral polio vaccine *was* being prepared in Stanleyville. Do we have any indications at all about how it might have been made? Paul Osterrieth believes that it was only several months after February 1958 that he "could attempt tissue cultures using trypsinisation". Fair enough. But what about cell cultures that did not require trypsin?

By good fortune, we happen to have a precise description of how a non-trypsinised tissue culture was prepared in Stanleyville. This features in an internal report published by the U.S. Armed Forces Epidemiology Board (AFEB) in 1959.⁵³ The AFEB report concerns the research into hepatitis in chimpanzees carried out by a Philadelphia virologist, Fritz Deinhardt, who arrived in Stanleyville on February 1st,⁵⁴ 1958, and stayed until the end of April.⁵⁵ He apparently collaborated with both Ghislain Courtois and Paul Osterrieth (in the microbiology and virology departments of the new laboratories), and he spent a lot of time at Lindi camp.

In order to continue his hepatitis research *in vitro* after he had returned to the US, Dr Deinhardt decided to transport chimpanzee cells back to Philadelphia. This was facilitated by the fact that, as explained in the AFEB report, there was already an existing sacrifice programme which he was able to plug into.

The passage in question reads: "*Several chimpanzees used for poliomyelitis studies at the Lindi camp had to be sacrificed at intervals. The doomed animals were bled one week prior to sacrifice, and the serum separated. The kidneys were removed under aseptic precautions, and transported to the laboratory in containers filled with Hanks' solution. They were then minced and to the washed pieces was added 5% isologous serum in Hanks' solution. These preparations were shipped in an insulated box without refrigeration to Philadelphia, where they arrived within 3 to 5 days. In spite of the long sojourn the tissue was viable and 3 of 4 specimens yielded, after trypsinisation, excellent cultures.... None of the cultures revealed evidence of foamy agents or other ECCO viruses.*"

This account has been confirmed and broadened by other testimonies (including those of Paul Osterrieth, during our first interview), and by a paper which appeared in the early sixties, which enlarged the number of chimp kidney shipments related to the hepatitis research from four to six.⁵⁶

In his article published in the Royal Society proceedings, Dr Osterrieth refers to this episode as follows: "It is true that six minced chimpanzee kidneys were sent to the Wistar Institute at the request of Fritz Deinhardt, who came to Stanleyville to

experiment with hepatitis infection of chimpanzees. Although I do not remember exactly when that was done, no cultures were retained in Stanleyville.”⁵⁷

This statement is highly revealing in two respects. Firstly, it contradicts Dr Osterrieth’s several earlier statements that chimp kidneys had been sent only to the Children’s Hospital Of Philadelphia (CHOP), and not to the nearby Wistar Institute.⁵⁸ The last of these statements by Osterrieth (“I also want to state very clearly that I never sent chimp kidneys to the Wistar Institute”) was apparently made in February 2000, and is quoted by Stanley Plotkin in his first Royal Society paper. However, by the time of his own speech to the conference, made seven months later, Dr Osterrieth is admitting that someone had indeed been sending chimp kidneys to the Wistar. The clarification is welcome. The key point is that although the six kidney shipments were intended for Deinhardt at CHOP, *they were sent via the Wistar Institute*. This does not, of course, reveal whether *other* chimp kidney shipments were also sent to the Wistar.

However, Dr Plotkin, in his various publications, reports that none of six persons (including himself) who used to work at the Wistar as students or technicians during this period ever saw or heard of chimpanzee cells at the Institute, and adds: “there is absolutely no evidence that chimpanzee kidneys...found their way to the Wistar”.⁵⁹ Though forthright, this stops short of an absolute denial. And now Paul Osterrieth has provided the evidence that Plotkin claimed to be lacking: that minced chimp kidneys *were* sent to the Wistar Institute. In fact, a similar account had already been given by the widow of Hilary Koprowski’s chief lab assistant, Tom Norton, who recalled her husband returning from Stanleyville to the USA in March 1957 with various chimpanzee materials (including several kidneys), and handing them to the Wistar Institute driver, who delivered them to the Wistar.⁶⁰ This was several weeks *before* Koprowski and Norton officially moved to the Wistar from Lederle in May 1957 (an apparent discrepancy that will be explored later in this manuscript). Mrs Norton’s account had already been published in *The River*, so I am surprised that Dr Plotkin stated that there was “absolutely no evidence that chimpanzee kidneys...found their way to the Wistar”.

All this begs the question of what chimp kidneys might have been used for at the Wistar Institute in 1957-1958? Does this not suggest that they might, after all, have been under investigation as an experimental vaccine substrate? This seems not unreasonable, in that it is hard to imagine Dr Osterrieth taking the decision to try out chimp kidneys as a substrate for CHAT in Stanleyville, unless he had already got the explicit approval of the scientist who developed CHAT – Hilary Koprowski.

The second revealing aspect of Dr Osterrieth’s Royal Society statement is the key detail that “cultures” were prepared from chimpanzee kidneys in Stanleyville (even if he goes on to say that no cultures were retained there).

Osterrieth’s description of the minced chimp kidney preparations as “cultures” is highly significant, for it reveals that he is referring to Maitland-type cell cultures, rather than trypsinised monolayer cultures. Maitland-type cultures were simple to make, even in the fifties: the process required mixing together some minced-up kidneys, some serum and some growth medium, and then adding a few drops of antibiotics. With Osterrieth’s new testimony, it becomes clear that Maitland-type

cultures were initially made from the chimp kidney cells, chimp sera and Hanks' solution in Stanleyville, and that later at least six shipments of these cultures were forwarded to Philadelphia, and were treated on arrival with trypsin, to produce "excellent" trypsinised monolayer cultures for Deinhardt's hepatitis work.⁶¹

It should be noted that both types of culture – trypsinised and Maitland-type – were approved substrates for making oral polio vaccines throughout the 1950s.⁶²

However, Maitland-type cultures were already considered a bit primitive, the "poor country cousins". In the abstract to his paper, Osterrieth apparently confirms this by stating: "In Stanleyville, at the time of vaccination campaigns, tissue cultures were primitive, experimental and used solely for diagnostic purposes."

With that statement, Osterrieth confirms that "primitive", or Maitland-type, cultures *were* being prepared in his lab, at the same time as the polio vaccinations. On this issue at least, we appear to have consensus.

Where we do *not* have consensus is where Dr Osterrieth states that: (a) the primitive tissue cultures (from whichever species they were made) were used only for diagnostic purposes, (b) that he neither handled nor manipulated the polio vaccines in his laboratory, and (c) that he himself never tried to make cell culture from chimp tissues.

d) The who and the how....

To try to unravel these issues, one needs to reexamine the limited archival material that does exist about the Stanleyville medical laboratory, and about Lindi – and then to ask some further questions.

In February 2000, Dr Osterrieth apparently gave a categorical assurance to Dr Plotkin that "at no time did I ever attempt to make cell cultures from chimpanzee tissues".⁶³ Yet by the time he delivered his own speech in September 2000, Osterrieth was acknowledging that chimp cultures *were* made in Stanleyville. He does not mention who might have done this.

Instead of diving in to that question straight away, let us take Osterrieth's statements at face value, and move to another question. If Osterrieth did not make *chimp* cultures, then which cultures did he make?

When I first asked Osterrieth about making tissue culture in 1993, he was initially unable to remember which species he might have used, but he did emphasise that it had not been the chimpanzee. Later in that interview, after I told him that Lépine had used baboons, he said that he too was likely to have done so. (This is interesting, because there were certainly no baboons at Lindi, and there were none to be found in the nearby rain forest, except for a few which might have been scavenging along the main roads through the forest. The nearest baboon habitat was the savanna grassland, 400 kilometres to the north. Baboons, in short, were not a very practical choice for a locally-available primate.)

However, there is a single paragraph in the 1958 medical lab annual report which, at first glance, provides a substantial level of support for Dr Osterrieth's statement. This states that, in 1958, tissue culture had been "exclusively" made from baboon kidneys, and that 200 tubes and 10 bottles had been prepared. It goes on to state that 36 of the tubes were used for viral analyses, while the remainder of the tubes and bottles were used to prepare adenovirus antigen for complement fixation. However, the adenovirus did not survive being transported without ice, and so the latter tissue culture was apparently wasted.

But this raises more issues than it settles. For one thing, this quantity of tubes and bottles represents the product of a small number of baboon kidneys – probably from two to four, unless the receptacles were unusually large.⁶⁴ For another, why emphasise that cell culture had been made "exclusively" from baboons? For a third, why go to all that trouble to train Osterrieth in making tissue culture, and then report that just a few tubes and bottles had been produced, carefully adding what every tube and every bottle had been used for – including the fact that most of them had gone to waste?

This odd emphasis on the baboon tissue culture seems all the more remarkable when one considers that the annual reports of the Stanleyville medical lab for 1956, 1957 and 1958 contain a total of eight brief sentences about the polio research then being conducted on 400 Lindi chimpanzees, which fail to provide any substantive information about the work taking place, which, as we now know, included the removal of kidneys and other organs.⁶⁵

The 1956 entry explains that experiments have started, and that 60 chimps have already been "used". Those for 1957 and 1958 each find reasons for not reporting anything about the research. The 1958 reference states that the OPV work on the chimps is almost complete, and is the subject of a report that is in the process of being edited. This promised paper is referenced in two other published works also. But it was never publicly released, even though one suspects that copies were produced and circulated, and almost certainly still exist somewhere in the USA and/or Belgium. So this is all that is revealed about a programme that involved a historically unprecedented mass-sacrifice of apes.

To me, this only underlines the fact that the work conducted on the chimps at Lindi, at least for the first two years, was highly secret (something which contemporary visitors to Stanleyville and Lindi camp, both African and non-African, have almost universally confirmed). Furthermore, the careful reporting of the *failure* to make any significant quantity of tissue culture, and the claim that *only baboons* had been used for this purpose, suggests to me that even back in 1958 a need may have been felt to provide a cover story.

Dr Osterrieth was a virologist, and had expressly been trained in how to make tissue culture. He admits making tissue cultures from baboons (which are not very common near Stanleyville),⁶⁶ yet he had a camp full of chimpanzees (which apparently "had to be sacrificed at intervals") just fifteen kilometres away. So when Dr Osterrieth denies making cultures from chimps, this begs a question. Why on earth not?

Was it not his job, his duty even, to at least *attempt* to do this?

According to that strangely precise entry in the 1958 lab report, no chimp tissue culture was made. In addition, the report inexplicably fails to mention that polio vaccine was being prepared – even though (under normal circumstances) this would have been a significant feather in the cap of the new lab, and one which would surely have been proudly announced. The deliberate misreporting of one detail, and the non-reporting of the other, are the first strong indications that something strange was going on here. Something so strange that it was felt necessary for the lab scientists to get their plausible denial enshrined in the public record, even if (at that stage) nobody was asking awkward questions.

Let us leave that for a moment, and return to the original question. If not Osterreith, then was there anyone else at Stanleyville who might have made Maitland-type cultures from chimpanzees? There are in fact quite a few potential candidates.

Between February and April 1955 the director of the Stanleyville lab, Dr Ghislain Courtois, went on a study tour to the Rockefeller Foundation labs in New York and Trinidad, and then on to the Osvaldo Cruz Institute, a Rockefeller-funded lab in Rio de Janeiro. The notebooks from that tour make it clear that he received a training course in virology, focussing on tissue culture, virus isolation and cultivation, and vaccine-making. There are notes on four different approaches to making Maitland-type cultures from monkey kidney, and on the growing of poliovirus in those cultures. There is also a page on trypsinisation.⁶⁷ Courtois and Koprowski apparently first met later in 1955, when plans were made to set up the chimpanzee camp, which opened in June 1956. So Courtois certainly had the knowledge and wherewithal to have produced tissue cultures in Stanleyville from an early stage. This, of course, is further confirmed by his assistant's recollection that tissue culture *was* being made in the lab, and that it was produced “mainly from chimpanzees”.

Then there is the histopathologist, Dr Gaston Ninane. During my first interview with him in 1992, Dr Ninane told me three times in the space of a few minutes that CHAT vaccine had been made in chimpanzee kidneys. When I pointed out that the literature of the day had mentioned only *monkey* kidneys, he suddenly retracted, saying this had been a slip of the tongue, and that this is what he had meant to say – that some sort of “monkey” (such as a chimp or a baboon, he added) had been used, but that he was not sure which.⁶⁸ It seemed to me that there had been genuine confusion (and he *was* speaking English for my benefit), and I accepted that he had made a mistake. Nowadays, I am much less sure that this was the real reason for his “slip”.

When I asked Ninane later in that interview whether polio vaccine could have been manufactured in Stanleyville itself, he denied it, saying that vaccine-making would have been far too technical an operation for a lab like theirs, and that anyway, no freezers had been present in the lab until 1960.⁶⁹

This is certainly incorrect, in that there is documentary evidence that chimp sera were being stored in a freezer at the Stanleyville lab by June 1959,⁷⁰ and one certainly would have expected the new medical laboratory that opened in September 1957 to have boasted at least one freezer. (Elsewhere in the Belgian colonies, walk-in refrigerators with a freezing capacity were installed in smaller labs such as Butare and Usumbura during the course of 1957.) Besides, freezers are apparently not essential to

the process of preparing, or amplifying, polio vaccine. Apparently a refrigerator would suffice for keeping the various materials such as organs, sera and growth medium below 4 degrees centigrade.

However, might there be a kernel of truth in Ninane's statement? If there was no freezer in the Stanleyville virology lab until, say, the end of 1958 or early 1959, might this help to explain why Dr Osterrieth had to make new batches of vaccine at regular intervals? (Polio vaccine that was merely refrigerated would have progressively lost titre over time, so that further passage would have been necessary not just to amplify the quantity, but also to boost the concentration of the attenuated poliovirus.)

However, if a virologist like Osterrieth had to keep making fresh batches because he lacked a freezer, is it not likely that for a number of reasons (such as minimising contamination risks, and maintaining titre) he would have made each vaccine batch from the last batch, in series? From the perspective of the polio vaccine, this would be a lot safer than continually opening, closing and pipetting from the original vaccine bottle, but equally it would mean that the twentieth batch of vaccine would potentially contain primate cells from all previous batches (1 to 19), rather than just from two sources: batch 1 and the tissue culture used to make batch 20. Since it is likely that different chimps provided cells for different batches, this would have substantially increased the risk of multiple (yet unrecognised) SIV contamination, and thus of recombination between different SIV strains.

Later, during my third interview with him in 1994, Dr Ninane talked for several minutes about the fact that he had "tried to make tissue culture in Stanleyville" in 1957, using the methods he had learnt at the laboratory of Professor Chevremont in the early fifties. At one point he mentioned that Chevremont had "described the possibility of [using] macrophages" for making cultures, but he did not elaborate. (This is intriguing, because macrophages, which were apparently rather "unfashionable" cells in the fifties, are now known as the natural target cells for SIVs and HIVs. I am informed that a few people were indeed growing viruses in macrophage cultures back in the fifties, but it is hard to know whether or not this has relevance for what was happening in Stanleyville.)

Ninane went on to repeat that in Stanleyville in 1957 he had tried to make tissue culture on his own, over a period of four or five months. Apparently they did not then have trypsin available in the lab, "whereas in the United States at that time, already they used trypsinised extract of organs". When I asked if it was chimpanzee tissue culture he tried to make, he answered: "not only chimpanzee but human". He reemphasised that this had involved using "the old system", and when I proposed that this would have involved making suspended cell cultures (in other words, Maitland-type cultures), he did not object.⁷¹

Another person who could undoubtedly have made Maitland-type cultures in Stanleyville was Dr Fritz Deinhardt. He was, in fact, a tissue culture expert, with eleven of his first thirteen published articles, from 1954 to 1958, featuring "cell culture" or "tissue culture" in the title, even if this previous work had concerned other types of culture (mainly human cells, including HeLa), rather than culture from primate kidneys.⁷² However, that was hardly an obstacle, and Deinhardt clearly had

the expertise to prepare Maitland-type cultures from chimp kidneys (or to help Osterrieth do this).⁷³

Another person who undoubtedly had sufficient experience was Dr Jean Vandepitte, who took over from Courtois as head of the Stanleyville lab during the latter's leave, from March to September 1958. However, according to Dr Osterrieth, he and Vandepitte did not get along, partly because Vandepitte wanted to do everything in a different way from Courtois. Osterrieth told me that he regularly used to disobey Vandepitte's instructions to, for instance, go and take blood from the local population, preferring to stay behind and do "the interesting work" in the lab.⁷⁴ On the one hand, this perhaps tells us something about how devoted the other doctors were to Courtois. However, it also illustrates how easy it might have been for someone to have followed a line of unofficial research on their own initiative, feeling that they were justified because "this is what the boss would have wanted". It is worth noting that it was Dr Vandepitte, together with the *agent sanitaire* Pierre Doupagne, who organised the reunion of the Stanleyville doctors in 1994 – a reunion also attended by Dr Osterrieth, Ghislain Courtois' son André, and one or two other Belgian doctors (though not, apparently, by Gaston Ninane). It is believed that my own investigations into Lindi and Stanleyville were probably the catalyst that prompted this reunion, and certainly information-gathering thereafter became more difficult.

However, what really matters here is not the identities of the doctors who made the kidney cultures in Stanleyville, but *the fact that they were made*. And since this is not a question of apportioning blame, but rather one of assessing opportunity, it is enough to note that there was no shortage of candidates.

Now let us turn to the more important question of timing. Dr Osterrieth says he is unsure *when* those chimp kidney cultures were prepared and sent to Philadelphia. But here I can provide some help. In 1993 Dr Deinhardt's former boss, Professor Gertrude Henle, told me unequivocally that Fritz Deinhardt had himself sent "chimpanzee tissue cultures" to Philadelphia during his time in Africa. In fact, it seems that four shipments of this material were sent by Deinhardt during his three-month stay, and a further two shipments were sent later, presumably by Osterrieth.⁷⁵

Immediately after this, I asked Dr Henle: "Had chimp tissue cultures been sent from there [Stanleyville] before, do you think?". There was a pause, and then she said: "Well of course, they had tissue cultures from the monkey kidney for making polio vaccine". At the time her comment seemed something of a *non sequitur*. When I asked her which monkeys, there was another pause, and then she said she had no idea. However, from the ensuing conversation it became apparent that she had been referring to primate kidneys which had been available in Stanleyville before Deinhardt's arrival in February 1958, and which had been used (somewhere) to make polio vaccine. At the time, I thought she meant that these kidneys were being sent overseas. Now it seems that she was referring to local production of vaccine in Stanleyville. As for the species, there is no evidence of any primate kidneys other than chimp kidneys being available in Stanleyville before 1958.

Dr Henle then asked me why I was so interested, and I realised that she had not been aware that chimpanzee SIV was the ancestor of HIV-1. I explained about the relationship between the two, and about the OPV/AIDS theory. Then, perhaps twenty

minutes or so later, I asked her again whether chimp kidneys had been used to make the polio vaccine. She declined to answer, but it was noteworthy that she did not withdraw what she had said earlier. Finally she said: “I don’t say you are wrong, but it might be futile [to try to follow this up]...Something has happened, yes, but what can you do about it?”

It is only now, in the light of the testimony of the various African witnesses, that the sequence of question and answer makes sense. It seems that Dr Henle had presumed that I already knew that they were using chimps to make polio vaccine in Stanleyville.⁷⁶ It was only when I pressed her on the question of the species that she declined to be more specific. However, she neither backtracked in the way that Gaston Ninane had done a year earlier, nor did she withdraw her statement. She merely pointed out that it might be difficult to prove.

Around this point, I realised that a pattern was beginning to establish itself, whereby the first responses of some of those scientists who were directly or indirectly involved with the Lindi research were rather telling, but were often followed by something more non-committal, or by retraction. By contrast, those without any agenda, like the African assistants, tended to be much more forthright.

This still leaves us with an important question – that of which tissues were used to make CHAT vaccine in Stanleyville. Given the evidence about chimp sacrifice and organ extraction, chimp tissues would seem to be a plausible substrate, but can anything be proved?

I believe it can. At this point, we need to look at all those statements which have been made by persons who were directly involved with the work at Lindi and with the Congo vaccine trials, and which confirm different details about tissue cultures, and whether or not polio vaccine was locally prepared.

First of all there is Gaston Ninane. When he was visited in hospital in February 2000 by doctors Koprowski and Prinzie, at a time when (according to his sister) he was recovering from a fall caused by Parkinson’s (and possibly Alzheimer’s) disease,⁷⁷ he apparently specifically denied just one aspect of all the many pages which are devoted to his testimony in *The River*. This related to whether he had ever tried to make cell cultures, and, more specifically, chimpanzee tissue cultures, in Stanleyville. Dr Ninane apparently signed a document which stated: “The statements which are attributed to me on this subject are false and are lies”.⁷⁸

However, I have the cassette tapes to prove that Dr Ninane said both these things. The signed statement he apparently gave to Dr Koprowski, and which is quoted by Plotkin, is incorrect, and I am hereby offering to play the relevant passages of these tapes to Dr Koprowski, Dr Plotkin, or to both of those gentlemen, to prove that fact. In return, I would like to see the original of Dr Ninane’s signed statement.

In any case, this point has since been confirmed by an impeccable source. In January 2001 (before my second trip to Kisangani), I spoke with Dr Maurice Kivits, the former assistant Inspector-General of Hygiene for the Belgian Congo. Between 1956 and 1960, it was Dr Kivits (rather than his elderly boss, Paul de Brauwere) who was effectively responsible for public health, including vaccinations, in Belgium’s African

colonies, and in 1959 he had visited Stanleyville and Lindi camp as part of a three month African tour of duty in which he travelled to all the medical laboratories, and assessed the impact of the various vaccine field-trials.

Even in his late eighties, Dr Kivits is still a very precise man. When asked what the Lindi chimps had been used for, he replied that it had been for the “preparation of the vaccine.” Shortly afterwards, Dr Kivits added: “the vaccines were tried in the chimps” – an apparent reference to testing the immunogenicity and/or safety of the vaccines. But his initial answer involved polio vaccine *preparation*, not testing.

When one doctor (Ninane) says that chimp kidneys were used to make the vaccine, but then insists that this was a slip of the tongue, then it is not unreasonable to accept his word. (I was not looking for a conspiracy theory, merely for the truth.) When a second doctor, the man with overall responsibility for the vaccinations, says the same thing, but seems disinclined to elaborate, then one ought to sit up and take notice. But even at this late stage, I still believed that the Koprowski vaccines had only been made in the United States and Europe. It was only with the further confirmation by the African assistants in Kisangani, three months later, that I finally realised the full significance of Dr Kivits’ remark.

To sum up: Osterrieth’s first lab assistant has reported that polio vaccines were being prepared in Stanleyville in early 1958, at the latest – and this appears to have been confirmed by Fritz Deinhardt’s boss (Gertrude Henle).

Dr Kivits and Dr Ninane, who were both direct witnesses to events, have both stated that chimpanzees (or chimp cultures) were used to make the polio vaccine – even if the latter subsequently modified that statement, and the former did not supply further details.⁷⁹

However, the crucial detail that chimpanzee tissue cultures were being prepared in at least two departments at the LMS is confirmed by Courtois’ assistant.

It is a shame that nobody has yet felt able to make one clear, unequivocal statement which sums up the entire chimpanzee/polio vaccine programme, but this is perhaps not surprising, given that these activities were shrouded in secrecy in the fifties, and continue to be so to this day. Indeed, it appears to have been policy among the Belgian doctors to keep the Africans in the dark about many of their activities – including, it now seems, the vaccine-making process. (Osterrieth’s first assistant explained: “The whites didn’t show us blacks what they were doing. That’s why, when they went back to Europe, nobody else could follow on that work.”)⁸⁰

None the less, if we put all this evidence together, then the only reasonable conclusion is that none of the European and American doctors involved with the Stanleyville medical lab, Lindi camp and the CHAT trials, has yet told the whole truth about what was happening.

I believe it is now clear beyond any reasonable doubt that tissue culture was being prepared in at least two different departments at the Stanleyville medical lab (virology and microbiology), starting in 1957 or before, and that most, if not all of the tissues were derived from chimpanzees. I also believe it is now clear beyond reasonable

doubt that Koprowski’s polio vaccines were being locally amplified, and that one of the persons doing this was the man who was gathering most of the organs and bloods from the Lindi chimpanzees – Dr Paul Osterrieth.

Scientists apparently like to talk in terms of parsimony – and the most parsimonious explanation, notwithstanding the protestations of those involved, is that CHAT vaccine was being amplified in the Stanleyville medical laboratory in the second half of the fifties, in the cells of chimpanzees. [See **Figure 4**, and the section on “Local amplification”, below.]

Figure 4: Was CHAT made in chimpanzee cells in the *Laboratoire Medical de Stanleyville* (LMS) in the 1950s? Testimonial evidence for (top section) and against (bottom section).

| NAME | TITLE | TESTIMONY |
|--------------------------------|--|---|
| | | EVIDENCE FOR... |
| Paul Osterrieth | Microbiology lab, LMS, 1956-1957; head of virology lab, LMS, 1958-1960 | Concedes that a few chimp kidney cultures were prepared in Stanleyville, and sent to the Wistar Institute. [However, also see other testimony, below] |
| Gaston Ninane | Head of histopathology lab, LMS, 1955-1960. | Originally says that chimp kidneys were used to make CHAT; later modifies this. Originally says that he himself tried unsuccessfully to make tissue culture (including human and chimpanzee tissue culture) in his lab; later retracts. |
| Joseph | “Nurse” and head of the African team at Lindi camp, 1956-1959. | Confirms that the work at Lindi was secret. Says that the sacrificing of chimpanzees was routine, and that most of the autopsies were done by Osterrieth (though others were done by Courtois and Ninane). Says that kidneys were always taken, and placed in liquid in metallic canisters. Thinks that most of them were sent overseas, including to the USA. |
| Louis Bugyaki | Head of Stanleyville veterinary lab, 1956-1959. | During first three interviews, explains that both Osterrieth and Ninane had told him that they had been taking kidneys from the Lindi chimps and sending many of them to America; begins vacillating after recent approaches by Belgian doctors. |
| First assistant to Osterrieth | Helped Osterrieth in LMS virology lab from February 1958 on. | Recalls Osterrieth frequently making polio vaccines, in response to requests from the provincial government, and says this began before his own arrival in February 1958. Associates the vaccine-making with the preparation of chimpanzee serum. Fed this same polio vaccine orally in several places including Stanleyville military camp, a known CHAT vaccination site. |
| Second assistant to Osterrieth | Helped Osterrieth in LMS virology lab from December 1958 on. | Recalls that Osterrieth prepared a vaccine; cannot recall which. Has recently received a letter from Osterrieth, asking him which of the African workers from the LMS and Lindi camp are still alive and which are dead. |
| Courtois’ assistant. | Helped Courtois in the microbiology lab of LMS from April 1956 on. | States that tissue culture was made in Courtois’ microbiology lab, and that this was “mainly from chimpanzees”. Says the process was linked to the autopsies conducted at Lindi camp. Says tissue culture was also made in Ninane’s histopathology lab, but he doesn’t know about the virology lab. |
| Hilary Koprowski | Director, Wistar Institute, Philadelphia from May 1957 on. | States that his polio vaccines were routinely amplified in labs all over the world, and that this may have included the Congo [but also see other testimony, below] |
| Priscilla | Wife of Tom | Says that her late husband brought kidneys and other materials |

| | | |
|------------------|--|---|
| Norton | Norton, Koprowski's chief lab assistant from 1946-1957. | from the Lindi chimps back to the US in March 1957, and that these were delivered to the Wistar Institute |
| Gertrude Henle | Joint head of virology lab, CHOP, 1950s. | Says that the "monkey kidney culture" in Stanleyville was being used to make polio vaccine. Later makes it clear that the "monkeys" in question were in fact chimpanzees. |
| | | |
| | | EVIDENCE AGAINST... |
| Hilary Koprowski | Director, Wistar Institute, Philadelphia from May 1957 on. | Denies everything |
| Gaston Ninane | Head of histopathology lab, LMS, 1955-1960. | Signs a statement for Koprowski and Plotkin shortly before his death denying that he ever said that he tried to make tissue culture, or chimpanzee tissue culture, in his lab. |
| Stanley Plotkin | CDC Epidemiology Intelligence Service based at Wistar, August 1957 on; later associate director at Wistar. | Indignantly denies all charges, calling <i>The River</i> "a house of cards built on a swamp of conspiracy theory, unsubstantiated allegations, and character assassination". He and his team have persuaded some witnesses to modify their stories; however, at least one such witness was sent a prepared letter to sign which did not reflect his own views, but rather those favoured by Dr Plotkin. Much of the information that he has presented in response to <i>The River</i> is inaccurate, or misleading. |
| Paulette Dherte | Nurse and pharmacist, LMS, 1955-1960. | "Laughed uproariously and said it was completely impossible", according to Stanley Plotkin. |
| Paul Osterrieth | Microbiology lab, LMS, 1956-1957; head of virology lab, LMS, 1958-1960. | Denies that he had significant dealings with the chimps at Lindi, or that he conducted autopsies on chimps. Denies that he was able to make tissue culture in the lab before mid-1958, and insists that even then it was only prepared from baboon kidneys. Denies ever making polio vaccine in his lab. However, has changed his story on several occasions, and refuses to answer certain of the key questions. |

e) The when.

The major question that remains to be answered about the African CHAT trials is: when did the making of vaccine in Stanleyville begin?

Osterrieth's first assistant began work on February 12th 1958,⁸¹ and says that his boss had been making polio vaccine before his arrival. According to Dr Osterrieth, he and his wife returned to Stanleyville "a very short time" (his wife says "just a few days") after Deinhardt's arrival in that city.⁸² Since the latter is documented as having occurred on February 1st; it seems that Paul Osterrieth must have returned from leave on or around February 4th. So had he been making polio vaccine for just a week prior

to the hiring of the first assistant? Or was he – or someone else – making polio vaccine even before that?

Here, for perspective, it may be helpful to return to Dr Ninane's statements. As I wrote earlier, Dr Ninane told me that he had tried, on his own, to make tissue cultures from both chimpanzee and human cells for a period of four or five months during 1957, using the old-fashioned methods of Professor Chevremont. Later, he told me that he eventually stopped these attempts, because "it was impossible to make tissue culture with the material we had." He added that Courtois had laughed at him, saying he was an old man who could not do anything right.

Ninane also told me, on several occasions, that he had been trained in tissue culture techniques in Dr Lise Thiry's lab in Brussels for ten to twelve weeks in the summer of 1957, and fortunately we have a paper written by Lise Thiry in 1958, which gives a sense of what these techniques might have involved. The paper reports that she had the Koprowski vaccines CHAT and Fox in her lab by July 1957 (when Ninane is likely to have been present), and that they grew CHAT and Fox (and several other viruses) on "several [trypsinised] batches of monkey kidney cultures" from (it is hinted) more than one species.⁸³ Experiments were also conducted with many other cell cultures, and we shall return to these later.

It seems reasonable to propose that the four or five months when Gaston Ninane tried to make tissue culture alone were from the time of his return from leave in September 1957, up to January 1958. (However, if he was really using techniques taught him in the early fifties, then it may have been that he was referring to the period before he went on leave – for instance from October 1956 to February 1957).

But in any case, there is no doubt that *somebody* at the Stanleyville medical lab was successfully making tissue culture during the late 1957/early 1958 period, because between January 8th and February 1st, 1958, at least 22,000 people were fed CHAT vaccine in response to epidemic outbreaks of polio in different towns in the surrounding province, *Province Oriental*. Dr Plotkin tells us that the vaccine used was pool 8 or 9 of CHAT (rather than pool 10A-11, which arrived in February 1958) – and it is clear that, like the other polio vaccine used in the Congo, this also must have been amplified locally. Since neither Osterrieth nor Deinhardt was present in the Congo during January 1958, this means that the preparatory work, the making of the cell culture, had to have been done by Courtois, Doupagne or Ninane – and, given the various testimonies, I would favour one of the first two. It is recorded that Ninane was the one who fed the vaccine in the epidemic outbreaks.

A document in the Belgian government archives⁸⁴ reveals that the supply of CHAT ran out half-way through the final "anti-epidemic vaccination", in Bambesa, on February 1st – and that contrary to what is reported by Koprowski,⁸⁵ only half the village was vaccinated. [See later for further discussion.] In other words, the four outbreaks used up all the vaccine which had been prepared from pool 8 (and/or 9) just as the new pool, 10A-11, was arriving in Stanleyville – almost certainly with Fritz Deinhardt – on that same day, February 1st.

But it turns out that Koprowski's polio vaccines may have been amplified in Stanleyville even before January 1958. First there is the testimony from Courtois's

assistant that tissue culture made “mainly from chimpanzees” was being prepared in the microbiology lab, and his allusion to both Courtois and Osterrieth working there, which makes it clear that here he is referring to the microbiology lab in the old building. This suggests that the making of tissue culture may have been going on before Osterrieth moved labs – which would also mean before July 1957, when Osterrieth set off on leave. Then there is Courtois’s claim that he was sending kidneys for tissue culture work from Stanleyville to Europe before December 1957 – kidneys which, as outlined above, must surely have been from chimpanzees. And then there is the evidence that sixty of the Lindi chimps had already been used in the polio vaccine research by the end of 1956.

Finally, there is the statement by one of the former Stanleyville vets, Joseph Mortelmans, that Courtois had already been conducting medical research on a number of chimps in his laboratory even before he (Mortelmans) arrived in Stanleyville. Mortelmans worked there only between December 1955 and June 1956, so this suggests that Courtois may already have been using chimps for tissue culture by late 1955. During this same interview, Mortelmans told me he thought that the chimp kidneys could have been used for the final passage of the polio vaccine. He has since told Plotkin that he was only expressing “a hypothetical possibility”.⁸⁶ None the less, for someone who was a friend of Courtois, and who was familiar with the latter’s chimpanzee research at this early stage, it was a telling comment.⁸⁷

As it happens, Dr Courtois was not the first man in the Congo to produce oral polio vaccines locally. Starting in 1953, Mortelmans’ fellow-vet, Alexandre Jezierski, was making polio vaccines (both killed and live) in a wide range of local primate tissues at his small veterinary lab at Gabu, some 500 kilometres east of Stanleyville. By 1954, he had already tried out chimpanzee tissues, and found that they were “very good” for growing poliovirus.⁸⁸ This will be discussed in more detail below.

f) Vaccine shortfall during the Ruzizi campaign.

But let us now return to the crucial period of February to April, 1958, the period for which we have first-hand testimony that CHAT vaccine was being prepared locally. This is also the period when Fritz Deinhardt was visiting Stanleyville, and when the new CHAT vaccine pool, identified as 10A-11, came onto the scene. We have some quite precise details about 10A-11, because there is a single page of paper from the Wistar which refers to this pool, and which can only have been written between January 23rd and January 27th, 1958.⁸⁹ This means that a sample of the new vaccine pool was almost certainly carried out to Stanleyville by Fritz Deinhardt, who flew out from the USA on January 30th, arriving on February 1st, with an insulated box for his hepatitis-infected stool specimens.⁹⁰ It is not hard to imagine that Deinhardt’s trip, which was paid for by the US army,⁹¹ would have been seen as an ideal opportunity to deliver a 100 c.c. bottle, or half-litre flask, of the new CHAT pool to Stanleyville, and it seems that as soon as Osterrieth arrived a few days later, he began amplifying the vaccine, just as his assistant reports he was doing when he himself started work on February 12th. Since Deinhardt was a tissue culture expert, it would have been convenient to have had him available, to offer advice or help if needed.

The new vaccine, CHAT pool 10A-11, was then fed by mouth to over 3,000 soldiers and their families in Stanleyville military camp, on the south bank of the Congo river

on February 27th.⁹² I have been told by a former Belgian colonial resident that *the soldiers based at this camp would, in all likelihood, have come from all over the Belgian Congo, and would therefore have dispersed to their various home regions at the end of their periods of service.*

10A-11 was also fed in the huge Ruzizi Valley trial which was staged between Bugarama (now in Rwanda), Kamanyola, Kabunambo and Uvira (DRC)⁹³, Kihanga and what is now Bujumbura (Burundi). The dates given in Koprowski's brief article on the CHAT vaccinations are from February 24 and April 10, 1958.⁹⁴ However, Dr Ninane recalled returning to the Stanleyville lab after the main Ruzizi campaign and picking up more vaccine which he then fed along the eastern shore of Lake Tanganyika, from Bujumbura down to Nyanza Lac, almost on the frontier with what is now Tanzania. Altogether, in the two campaigns in the Ruzizi Valley and along Lake Tanganyika, some 215,500 persons were vaccinated.

It seems likely that the mass-vaccination of the town of Lisala (in Equatoria province) by Dr Ninane, an event to which he frequently referred (and which clearly took place during the early round of field-trials, when he himself was directly involved with the feedings), may have happened during the same time period.

It is worth noting that despite the quite specific details about the first African CHAT feedings which are provided in the brief, but key, article which appeared in the July 26th 1958 edition of the *British Medical Journal*, there is no mention at all of three of the above-mentioned vaccinations: those at Stanleyville military camp (February 27th), along the Lake Tanganyika shoreline (April 1958), and at Lisala (date unknown). According to Ninane, it was Koprowski (not the lead author, Courtois) who wrote this article, and who merely sent it to Stanleyville for checking. But the failure to mention these three field-trials (at least two of which Koprowski would surely have known about) is intriguing. Is it possible that they were somehow "more experimental" than the others?

Two American doctors were present for the Ruzizi Valley mass-trial of CHAT, one of whom was Koprowski's long-time collaborator George Jervis, who headed the laboratory at Letchworth Village (a huge facility for developmentally disabled children in upstate New York), and who had previously prepared experimental Koprowski vaccines there in monkey kidney tissue culture.⁹⁵ The other was Agnes Flack, the medical director of Clinton State Farms, the women's penitentiary in New Jersey where, since late 1955, Koprowski had been testing his vaccines on infants born to the prisoners. It is her diary which provides additional insights about how the Ruzizi campaign was organised.

According to Dr Plotkin, on March 4th 1958, eight days after the start of the mass-trial, Dr Ninane telegraphed Dr Koprowski, asking him urgently for more vaccine capsules, and more liquid vaccine.⁹⁶ Koprowski apparently refers to this telegram in a letter which he wrote to George Jervis in the Congo that same day (which is quoted by Plotkin), in which he tells Jervis that he has advised Ninane "to request from you more of liquid Type 1 which will be sent to Usumbura end of March".⁹⁷

The suggestion that Ninane should request more vaccine *from Jervis* is strange, not least because Jervis was due to leave the Congo a fortnight later, on March 17th.⁹⁸

Unless, that is, Koprowski knew that Jervis was preparing fresh batches of the vaccine at the medical laboratory in Bukavu, where he appears to have been based for at least part of his four week stay in Africa.⁹⁹ As the lab man on the trip (a last minute replacement for Tom Norton, who had had a heart attack over Christmas 1957), it would be natural for Dr Jervis to have taken responsibility for both control of the vaccine, and for local amplification. It appears, therefore, that Koprowski was expecting Jervis to fill the gap until more vaccine could be sent out at the end of the month.

The Bukavu laboratory had opened seven months before the lab in Stanleyville, at the end of February 1957.¹⁰⁰ The official opening, on February 25th, was followed by a week-long conference on the standardisation of lab techniques, attended by the directors of all the medical labs in the Congo.¹⁰¹ This included discussion about the essential tasks of the labs, which (it was stated) included the production of vaccines. There was a special showing of a film from the Eli Lilly corporation about making polio vaccines. (Although Eli Lilly made IPV, the film would almost certainly have included details about how to make “monkey kidney tissue culture”.) The same laboratory directors met again at the virus symposium that coincided with the opening of the Stanleyville lab in September 1957.

The Bukavu lab was an impressive building of three storeys, and according to Agnes Flack it was “beautifully equipped”, and had “modern instruments”. Recent research has revealed that attached to these labs was a large animal house where several primates, including chimpanzees, were caged.¹⁰²

We do not know where the vaccine for the Ruzizi trial came from. George Jervis’s widow, Ruth, believes he may have carried the vaccine with him, but this has never been confirmed, and Koprowski refused to answer questions on the subject. But even if Jervis did carry the vaccine, there may still have been a problem. This is because Jervis and Flack’s plane was delayed by bad weather in New York for 24 hours, so that the journey to Africa took three days instead of two. If they *were* carrying vaccine with them in an insulated box, the 24-hour delay may well have been crucial, meaning that the vaccine was no longer viable on arrival.¹⁰³

However, this should not have been so serious, because Paul Osterrieth was at that time amplifying the new vaccine pool in Stanleyville. Agnes Flack’s diary reveals that she and Jervis stopped off in that city for three hours on February 20th, en route to the Ruzizi Valley, and that during this time they visited the medical lab. In other words, Osterrieth may have been able to give them enough vaccine to tide them over for the start of the Ruzizi vaccination. We do know from the diary that on the first eight days of the mass trial, from February 24th up to and including March 3rd (the day before Ninane sent his telegram), only 15,000 people were fed. By the time Jervis left, on March 17th, the rate of vaccination had more than doubled, and the total had risen to 72,500, so it seems likely that some new vaccine had kicked in from somewhere, whether from Stanleyville, Bukavu or elsewhere. After March 17th, the supply problem seems to have been solved, because they began vaccinating upwards of 10,000 persons a day, and Flack wrote about “production lines...competing with Ford and General Motors”.

And which cells might have been used to amplify the vaccine locally? Each batch would have needed fresh cells from a primate or primates: the so-called “primary monkey kidney tissue culture”. So Jervis might have sacrificed one of the primates in the Bukavu animal house – or he might have used some of the chimp cells that we know were then available in Stanleyville. If the latter, then he could have carried the fresh chimp tissue culture with him to Bukavu, and then inoculated pool 10A-11 into different bottles of that culture, to produce further batches of vaccine.

The secrecy that surrounds this episode continues to this day, for Dr Koprowski has not responded to requests to release copies of Dr Ninane’s telegram, or his own response to Dr Jervis. All that we know, therefore, is that the Ruzizi Valley vaccine may have been amplified in Stanleyville, in Bukavu, or both. My own hunch is that Osterrieth supplied vaccine for the first few days, and that Jervis amplified some of this vaccine as soon as he arrived in Bukavu, and this latter vaccine began to be used during the first days of March, with more becoming available by the middle of the month.

The hypothesis that one of Dr Jervis’s major roles in the Congo would have been to prepare batches of vaccine locally would seem to be tenable, for all the necessary “ingredients” were present. The fact that Jervis was called in to replace Tom Norton after his heart attack indicates that in addition to an experienced vaccinator (Flack), a lab man was felt to be necessary at this crucial trial. And Dr Plotkin has admitted that it was their habit at the Wistar to produce vaccine from vaccine, rather than from seed virus,¹⁰⁴ which demonstrates that this same “easy” method could have been used in the Congo.

I have checked the details of this process with three eminent virologists who worked with poliovirus in the fifties (either in vaccine houses or labs), and these men have confirmed that to make a new batch of vaccine from scratch (making a Maitland-type cell culture, inoculating that with a small quantity of poliovirus or polio vaccine, and checking the titre of the new vaccine batch) would have taken about two weeks. All of them made it clear that this would have been a simple task, a kitchen sink operation, even back in the fifties. No special materials were needed, and it could have been carried out in virtually any lab.

Thus, Dr Paul Osterrieth’s statement that “vaccine could not have been prepared in Stanleyville” in 1958 simply doesn’t stand up.

This is the simple truth that any virologist worth his salt knows – but that I did not know when doing my interviews for The River. During the fifties it was eminently possible to make batches of polio vaccine locally, using cells from whichever primates happened to be available.

g) Local amplification of polio vaccines in Europe and Africa.

Was amplification of CHAT only done in the Congo? The answer provides further important perspective on the “hidden issue” of local production of live polio vaccines.

After my trip to Kisangani in April 2001, I undertook a further review of the papers pertaining to the world-wide CHAT field-trials which took place from 1957 onwards.

These papers (combined with certain comments by Dr Koprowski) revealed the surprising and important information that during this period it was *common practice* for live polio vaccines to be passaged again in the country which hosted the trials.¹⁰⁵

I looked, for instance, at the papers describing Koprowski's major field-trial, which involved feeding CHAT to more than seven million children in Poland, starting in June 1959. Papers written by Koprowski's Polish collaborators made no mention of local amplification of the vaccine. However, they revealed that CHAT *must have been passaged again in a locally-prepared tissue culture* before it was administered.¹⁰⁶ This practice both amplified the quantity of vaccine, and potentially boosted its titre, which might otherwise have fallen during the long sea voyage to Poland, for even frozen vaccine loses strength quite quickly after it leaves the original lab.¹⁰⁷ Subsequent amplification in the recipient lab meant that less vaccine had to be sent overseas, but that – after dilution of the amplified vaccine to a suitably immunogenic level – more persons could be vaccinated. Local amplification thus made a lot of sense.

It is not known which substrate was used for amplification in Poland, but the kidneys of rhesus or cynomolgus macaques from Asia seem very probable. The cells of these two species were then being used to make polio vaccines throughout Europe, and the Polish authors report that these same two species were used for safety testing the Koprowski vaccines in Poland.

It is known that CHAT vaccine was also amplified in Sweden in 1960-1962, using cynomolgus cells as a substrate, because a 1966 article reported this fact.¹⁰⁸ And the evidence strongly suggests that the same thing happened to the CHAT that was fed to approximately 1.7 million children in Switzerland and Croatia during the same period.¹⁰⁹

This local amplification of the vaccine virus is not mentioned in any of the early articles about the Koprowski field-trials. In fact, the first literature reference I have managed to find to local preparation of a Koprowski vaccine comes from a Yugoslav journal published in 1964.¹¹⁰

By contrast, the several articles which describe the large-scale CHAT trials of the late fifties allude only to “dilution” of the vaccine virus – or else hide behind an imprecise use of language. (The possible reasons for this apparent coyness on the part of Koprowski and his collaborators will be discussed below.)

The paper which reveals the truth about the Polish trials is interesting. In the initial references to the CHAT vaccine that was “supplied by Dr H. Koprowski”, the titre is noted as log 7.0 TCID₅₀¹¹¹ (50% Tissue Culture Infectious Doses).¹¹² Later in the paper, however, the process of sending out the CHAT vaccine from the central lab in Warsaw to the provincial laboratories is described, together with the recommendation that these labs should dilute by a factor of 500:1, to end up with 200,000 (or log 5.3) TCID₅₀ per vaccinee. This reveals that the vaccine which those labs were diluting had a titre of log 8.0, and was therefore ten times stronger than the Koprowski original. This can only mean that the original vaccine as supplied by Koprowski had been amplified in Warsaw. (Indeed, a later paper by the same team reports on the analysis of several different batches of CHAT at titres ranging from log 6.8 to log 8.3;

these batches have clearly been made locally from the log 7.0 original supplied by Koprowski.)¹¹³

When I went back to the transcripts of my second interview with Hilary Koprowski in December 1993, I found that despite his caginess about matters pertaining to Lindi and the African trials, *he quite openly acknowledged that other labs around the world had produced their own versions of CHAT. He said sometimes this was done through local passage of the vaccine itself, and sometimes by cloning (or making a new pool from a seed lot).*¹¹⁴

During the course of the interview, he stated that as far as he knew local amplification had occurred in Poland and Croatia, and that it might also have occurred in other places that had received his vaccine strains, such as Switzerland, South Africa¹¹⁵ and the Congo.

At the time of the interview, I failed to realise the full significance of Dr Koprowski's statements. But in retrospect it seems that he wished to place it on the record that anyone, once they had a sample of a live polio vaccine like CHAT (whether it be the seed virus or the vaccine itself), could have produced further vaccine locally, simply by onward passage through another cell culture.¹¹⁶

So, what of the articles about the vaccinations in the Congo? Do any of them refer to, or hint at, local passage of the virus? They do not. Just like the articles about the Polish and Swiss vaccinations, they refer only to "dilution". In Koprowski's one brief article about the early Congolese vaccinations,¹¹⁷ he merely states that the approximate minimum dose given to vaccinees in all the early trials (from Stanleyville in February 1957 to the Ruzizi trial ending in April 1958) was 5.3 log doses, or 200,000 TCID₅₀ – the same titre as in Poland.

However, there is also an article by Ghislain Courtois that describes the Ruzizi campaign from the Belgian perspective.¹¹⁸ He writes: "The vaccine used was pool 10A-11 of the CHAT strain. Since the titre of the mother-solution was 7.2 cytopathogenic units per cubic centimetre, it was decided in accord with Dr Jervis to dilute the mother-solution in such a fashion that each vaccinee received the equivalent of more than 250,000 cytopathogenic units." [Author translation.] According to this account, the "mother-solution" (which was just under 16 million units per c.c.) was diluted to produce a final vaccine which was about 63 times weaker.

What is interesting is the single page of paper relevant to CHAT pool 10A-11 which the Wistar Institute released to the Wistar's AIDS/poliovirus advisory committee in 1992, and which was later passed on to me by David Ho.¹¹⁹ This reveals that pool 10A-11, "which is to be used in the 1958 Congo trials", had been tested for immunogenicity by vaccinating infants born at Dr Flack's prison (Clinton State Farms) with serial ten-fold dilutions of the vaccine virus. This paper, which must have been written between the 23rd and 27th January, 1958,¹²⁰ revealed that the original titre of CHAT pool 10A-11, as measured at the Wistar, was 6.7 log doses,¹²¹ or 5 million units of virus.¹²²

So the Ruzizi trial vaccine, CHAT pool 10A-11, as measured by Dr Jervis in the eastern Congo, was 7.2 log doses (nearly 16 million doses of virus), and was thus over

three times more concentrated than the 6.7 log dose vaccine that had left the USA. I am told that this difference of 0.5 log doses might fall *just within* the margin of error to be expected when titrations are being performed in different labs.

But there is another factor here. In practice, polio vaccine starts to *lose* titre as soon as it leaves the laboratory where it was made. In fact, the titre of Type 1 polio vaccine apparently falls within days of preparation, even when it has been frozen at minus 20 degrees centigrade. The only contemporary paper I have been able to find concerning the loss of vaccine titre over time relates to a titration study of frozen Type 1 drageé-candies manufactured from Sabin virus in the former Soviet Union, which reports a fall in titre of 0.3 log doses within three days and of 0.6 log doses within fifteen.¹²³ Another paper by Roderick Murray and colleagues from the National Institutes of Health (NIH) which examines polio vaccine Type 1, 2 and 3 strains from Koprowski, Sabin and Cox, finds that all Type 1 strains have lower titres when tested at the NIH in Bethesda, than when tested at the source laboratory. CHAT pool 13 has a titre of 0.3 log doses lower at the NIH, while the Lederle Type 1 has fallen by 0.4 log doses, and Sabin's Type 1 has fallen by 0.6 to 0.9 log doses.¹²⁴

Since the CHAT pool 10A-11 used in Ruzizi is clearly the same CHAT 10A-11 which was tested at Clinton in January, the pool which was intended for the "1958 Congo trials", then there are only two possibilities.

One is that the vaccine was brought out on the plane by Flack and Jervis, and had therefore spent three days in a cool-box surrounded by ice. This, of course, is very different from being frozen at minus 20 degrees, but none the less, let us assume that the fall in titre was merely 0.3 log doses, like the Soviet candy-drops. This would mean that the vaccine which arrived in the eastern Congo would have had a titre of *no more than 6.4 log doses*, more than six times weaker than the 7.2 log doses of the mother-solution alluded to by Courtois. I believe that this difference of 0.8 log doses falls outside the boundaries of testing error.

The second possibility is that Jervis picked up more locally-produced vaccine from Osterrieth when he passed through Stanleyville on February 20th, at a time when Dr Osterrieth's assistant says he was already "making polio vaccine".

Whichever is the correct scenario, it is apparent that *the Ruzizi vaccine must have been locally amplified*. It is also clear that when Courtois writes that he and Jervis decided "to dilute the mother-solution in such a fashion that each vaccinee received the equivalent of more than 250,000 cytopathogenic units", he is falling in line with the other early Koprowski collaborators by mentioning the dilution, but not the prior amplification.

Further supporting evidence comes from what is known about other OPVs that were being used in Africa at this time. For instance, in 1957 an oral polio vaccine was being administered in parts of French Equatorial Africa (such as present-day Congo Brazzaville and in rural Gabon), as Simon Wain-Hobson discovered when he interviewed former workers from the Pasteur Institute satellite in Brazzaville in 1999. One of these ex-workers told Wain-Hobson that (in the latter's words) he "grew polio on local monkey kidney cultures". This man was not sure which species was, or were, involved, but said it probably included the moustached monkey, *Cercopithecus*

cephus cephus, which was and is the most common primate in the area.¹²⁵ It is not clear whether Simon Wain-Hobson specifically asked whether this man had grown polio *vaccines* in local primate kidney cultures, but given the fact that (apart from very minor trials) virtually all OPVs seem to have been amplified locally to boost titre, this “last step” would seem to be extremely likely. In any case, as Wain-Hobson has since put it: “the principle is established. It would be hard for anyone to deny this.”¹²⁶

In any case, there is documentary evidence that OPVs were being amplified locally in the Union of South Africa, where scientists at the Poliomyelitis Research Institute, under James Gear, “commenced cultivating and reaping the 3 strains of Sabin polio [vaccine] virus during 1957”.¹²⁷ Gear’s team was using the kidney cells of the locally abundant vervet monkey, *Cercopithecus aethiops pygerythrus*,¹²⁸ and the vaccines so produced were apparently first fed in Kenya, Uganda and Mauritius in 1959.¹²⁹ The date when South African local OPV production began is significant, because Gear and Koprowski were very close throughout this period.¹³⁰ It was Koprowski’s intention to travel to South Africa to discuss OPV field trials with Gear after his visit to the Belgian Congo in February 1957,¹³¹ though I have been unable to get definite confirmation that he made the trip.

James Gear was very much in the thick of polio vaccine research. He was on the July 1957 WHO Expert Committee on Poliomyelitis which recommended that OPV field trials could go ahead in places like Africa under certain conditions. He had been producing Salk’s IPV in the kidneys of vervet monkeys since 1954, and in an article published in June 1956, he revealed that attempts to *develop* oral polio vaccine strains were being made in his Johannesburg lab, just as in those of Koprowski and Sabin.¹³²

The most dramatic example of local, small-scale African production of OPVs, however, relates to Dr Alexandre Jezierski, the Polish vet who had, since 1953, been producing both inactivated polio vaccine (IPV) and OPV in the cells of local African primates at his small laboratory at Gabu in the eastern Congo. The reason why a Congo-based vet working for INEAC¹³³ (an agronomic research institute) had been cleared by his superiors to concentrate on experimental polio vaccine production in the heart of Africa over a four-year period is not immediately apparent, but it is known that he had close links with, and paid frequent visits to, both the Pasteur Institute in Paris, and James Gear’s labs in South Africa. After his death, one of his relatives told me that Jezierski had been unexpectedly wealthy, and that he used to move gold bars around Europe, from bank to bank, in his ancient Citroën. However, it is not known whether this is in any way related to his earlier activities at Gabu!¹³⁴

Whatever, Jezierski’s published papers reveal that he experimented with the kidneys of fifteen different African primates, including chimpanzees, and found that all of them produced “very good” cultures;¹³⁵ and that he grew his polio vaccine virus in these chimp cell cultures when he conducted comparative titrations on OPVs which he had prepared in cells from three different colobus species: *Colobus abyssinicus*, *C. badius* and *C. angolensis*. These vaccines were later fed experimentally to chimpanzees and to human volunteers.¹³⁶

At some point during the early part of February 1957, Hilary Koprowski and his assistant Tom Norton spent “about three days” with Dr Jezierski – apparently at the